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Part First.

ORIGINAL COMMUNICATIONS.

Article I.—*Case of Strangulated Hernia, in which the symptoms continued after reduction of the Tumour.* By James Syme, F.R.S.E., Surgeon to the Queen in Scotland, and Professor of Clinical Surgery.

On the evening of Saturday, the 23d of November last, I was requested to visit a patient in Roxburghshire, said to be suffering from the symptoms of strangulation, after reduction of an inguinal hernia of the right side, which Dr Purves, of Dryburgh, informed me had been accomplished without much difficulty on the preceding Tuesday. I found a tall and very strong man, apparently between 50 and 60 years of age, with his abdomen much distended, but nothing perceptibly wrong in the groins. He had experienced no relief from return of the bowel, but, on the contrary, during the four following days had continued to suffer from sickness, with frequent vomiting and obstinate constipation. Under these circumstances, we concluded that the sac had been reduced along with its contents, and that the bowel being still in a state of strangulation, there was no time for delay in resorting to efficient means of relief.

I therefore made a free incision over the right spermatic cord, and cut through its coverings so as to expose the external ring, into which I introduced my finger and pushed it up, until the lower surface of a bag could be felt. This was brought into view by dividing the aponeurosis of the external oblique muscle to a sufficient extent, and cautiously opened, as it contained no fluid, and merely a portion of intestine. At the neck of the sac there was a very tight stricture, through which a probe-pointed bistoury having been guided by my finger, free admission was afforded to the protruded bowel. There was no bleeding, and a couple of sutures closed the edges of the wound, which was covered by a compress, properly secured.

No sooner had the operation been performed than the patient declared that he felt relieved. In less than half-an-hour afterwards the bowels were freely evacuated; and everything went on well, without any occurrence worthy of notice.

In connexion with this case I beg to reproduce the following one, of a similar kind, which appeared in this Journal for January 1850:—

"The reduction into the abdomen of a hernial sac, with its contents in a state of strangulation, is an occurrence so rare in the practice of Surgery, that systematic teachers and writers generally seem to regard it as a subject hardly deserving of their notice. Mr Teale, of Leeds, in his excellent work on hernia, has collected and presented in a tabular view all the cases of this kind which have been recorded by Arnaud, Dupuytren, and, more recently, Mr Luke, of London, from which it appears that the only instances of an operation performed with success, under such circumstances, in Great Britain, are one recorded by the last-mentioned gentleman, and another by Mr Wade. I have several times been asked to see patients on the point of sinking under the effects of this "reduction in mass," as it is termed by the French surgeons; and on two occasions have had an opportunity of examining the parts concerned after death. The stricture caused by the neck of the sac was then so obviously the cause of death, and might have been so easily remedied by incision, that the non-performance of an operation within due time seemed matter of deep regret, and suggested as an imperative duty to operate whenever the grounds of suspicion should warrant this proceeding.

"Upon the 13th of October last, I was asked by Mr Sidey and Dr Newbigging to see a patient, who appeared to be suffering from an internal strangulation of the bowel. He was a man about 50 years of age, the butler of a family in town; stout in frame, and rather corpulent. For eleven years he had been troubled by an inguinal hernia of the right side, and worn a truss to prevent its protrusion, until the last six weeks, during which he had not observed any swelling, although the bandage had not been used. At twelve o'clock of the preceding night he had suddenly felt very ill, and sent for Mr Sidey, who saw him at one o'clock in the morning, complaining of intense abdominal pain, with quick pulse, cold perspiration, and vomiting. Upon careful examination, a tumour, about the size of a hen's egg, was felt in the right iliac region, without any external enlargement or thickening of the parietes. Leeches were applied, and several injections administered during the night, with some palliation, but no material alteration, of the symptoms; which continued much the same as they have been described, until we met about two o'clock in the afternoon—fourteen hours from the commencement of the attack.

"As there could be no doubt that strangulation of the bowel existed, we examined the suspected region with all the care in our power, but, probably from the abdomen having become more distended, without being able to detect the tumour, which had been felt the night before. All that we could perceive was a slight degree of induration or resistance, opposite the internal ring, over a space not much larger than the point of a finger. In these circumstances I should not have felt justified in undertaking any operation, unless Mr Sidey's accuracy of observation had been well known to me, and, in absence of any positive information that could be obtained at the time of our examination, rendered his detection of a tumour in the first instance sufficient ground for surgical interference.

"Having placed the patient in a convenient position, I made a free incision of the integuments throughout the whole extent of the inguinal canal, tied the superficial epigastric artery, inserted my finger through the external ring, and guiding upon it a probe-pointed curved bistoury, divided the aponeurosis of the external oblique muscle, up to the internal opening for the cord. There being still no appearance of a tumour, but a more distinct feeling of resistance, I divided the other coverings of the cord, and brought into view a dark-coloured mass, at the internal ring. Pulling this toward me, I readily drew out a hernial sac, about the size of a hen's egg, which, being opened, was found to contain a portion of the small intestine. Searching for the stricture, I encountered a difficulty from the sac yielding to the slightest pressure, and returning with its contents into the abdomen. I therefore seized it with a pair of forceps, and thus obtained the requisite tension, until I succeeded in passing the edge of my nail beyond the stricture, and guiding the bistoury upon it, effected the dilatation necessary for accomplishing reduction of the strangulated part. The patient was speedily relieved from all his distress, and, although a very unfavourable subject for any operation, recovered completely, and is now quite well.

"This case will, I trust, afford encouragement to operate for the remedy of hernial tumours, reduced in a state of strangulation; and may also be regarded as possessing some interest from being, so far as I know, the only instance of recovery from a hernia which had become strangulated within the abdomen."

ARTICLE II.—*Case of Death from Coal Vapours.* By DOUGLAS MACLAGAN, M.D., Professor of Medical Jurisprudence in the University of Edinburgh.

ON Friday, 25th January 1867, Peter Dignan a labourer, accompanied by his wife and a lodger named Samuel Toll, took up their abode in a room which they had taken in Ayr, to which place they had come from the neighbouring village of Annbank. It would appear, from the evidence of several witnesses, that these three persons were, on the evening of Friday, in perfect health and quite sober. One of the most important of these witnesses, Ann M'Guire, was in the room occupied by the accused and the now deceased Mrs Dignan, at about 10 o'clock on the evening of Friday, whilst

Bernard M'Guire, the husband of the preceding witness, was in the house, and left the party at about 7 o'clock on the same evening. At 7 o'clock on the following morning (Saturday), this witness called at the house of Dignan, and knocked at the door, but failed to gain admittance. He again called at 8 o'clock, at 11, and again later on in the day, just as it was getting dark. On the occasion of these visits the witness, although he was not admitted into the house, heard Peter Dignan cough, and looking through the window he could see that the persons in the house were still in bed. Finally, the witness called at Dignan's house at 10 o'clock on Saturday evening. Having threatened to break open the door if this were not opened, the witness heard Dignan jump out of bed and come to the door, which was then opened. On gaining admittance M'Guire found Mrs Dignan lying dead in the bed from which her husband had just risen, and which was covered with vomiting and fæculent matter.

The lodger, Samuel Toll, was lying in his bed apparently asleep. When roused and told to stand up, he was at first unable to do so. His bed, too, was covered with fæculent matter. On being removed to an adjoining house, Samuel Toll soon recovered.

Under these circumstances, Peter Dignan and Samuel Toll were taken into custody on suspicion of being concerned in the death of Catherine M'Guire or Dignan.

On 30th January a variety of articles, including the stomach and other organs of the deceased, were delivered to me for analysis,—the information, conveyed to me in the usual form of a note from Crown Counsel, being to the effect that, though there was a suspicion of poison, it was suspicion merely, and that if any poison was present, it was one capable of causing (1*st*) vomiting, (2*d*) purging, and probably, also (3*d*), drowsiness and somnolence before it caused death.

The result of my investigations, and indeed the substance of the whole case is stated in the following report, which I therefore give *verbatim*:—

"*Edinburgh*, 6*th February* 1867.

"On 30th January Constable James Findlay of the Ayr Burgh Police delivered, during my absence from town, to my assistant Dr Gamgee, a series of articles, upwards of thirty in number, for analysis in reference to the case '*v*. Peter Dignan, or relative to his wife's death.' As the larger part of these productions have, for reasons which will presently appear, not been analyzed, it is unnecessary for me to lengthen this report by formally enumerating them. I shall, therefore, content myself with specifying those which have been subjected to examination, and state the results:—

"No. 6.—A stone jar, corked, sealed, and having attached to it a label bearing—'Case *v*. Peter Dignan, or relative to his wife's death. No. 6.—*Ayr*, 17*th January* 1867.—Stomach and contents, one kidney, spleen, portion of liver and brain. (Signed) John C. Haldan, Robert Dobbie, M.D., William M'Dowall, James Findlay.'

"This jar was found to contain organs corresponding to the label, which were examined, with the following results:—

"1. *The stomach.*—This had been opened. There were no contents beyond a little grayish opaque mucus, and a few white particles, which were seen under the microscope to be minute fragments of animal matter. No crystalline or other suspicious matters were seen. There was no smell of opium or other drug. The mucous membrane was of a uniform pale pink, which was intensified into bright red at the margins of the openings into the stomach, where the blood in the jar had come into contact with the mucous membrane. It did not present any appearance of the local action of an irritant. The stomach was washed out with distilled water, and the whole fluid subjected to Stas's process for the detection of organic bases, but not a trace of anything bitter or acrid was got.

"2. *The liver.*—The piece weighed 3¼ ounces. 450 grains were subjected to Reinsch's process for the detection of metallic poisons, but the experiment showed that no traces of these were present.

"A portion of the blood in the jar which had exuded from the organs was tested for saltpetre, but none was found. The other organs in the jar were not analyzed.

"No. 8.—A wide-mouthed bottle, with sealed label attached, inscribed similarly to No. 6, except that it bore 'No. 8—Urine.' Of this half an ounce was treated by Reinsch's process for metals, and by the ordinary method for saltpetre, but no trace of either was found. Two drachms were tested with the iodohydrargrate of potash, which gives a precipitate with the organic bases, but none were found.

"No. 1.—This was a stone jar, having a sealed label attached, dated and signed like the other, but marked No. 1, and bearing 'two pieces of drawers, one piece petticoat, one piece cotton sheet.' I examined the piece of petticoat, cutting from it a portion 5 inches long and 2½ broad, with remains of undigested food adhering to it, and having the offensive smell of old vomitings. This was subjected to Reinsch's process, but did not yield a trace of metallic poison.

"No 2.—This jar bore a sealed label, dated and signed like the other, but marked from 'Toll's bed, No. 2, portion of sheet.' From this I cut a piece 3 inches long and 2 broad, which was deeply stained with fæculent matter. This was subjected to Reinsch's process, but the result, as in the other instances, was negative. Another similar piece of the sheet was treated by another process by which any metallic substance present would have been detected, but it also was negative in its results.

"No. 7.—This small wide-mouthed bottle had a label attached, dated and signed like the other, but marked 'No. 7—Tartaric and three powders.' These were found to be respectively tartaric acid, bicarbonate of soda, and starch.

"No. 5.—A small glass phial duly sealed, labelled, and marked as above. It had pasted on it a printed label, 'Tincture of Rhubarb,' but was found to contain laudanum.

"No. 30.—A small paper packet, with sealed label attached, bearing 'Case *v.* Peter Dignan, or relative to his wife's death. No. 30—Saltpetre—*Ayr*, 17*th January* 1867. (Signed) James M'Derment, John Brown.'

"This was found to contain a few grains of saltpetre.

"Refraining, till further orders, from multiplying my experiments, I beg to direct attention to the conclusions to which I am led by those narrated above.

"Of the substances examined by me, none are important, except the laudanum and the saltpetre; of them no traces were found in the contents of the stomach, or the urine.

"The information conveyed to me by Crown Counsel's note was to the effect that, if there were poison in the case, it was one capable of producing vomiting, purging, and probably also drowsiness, before causing death. Now, without ignoring the fact that drowsiness is sometimes seen in acute poisoning by arsenic and other metals, it may be stated that the conjunction of symptoms in this case points either to compound poisoning, such as by arsenic or antimony, and opium, administered simultaneously, or to another cause of death, to be noticed below, which is unconnected with the administration of poison in food or medicine. But in the above analysis I found no metallic poison in the liver of the deceased, and no narcotic in what little the stomach contained. Then, to meet the natural remark that the liver may have contained no poison because it had all been expelled from the system, we have the negative results of the analysis of the petticoat covered with the woman's vomitings, and of the urine, one or both of which would have yielded metallic poison even had it all been removed from the liver, which, in acute and speedily fatal cases, is a very rare event.

"Further, from the fæculent matters on the sheet of Toll's bed, it appears that this man, who survives, had been affected with diarrhœa, probably accompanied by profound drowsiness from the stools being passed in bed; and here, also, no trace of metallic poison was found.

"It appears to me conclusively established, therefore, not only that the liver—the most likely organ—of the deceased woman contained no poison, but that there is evidence that no poison was being expelled from the system by vomiting, or in the urine in the case of the woman, or from the bowels in the case of the man.

"From the negative result of these experiments, I am led to the opinion that the death of the woman, and the illness of the man Toll, were due to something unconnected with the administration of poison in food or medicine. I believe that these most probably were caused by poisoning by the vapour of burning fuel—most probably coal. To make this out positively, it would be requisite to establish that there was a fire either in the room where these persons were, or in some adjoining apartment, from which the vapours would readily pass into the room, and that the last was not properly ventilated. It is not necessary for the production of this form of poisoning that the room should be absolutely close; it may occur even though some air gets in, if the room be small, the amount of burning fuel considerable, and the ventilation not sufficient. The symptoms, so far as made known to me, are quite consistent with this theory, inasmuch as vomiting, passing of fæces in bed, and profound drowsiness, are symptoms of this form of poisoning. The emanations from burning coal are chiefly carbonic acid and carbonic oxide, with traces of sulphurous acid and sulphuretted hydrogen. Of these the carbonic *oxide* is held to be the most important. I state this only as a probable theory until I am informed whether or not there may be facts in the case which must set it aside, but in the meantime I have to state that this opinion rests not merely on the negative evidence furnished by my experiments, and inferences from the symptoms, but that I have, by the examination of the blood which had flowed from the organs in jar No. 6, obtained results which support this theory. I subjected the fluid to examination by the apparatus called the spectroscope; and as this is a form of investigation with which the authorities are probably not familiar, I beg to offer a short explanation.

"A prism is so arranged that the spectrum formed by it can be examined through a microscope. If now a layer of blood dissolved in water is so placed that the light passes through it before being decomposed by the prism, the bloody fluid absorbs certain portions of the light, so that dark bands are formed across the spectrum as viewed through the microscope. These so-called 'absorption bands,' in the case of blood, are two dark lines, one where the yellow and green rays join, the other in the green—a green space of course remaining between the two. It is the oxygenated blood colouring matter which forms these absorption bands. If now some 'reducing' substance, such as sulphide of ammonium, protoxide of iron or of tin, is introduced under certain precautions—the blood colouring matter loses oxygen, and the absorption bands disappear. But if the blood be saturated with carbonic-oxide gas, as occurs in poisoning with vapours of carbon, the absorption bands do not disappear. A portion of this bloody fluid was treated in this way—the reducing agents employed in different trials being sulphide of ammonium and a solution containing protoxide of iron —and the absorption bands were found not to disappear, in one trial remaining distinct for 24 hours; whilst in a sample of healthy blood, subjected to the same reducing agents, the bands disappeared in a few minutes. I consider these results, therefore, as strongly confirmatory of the theory which I have advanced, and think it right only to add, that spectroscopic examination, though a comparatively new method of research, has been, by the careful investigations of numerous men of science, found to be as worthy of confidence as other methods of analysis.

"I am therefore of opinion—1. That there is not the least evidence in this case that any poison had been administered; 2. That it is most probable that these were cases of poisoning by carbonic oxide gas arising from the combustion of fuel in an imperfectly aired apartment.

"I shall wait for further instruction from Crown Counsel before I proceed to multiply experiments which I believe to be uncalled for, by analyzing the multifarious articles now in my custody."

In compliance with a letter from me to the Crown Agent stating the opinions embodied in this report, before the formal document was sent in, Crown Counsel ordered a special examination of the premises, in order to ascertain whether or not the facts of the case were in accordance with my theory of the woman's death. Excerpts from the precognitions taken with this object were submitted to me, from which I extract the more important passages.

"Police-constable John Brown deponed that he was left in the house on the night of Mrs Dignan's death. He lighted a fire, but found that the vent was bad, as the room smoked, but was rather better when the door was opened."

"Janet Brown or Borland deponed that she was the first to enter the house after M'Guire got the door opened. She had heard Dignan coughing very much, and when she went in and found Mrs Dignan dead, she told Dignan that 'he was a strange man, that coughed so much, and did not notice that his wife was dead.'"

Of course this circumstance, which to the mind of the witness was an element of suspicion against the man, is easily reconciled with his innocence upon the theory of his being at the time insensible from coal vapours. This woman had previously occupied the house, and stated that "it always smoked a little when a new fire was put on if the door was shut; but no matter what kind of a fire was on, there was never any smoke if the door was open. The windows do not lift up or down."

"Barbara Turner, or Bowie, had also been a former occupant of the house, but as this was only during summer, the door was generally open, and she was little annoyed, except on two occasions, when she had to take off the fire during the night, as she felt the house smoking and rather close. There was a broken pane of glass in one of the windows, which she allowed to remain in that state because it helped to carry off the smoke when the door was shut."

There is thus sufficient evidence that when the door was shut, as on the night of the woman's death, the room did not vent properly. The daughter of the witness, Janet Borland, had been in the house that night before the inmates retired to sleep, and the fire was then burning clearly—in short, not smoking, though giving off the deadly products of the combustion of carbon.

I had no difficulty as to the conclusion to be drawn from these precognitions so far as they went, and accordingly sent in the following supplementary report :—

" *Case* v. *Peter Dignan, or in reference to his Wife's Death.*

" *Edinburgh, 7th February* 1867.

" Since writing my report upon the case bearing yesterday's date, I have received Crown Counsel's notes of 1st and 5th inst., with extracts from the precognitions of Constable John Brown, Janet Brown or Borland, and Barbara Turner or Bowie. These I have perused, and have to state that they not only do not furnish any reason for modifying the opinion expressed in my report of yesterday, but they fully confirm me in the belief that the fatal occurrence was here owing to coal vapour. To the capacity of the room as a sleeping apartment for three persons I have no objection to make, so far as regards the present inquiry, as it allows about 600 cubic feet for each person. This is not good in a sanitary point of view for a continuous residence, but that is not the question before me. Three persons might sleep in the room without so vitiating the air as to produce any recognisable illness, if there was no fire in the room. But it results unmistakably from the precognitions before me that this room, with a fire on, cannot be ventilated properly unless the door is open, or a pane of glass in the window is broken. The windows do not open at all; so if the door is shut, there must be a collection of vapour from the fire in the room. The witnesses all speak of the *smoking* of the fire, but this is not the really important evil. Smoke so annoys inhabitants that they are fain to open the door or window to get rid of it. It is when the fire is burning clearly, as witnessed by Mrs Borland's daughter on the Friday evening, that the real danger in such an apartment exists; and if now the door and window are closed, the odourless, stupifying gases accumulate, and produce the fatal effects. I have not had communicated to me any information as to the state in which Dignan and Toll are alleged to have been found, but without these there is before me quite enough, taken along with the analytical results obtained by myself, to leave no doubt on my mind that this was a case of poisoning with carbonic vapours."

On the authorities finding that there was no evidence of crime, and that there was thus an explanation of the cause of the woman's death, the case came to an end, and the two men were liberated.

As this case appeared to be of some interest, and as I subsequently learned that the opinion independently formed by me had been canvassed, but had not, at all events at first, found favour with the local medical and legal authorities, I obtained permission from the crown officials to see the whole of the precognitions which had not been already submitted to me.

Of the general evidence I need say nothing more than that there were not the least moral grounds of suspicion against the inculpated parties. These further documents, however, included the report of the post-mortem examination, which I subjoin, and also the declarations emitted by the two men, which are so far interesting as indications of their sensations on the night in question.

"*Copy Report on Post-mortem Examination of the Body of Catharine M'Guire or Dignan.*

"*Ayr, 27th January* 1867.

"We hereby certify, on soul and conscience, that we this day examined the body of Catharine M'Guire or Dignan, wife of Peter Dignan, residing in Becket's Close, High Street, Ayr, and found as follows:—

"There were no external marks of violence on the body. There was considerable rigidity of the body. The right arm was closely fixed, and the fingers of the right hand were closed upon the thumb. The face and neck especially, and the body generally, were of a deep reddish, livid colour; and there were numerous small purple spots on the lower part of the chest and the upper part of the abdomen. The pupils were somewhat dilated.

"The mouth and pharynx were in a natural state, with the exception of the gums, which were pale and somewhat livid at the roots of the teeth. The lining membrane of the larynx and trachea were injected.

"*Thorax.*—The surface of the lungs, especially of the right lung, was remarkably mottled with dark purple spots of a rather large size. The right lung, on being cut into, was found much congested, and much froth and blood of a florid colour could be squeezed from the cut surfaces. The right ventricle of the heart was empty. The right auricle contained a small quantity of dark fluid blood. The valves of the heart were healthy.

"*Abdomen.*—The stomach appeared healthy. It contained about a drachm of a thin pulpy fluid, mixed with mucus. The intestinal canal was nearly empty. The liver was somewhat congested. The spleen was natural. The kidneys were healthy, but much congested, the blood being of a florid colour. The urinary bladder was empty. Its mucous membrane was healthy. The uterus had the appearance presented in women who have borne children.

"*Head.*—The vessels of the scalp and of the membranes of the brain were much injected. The cineritious portion of the brain was much injected. On cutting into the substance of the brain, more than the usual amount of bloody points was observed on the cut surfaces. There was not more than the usual amount of serum in the ventricles. The appearances not being sufficient to account for death from natural causes, we have removed the stomach and its contents, a portion of the liver, the spleen, one of the kidneys, and a portion of the brain, for the purpose of analysis.

(Signed) "JOHN C. HALDAN, F.R.C.S., Edinburgh.
"ROBERT DOBBIE, M.D., L.R.C.S.E."

Peter Dignan declared that he was a labourer, aged about fifty years of age; and that on Friday, 25th Jan., he removed, with his wife and a lodger named Toll, from Annbank to Ayr. He deponed to having, before leaving Annbank, purchased a shilling's-worth of whisky, which was divided between Samuel Toll, himself, his wife, and a carter; he stated that this was all the whisky which he took on that day, but that his wife had informed him that she had, before leaving Annbank, drank a gill of whisky with two other women.

After stating that his wife and himself had been out purchasing provisions, but that she had returned before him, the prisoner proceeded to say:—

"I then returned to my own house, where my wife had arrived before me; she had tea and sugar, bread and butter, and fish. She was taking the fish off the fire, where it was boiling, when I went in. Toll was in the house when I came in. I partook of tea, the bread, the butter, and the fish, and the others did the same. Portions of all these articles remained after we had finished our meal."

After stating that after supper he went to the house of M'Guire for some time, the prisoner proceeded to say:—

"I remained in M'Guire's till about 10 o'clock, when my wife came for me, and we returned together. We sat at the fire for some time, and then all went to bed. Toll kept near the fire, on the floor; my wife and I in a bed near the door, my wife being next the stock or outside, the bed being against the wall. My wife fell asleep first; I did not fall asleep till after one o'clock. I heard the town clock strike one. I never awoke till Saturday night. The first thing I recollect was M'Guire calling 'Peter! Peter!' and wishing me to open the door. I shook my wife, and told her to get up, but found she was cold and made no reply. I thought something was wrong, and jumped out of bed and let M'Guire in. The door had been locked, and I drew the bolt. I shouted out to M'Guire that my wife was cold, and told him to bring a light. I also shouted to Toll to rise, but he could not do so for some time. He could not stand on his feet. M'Guire came with a light, and we then saw my wife was dead. She was lying on her right side, with her mouth and nose covered with the blankets. There was the appearance of blood and vomiting about her mouth and on the clothes. There was also blood upon her petticoat. The bed was covered with filth or dirt from my wife; some came away from my wife's person, and dirtied the front of my shirt when I was rising. I had done nothing of this kind in the bed myself. I felt very tired and fatigued, and my body all sore, when I got up, but there was nothing else the matter with me. I never slept so long before, and do not know the cause of it on this occasion. I did not hear any sound either at the door or window before I was awakened by M'Guire. I put no poisonous article into any of the food used by my wife and Toll, and myself," etc.

In the concluding part of his deposition, the prisoner referred to the small bottle of laudanum which had been found in the house, which he said had been purchased at Annbank by his wife; and to the little packet of saltpetre found upon him, and which he stated he used for making matches, etc.

Samuel Toll deponed that he was a labourer, aged 43 years, and had lived as a lodger with Dignan for five months prior to Friday, 25th Jan. On that day he accompanied Dignan and his wife from Annbank to Ayr. Before leaving Annbank the prisoner Dignan purchased a mutchkin of whisky, which was divided between Dignan, Toll, Mrs Dignan, and a carter who was employed to remove furniture from Annbank. They all arrived in Ayr at about five o'clock. The prisoner having stated that, before going to bed, Dignan, the now deceased Mrs Dignan, and himself, partook of fish, tea, and bread and butter, proceeded to say:—

" Mrs Dignan prepared the beds when she came back." (This refers to Mrs D.'s having been out to buy provisions.) "The one on which Mrs Dignan and her husband slept was close to the door. I slept on a shake-down close to the fire. We went to bed about half-past ten. I know the time from having heard the ten o'clock bell ringing. Mrs Dignan appeared hearty and well when she went to bed. I felt perfectly well when I went to bed, and soon fell asleep. The first thing I was conscious of was Dignan calling out 'My God, Sam, will you not rise, and Katey lying cold dead!' Dignan was standing on the floor in his shirt calling me. I attempted to rise, but was unable to do so; my legs were stiff and powerless. Immediately after this Bernard M'Guire came in with a light, and assisted me to dress. I felt in a very confused and stupid state." The prisoner further on declared—"I did not see whether Mrs Dignan was dead. I heard them say she was dead, and I saw her figure in the bed, but cannot say whether she was dead. My bed was in a very filthy state from involuntary evacuations during sleep. I never was subject to such weakness before, and cannot account for it. I never before had the same feelings of stupor and stiffness of my legs, and I never before slept for more than five or six hours at a time. I smoke and chew tobacco, and had not taken more than my usual quantity on the day in question."

In the concluding part of his declaration, Toll deponed that a small bottle of laudanum, found in the house, had been purchased by Mrs Dignan at Annbank some time before; and that when he saw the bottle at Annbank, it contained about as much laudanum as at the time when the declaration was taken.

There is no need for my lengthening this paper by any extended remarks. I have thought it right to publish the case—*first*, because it affords another example of that which is familiar to toxicologists, the effects of the emanations from burning carbon giving rise to a suspicion of criminal poisoning; and, *secondly*, because it is, so far as I know, the first application to death from carbonic oxide of the new method of investigation by the spectroscope.

Although the theory of death by coal vapour did not receive countenance at the time of the investigation, from the two very competent medical men who made the post-mortem examination, I think that little doubt can now remain in the minds of any who peruse the above narrative, that this is the true explanation of the case. It is quite clear to my mind that my friends, Mr Haldan and Dr Dobbie, followed the proper course in reporting that the case could not be cleared up without an analysis; and it was the totally negative result of the chemical investigations, which led me to make those further researches and inquiries which have satisfied me that this was a case of poisoning by coal vapours. I am inclined to suspect that death may have been caused in this way in some of the numerous cases which occur to every one engaged in medico-legal practice, where there is suspicion of some kind of poisoning, but where an exhaustive series of researches fails to discover the least trace of poisoning of any kind, inorganic or organic. Now that we can call in the aid of the spectroscope, I should not fail to apply it in any case where ordinary chemical analysis gives negative results. The blood exuding from the organs will suffice for the purpose, as in this case, where we had the advantage of having cold weather and the materials fresh. It appears, from results obtained in my laboratory, that blood impregnated with carbonic oxide retains this gas for a length of time; but it is obvious that it would be always advantageous to have the blood as carefully preserved as possible, especially in warm weather; and it is therefore desirable that, in any case where such an inquiry may possibly be called for, a small quantity of the blood should be set aside for examination in a completely filled and carefully sealed phial.

On the facts of the case I have only one or two observations to make.

The post-mortem appearances are entirely consistent with the view that death was caused by coal vapour, *i.e.*, essentially by carbonic oxide, which, of the two essential products of the combustion of carbon, appears to be the most deadly. The reddish-livid surface of the body—the purple maculation of the skin, and of the pulmonary pleura—and, especially, the florid colour of the blood—are notable appearances.

The blood seen on the body of the dead woman (Dignan's declaration) about her mouth, and on her clothes, had most probably come from the nose. Epistaxis has been noticed as a phenomenon in many of the continental cases of death from charcoal fumes.

The vomiting and purging—or rather, it should be called, involuntary evacuation of the bowels—which seem to point to irritant poisoning, are also of frequent occurrence in cases of death from carbon vapours. It appears to me that these symptoms have not received sufficient notice in the descriptions given in our text-books of this form of death. Dr Taylor (speaking of carbonic acid[1]) says —"In some instances there appear to have been irritability of the stomach, for the affected person has vomited the contents of the stomach in a semi-digested state." Dr Guy, nearer the truth, I believe, says—"In many cases vomiting." Wharton and Stillé say—"Then nausea and sometimes vomiting occur." None of these authors mention the discharges from the bowels; but these seem to occur in a good many cases, although by no means constantly. Of eleven cases given by Casper in his "Handbuch," the bodies were found soiled with ordure in five, four of these having occurred by one accident; whilst in ten additional cases, given by him in his "Klinische Novellen," the occurrence is not once noticed. At any rate, it is worth bearing in mind, that in poisoning by the vapours of burning coal or charcoal, vomitings and fæcal discharges may be found upon a dead body, and thus readily give rise to the suspicion of irritant poisoning.

EDINBURGH UNIVERSITY, *Dec.* 1867.

ARTICLE III.—*On Chloroform Vomiting.* By CHARLES KIDD, M.D. London; Member of the Royal College of Surgeons, England; Associate Member of the Surgical Society, Ireland, etc.

THE observation of Dr Keith—that in his last 14 cases of ovariotomy he had one fatal case in which "much local difficulty from pelvic adhesion was known to exist," but he had "little doubt that the excessive vomiting from chloroform gave rise to accidents which were the cause, in a great measure, of the fatal result"—brings to my mind many cases of severe vomiting that I have known in London, which were ascribed to the chloroform, but where, in point of fact, like shivering, rigor, anoræxia, etc., the vomiting was more probably rather a sign of surgical fever. Thus we read of the pulse as "ranging between 104 and 118, and severe chloroform vomiting;" then of the pulse about 120, skin hot, above 100°, and chloroform vomiting; and so on in Dr Keith's cases.

I believe the common or ordinary vomiting by chloroform is a reflex action caused by the condensation of some of the vapour, when it is given largely, in its passage through the fauces and pharynx,—this fluid chloroform trickling into the half-open passive œsophagus and cardia, and there exciting reflex spasms in the inferior and superior laryngeal nerves.

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[1] It would be conducive to a more clear comprehension of the subject were poisoning by carbonic acid alone, as in brewers' vats, and of carbonic acid and carbonic oxide mixed, as in carbon vapours, were kept more distinct than they are in most books.

"Nothing like leather" seems the legend of anæsthetics now in London. Simpson, or Snow, or Morton, may have advised the experienced, skilful, quiet administration of chloroform alone, but our louder students are unhappily now neglecting the essential study of chloroform for endless carbo-hydrogen novelties. Hydride of caproyl is the last gone to the tomb of the Capulets; the tetra-chloride of carbon of Dr Protheroe Smith, curiously vaunted in Dublin as the best, being now pronounced by my friend Dr Richardson the worst of the anæsthetics; amylene, lately revived again in the shops, being in very bad odour indeed; while ether and ether mixtures of the shops have universally disappointed practical hospital men.

What I would say perhaps as to vomiting, in so many words, is, that I have an increasing suspicion, from a large range of cases, that there are at least *two* forms of chloroform vomiting so called: one mixed up very much with surgical fever, general constitutional disturbance, etc.; the pulse usually high—about 120; temperature high—above 100°;—in fact, a form of vomiting, with shivering and sickness, familiar to surgeons long before the discovery of chloroform; and also a second form of vomiting, that usually passes off, but occurs in the excitement stage of the chloroform administration, but having its rise, in a large measure, in the mode of administration, or rather in the local influence of chloroform itself on the pharynx, etc., which excites reflex action at the cardia. Some patients vomit if the surgeon only examine or touch the back of the tongue. These subjects will be found to bear chloroform badly. I have known patients subject to vomiting afraid of chloroform (the cause probably in the stomach itself), and yet by care not vomit at all when it was administered.

It is quite easy to imitate the vomiting of chloroform, in experiments on animals, by irritating the œsophagus and cardiac end of the stomach (*not the pyloric end*); and I think it will be found, where chloroform is used in very large quantities, as in Scotland, perhaps, and now in London, that vomiting is more common than where it is only half given, as in Paris. It is difficult to express this contradiction in words, but the experienced chloroformist at once sees, at surgical operations, that it is not in the state of deep, full anæsthesia that vomiting is urgent or present at all, but where bad chloroform is half given, or rather where the inhaler is largely charged with the vapour—where the stomach is probably full of food, in nervous timid patients. In other words, the vomiting of chloroform is not, as in older books supposed to be, of the nature of vomiting in apoplexy, or injury of brain, coma, etc.: it does not occur in the coma stage at all; it is often preceded by alarming syncope, however.

It requires no little deductive force to keep in mind the probabilities of vomiting in a given case, the bearing of surgical fever, pyæmia, erysipelas, coma, etc., on the actual facts observable in the operating theatre.

We want, perhaps, a better method in the logic or study of anæsthetics generally—deductive rather than inductive. If we can shorten or abolish the excitement stage of the chloroform administration, we shall also probably remove vomiting. Our London University has had a science debate during the past month as to the admission of ladies to their degree, one set of medical disputants alleging fiercely that Medicine is not a science at all; another, that it is not an art, but a science; the calm fact being probably that it is both. And so of chloroform administration; it is an art, in the mechanical adaptation of inhalers, mixtures, modes of resuscitation, etc., as much as the art of removing a cataract or glaucoma. It is a science or study requiring a calm logic applied to the thousands of facts, as in glaucoma again, for in almost every patient the anæsthetic acts differently. The St Bartholomew's school has been telling us, wisely too, in half a dozen "Introductories," more or less, the key-note struck perhaps by Mr Paget (out of Butler's "Analogy"?), of the doctrine of probabilities, or the vast value in Medicine of the scientific import of probabilities (insurance offices, to quote one instance, insure lives on probabilities). So in chloroform inhalation, we rest entirely on probabilities: ten thousand to one that all will be right if the administration be carried on with care or average skill. No, say others, you must have Bacon induction, some infallible advertised balloon inhaler, or quack mixture, to prevent the danger arising from diseased heart, overdose, etc.; some induction from a couple of experiments on pigeons.

Again, our students who administer chloroform do it too often in a rough mechanical way that leads to uncertain results, vomiting among the rest. Patients, too, "to keep their courage up" at operation, steal a march on the nurse and surgeon, and gorge themselves, as I have known dozens of times, with a large dinner and a bottle of porter; yet it is notified at the time, they had had no dinner by special order of the surgeon. Some error exists, as I think, as to the explanation of the post-mortem appearances observable in patients who die suddenly of chloroform. This kind of showy induction is observable everywhere in anæsthetics. Thus, in the "Biennial Retrospect" of the Sydenham Society this year, Dr Fagge gives us something new for the medical jurist—the fact that in some 46 cases of rapidly fatal deaths, rigor-mortis was noticed in 11; the blood was frothy, and contained bubbles of gas! Casper and some Germans are quoted to show that this is very diagnostic of chloroform in the blood. (The Germans, be it said, have displayed very little knowledge of anæsthetics as an art, while the French school, and that of America, have analyzed the thing thoroughly, both as a science and an art.) These bubbles in the blood are a coincidence, and not chloroform bubbles at all. They are only a sign of the manner in which the post-mortem is made, as Dr Wilkes or any of the experts in the dead-house at Guy's would have told this purely theoretic writer for the Sydenham Society.

In Germany, the law-books look on these bubbles as infallible; proving a death to be suicide or murder; and further, to be suicide if the sponge or handkerchief be found spasmodically clenched in the dead man's hand. This latter has been often observed, bearing incidental evidence of the truth of my theory of the cause of death, viz., in small spasmodic irritating doses; and that the death is from sudden spasm of the glottis, with apnœa, rather than the often quoted paralysis of the heart, over-doses, cardiac syncope, etc.

In conclusion, where the surgeon has to fear vomiting from chloroform, I think experience teaches he should observe a few practical points. He should use as little chloroform as possible, and repeat it often, rather than administer large quantities at once, when some of the superabundant vapour is almost certain to be condensed and find its way into the œsophagus during the respiration. The operating room should be warm, etc.; the patient should have had no food for three hours previously, etc.

It is pretty clear that vomiting may be avoided by any other form of conveyance of chloroform or anæsthetics into the system that does not engage, as inspiration does, the irritability of the pharyngeal and laryngeal nerves (so intimately associated with the *par vagum*). Chloroform vapour, to wit, passed directly through an opening in the trachea, is said to be free of vomiting; whilst inhaled higher up, through mouth, nose, pharynx, etc., in the same animal, it will excite active vomiting. This, of course, is only suggestive, viz., that we might with advantage, in a special case where we have to fear vomiting, try what could be done by subcutaneous or other injection of morphia or other narcotic, so as to lessen the necessity for a large amount of chloroform, or we should deaden the sensibility of the pharynx, or mix chloroform with hot water vapour.

All the later anæsthetics—tetra-chloride of carbon, common ether, bichloride of methylene, amylene, etc.—are equally unfortunate in producing vomiting. Indeed, one can see little if any advantage in any of these over chloroform, the doses and habitudes of which are so much better known, its orange odour so much more agreeable than that of fluids with theoretic showy names, low boiling points, dense vapours, etc., that sell at ten times the cost of chloroform, etc., but which the medical text-books describe, when pure, as smelling remotely of "decayed cabbage," and which, practically, one has the utmost difficulty in getting patients to inhale at all, more especially as not one of them is superior to plain chloroform.

I have often thought, in an important case, that a gargle of aconite would so benumb the pharynx, glottis, etc., that vomiting would not occur; or, as in hooping-cough, a little alum and honey would act, specifically applied to these parts, in a similar manner; kreosote I find a better remedy than cadmium; for the vomiting of pregnant females it would probably be useful too.

If we are to understand various points in anæsthetics, however, such as the remedies for vomiting, the causes of death, the best mode of resuscitation, the signs of suicide, we must go on in our medical study of chloroform rather than give too much heed to a one-sided science, entirely unpractical, of things with only big words. It is to be regretted that, in London at least, like mules for Abyssinia, it is all now a matter of "shop" and advertising!

It takes a long time for men to get rid, too, of popular prejudices. We know, for instance in midwifery cases, that passing the hand into the uterus, in some patients, *without* chloroform, excites puerperal convulsions and vomiting; under chloroform these will probably be absent; but if present, chloroform will get the bad credit of it. With the pure laboratory men and chemists, the reasoning here is one-sided—the "post hoc," by men who have not large views of the subject, taken for "propter hoc." A very extraordinary trial took place, in which the great question at issue—whether a poor family of a surgeon should be ruined or not, that is, whether he committed suicide, and they could not claim some £2000 insurance—proved to turn upon these bubbles in the blood; and yet all depended on the manner of tearing off the calvaria at the post mortem.

All this has a direct bearing on the subject before us, viz., that of vomiting, and its probable causes and remedy. We require wide deductive examination of the entire thing. I have observed that where the bowels have been well cleared out before an operation, and no food is taken for three hours, vomiting under chloroform is not common. It does not appear that vomiting is the cause of so much harm in eye operations as formerly believed. The mischief in ovariotomy, or after hernia operations, is greater; this is the clinical experience of hospitals.

A little ice to suck at the time of the threatening of the vomiting seems to act very well in stopping it. Probably the ice constringes the opening of the œsophagus, and dilutes any loose chloroform that may be in that passage. A little iced claret is an excellent form of administering the preventative, taken immediately before the chloroform.

Ovariotomy, somehow, as Dr Keith will have remarked, is not a very painful operation, especially when the integuments, as they usually do, present a very thin appearance; the incision exactly in the median line is not at all followed by the cries, on the part of the woman, one would expect. It might therefore be possible, where there were no adhesions, to operate in such with ice or a local anæsthetic; but of course with adhesions, it would be unwise, if not cruel, the pain being then greatest; under "ether spray" the lips of the wound, too, may mortify.

It is well to get the patient as quickly as possible from under the effect of the chloroform, if you wish to avoid "vomiting." This is accomplished by giving as little as possible of the anæsthetic, and what I find acts admirably, fanning the patient briskly with a common lady's fan, which seems to wake the patient up and direct the reflex irritability by the respiratory apparatus generally away from the struggling stomach and laryngeal nerves. This form of vomiting is no doubt very different from that attending surgical fever and grave constitutional disturbance.

ARTICLE IV.—*Thermometrical Observations.* By T. J. MACLAGAN, M.D.

THAT the exact value of the thermometer as a guide to the diagnosis, prognosis, and treatment of disease is as yet undefined, is abundantly proved by the diversity of opinion regarding its utility which is found amongst medical men. By some it is lauded as an infallible guide to the attainment of a proper knowledge of many diseases; whilst others regard it as an unnecessary and fallacious attempt at refinement—"a mere scientific toy." Extreme views are apt to be erroneous, and probably are so in the present case. An instrument which has been found valuable in the hands of men noted for professional and scientific attainment cannot be a mere toy: at the same time, it does not become us, as honest searchers after truth, to accept the conclusions of even such men as Wunderlich, Ringer, Aitken, etc., without submitting them to the proof, and testing, by further observation, the value and accuracy of the conclusions at which they have arrived. Imbued with this impression, I began to observe for myself, and was, ere long, led to regard the thermometer as a very valuable aid in acquiring a knowledge of various diseases, but as not possessing those high virtues which some have ascribed to it.

My object in the present paper is to try to summarize the results of my own observations, and to give the practical conclusions which I have been led to draw, inserting occasionally, for the sake of illustration, the particulars of an individual case, but avoiding, as much as possible, unnecessary detail.

The instruments used were Casella's straight self-registering, as recommended by Aitken. The observations were made in the axilla, and the directions usually given were strictly attended to.[1]

In many cases the observations were made four times a day—at 10 A.M., 2 P.M., 7 P.M., and midnight. I found, latterly, that for all practical purposes, it was sufficient to note the temperature twice a day—morning and evening—with an occasional observation at midnight.

The pulse and respirations were noted at the same time. After many careful observations, I came to the conclusion that the mere rapidity of the respirations was not to be regarded in all cases as an exact index of the patient's condition; that the number of respirations taken per minute were apt to vary, and be influenced by very slight causes, such as the swallowing of saliva, or the existence of a slight catarrh; and that in cases in which there was much affection of the nervous system (typhus, for instance), the character of the respirations was more worthy of note than the mere frequency. I accordingly, in many cases, did not note the number of the respirations with anything like regularity, but confined myself to an occasional record of their frequency, and a general description of their character—irregular, cerebral, etc.

For clearness' sake I shall take separately the diseases on which a sufficient number of observations were made to enable me to form some definite opinion regarding the utility or otherwise of the instrument, and shall further subdivide what I have to say into remarks on diagnosis, prognosis, and treatment.

Typhus Fever.—The range of temperature which I have found in this disease does not altogether accord with that which has been noted by other observers. As my object in this paper, however, is rather to give the results of my own observations than to criticise those of others, I shall simply note the fact, and remark that the difference may in part be due to the varying circumstances of different epidemics: those symptoms which usually accompany a high range of temperature may be more marked and frequent in one epidemic than in another; and as the state of the temperature is the effect, not the cause of the condition which produces these symptoms, it will naturally vary according as they are more or less marked. Such appears to me the most probable explanation of the fact, that I have never found in any of my cases such a high range as that given by Wunderlich and Aitken, and given too as the typical range. At page 387 of the last edition of his work on the Science and Practice of Medicine, Aitken gives, from Wunderlich and Traube, a table to show the typical ranges of the temperature in Typhus and Enteric fevers, in which the typhus range is shown to be above 106° on the 5th, 6th, 7th, 8th, 9th, 10th, and 11th evenings; and again, at page 434, he gives another typical case, in which the morning temperature is for nearly a week above 105°, whilst the evening temperature is, during the same time, above 106°; and on the 4th evening is as high as 107°. The highest temperature that I ever found was in a girl aged 17 years, with very bad head-symptoms, who for 36 hours before death had complete suppression of urine, who died comatose on the 14th day, and in whom, a few hours prior to the fatal termination, the thermometer indicated a temperature of 106·4°. In cases which terminated favourably, I never found the temperature higher than 105·2°, at which I have several times noted it.

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[1] One of Aitken's instructions, that the thermometer should be slightly warmed by holding the bulb in the hand, was at first neglected; I afterwards found that neglect of this precaution was apt to lead to an understatement of the temperature. I accordingly abandoned all my earlier observations, and now only use those which were made after the plan of holding the bulb in the hand till the mercury showed a temperature of 95° was adopted. They were made during the years 1865 and 1866 (chiefly the latter) in the Dundee Infirmary, while I was superintendent of that institution.

Aitken states that both in mild and severe cases the temperature always rises above 104·7°; it only did so in 13·7 per cent. of my cases, and these were all severe.[1]

The following table shows the average temperature at morning and evening on each day up to the time of commencement of defervescence in 58 cases which recovered, and in the selection of which nothing was considered except that there should exist no complication; that no disturbing influences (such as those to be hereafter noted) should prematurely and abnormally lower the temperature; that each should have a definite commencement, and that no doubts should exist as to the time of onset of the disease.

		Morning.	Evening.
In 1 case the temperature was on the	1st day		100·8°
,, 3 cases the mean temperature on the	2d day	102·4°	102·5
,, 10 ,, ,,	3d	102·7	103·2
,, 20 ,, ,,	4th	103·1	103·5
,, 30 ,, ,,	5th	103·3	104·
,, 41 ,, ,,	6th	103·1	103·3
,, 47 ,, ,,	7th	103·	103·5
,, 54 ,, ,,	8th	102·8	103·4
,, 58 ,, ,,	9th	102·8	103·1
,, 55 ,, ,,	10th	102·8	103·1
,, 37 ,, ,,	11th	102·4	103·4
,, 28 ,, ,,	12th	102·6	103·1
,, 17 ,, ,,	13th	102·7	103·
,, 13 ,, ,,	14th	102·5	102·9
,, 7 ,, ,,	15th	102·6	102·9

From this it appears that the morning temperature was habitually lower than the evening: the average of the former being 102·7°, that of the latter, excluding the first day's observation, 103·2°. In 26 of these cases in which the observations were also made at 2 P.M., the mean temperature at that hour was 103·1°, or very little below the evening average. In 31 the mean temperature at midnight was 102·8°, a little over the morning average.

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[1] The range which I have found accords with that given by more recent observers. See papers in St George's Hospital Reports, vol. i., by Dr Thompson; St Bartholomew's Hospital Reports, vol. ii., by Dr Warter; Dublin Quarterly Journal, vol. xlii., by Dr Compton; the same journal, vol. xliii., by Dr Grimshaw. It seems, indeed, that Dr Aitken's cases cannot be regarded as representing the typhus usually met with in this country; it is possible, as suggested by Dr Grimshaw, that the instruments used were not sufficiently accurate. As Aitken's observations are most generally known and read, it is well that their probable inaccuracy should also be known.

The average maximum range was 104·3°—the highest being 105·2°, and the lowest 103°.

It would seem that the temperature gradually and steadily rose, showing little or no tendency to a morning fall, till it reached its highest point on the 5th evening, that it then fell somewhat, and did not again reach the same height; and that on the 6th day commenced those morning falls which characterized the remaining course of the case, and which, though occasionally slight, were generally quite appreciable. The range above the average on the 6th, 7th, and 8th evenings I am inclined to attribute to the fact, which I have several times noted, that in those cases in which the appearance of the eruption is delayed for a day or two beyond the usual time, the temperature does not reach its maximum so early as in those in which it is out on the 5th day. It seems, in fact, that the febrile disturbance, as indicated by the temperature, reaches its height about the time at which the rash is fully developed.

The following table shows the rate of the pulse in the same cases from which the last table of the temperatures was compiled:—

				Morning.	Evening.
In	1 case the pulse on the 1st day				120
„	3 cases the mean pulse on the 2d day,			125·3	120
„	10	„	„ 3d	113·	114·2
„	20	„	„ 4th	108·1	115·6
„	30	„	„ 5th	110·4	116·2
„	41	„	„ 6th	115	116·3
„	47	„	„ 7th	115	118
„	54	„	„ 8th	117·3	118·5
„	58	„	„ 9th	116·3	115·2
„	55	„	„ 10th	114·6	114·9
„	37	„	„ 11th	119·3	118·8
„	28	„	„ 12th	118	118·1
„	17	„	„ 13th	122	124·7
„	13	„	„ 14th	123·3	121·3
„	7	„	„ 15th	117·5	119·5

Those cases in which the observations were made on the 1st and 2d days were children; hence the high rate of the pulse at that early stage of the illness. Calculating only from the 3d day, the average morning pulse is found to be close on 116, and the average evening rate nearly 118°. On the 5th day there is no rise corresponding to the increased range of temperature. Before that day, contrary to what was observed in the case of the temperature, there is a greater difference between the morning and evening rate than is to be found later on in the case. The pulse remains pretty equable, varying only a few beats, on either side of 116 up to the 13th day, on which, and on the 14th, it is over 120; thus showing a tendency to rise at a time when the temperature showed a slight fall. From these tables it would seem that, though the pulse and temperature keep pretty well together, there is no fixed and definite relationship betwixt them. This fact will be further illustrated in an after part of the paper.

Diagnosis.—For the purposes of direct diagnosis in typhus fever, the thermometer is only of use in so far as it shows that the main characteristic of a febrile state, abnormal heat, exists; it conveys no information which enables us to say "this is a case of typhus"—the information requisite for this must be got from other sources.

Prognosis.—When once a case is diagnosed as typhus, it must, to a conscientious practitioner, be one of more or less anxiety, and anything which enables us to form a more definite idea of the actual state of the patient, and adds certainty and precision to our knowledge, even in only a few cases, is to be regarded as a most valuable adjunct to the other means at our disposal. Such I believe the thermometer to be: alone, it is in many cases one of the least valuable of the means by which we judge of a patient's state; but, taken in conjunction with the general aspect of the patient, the state of the pulse, heart, nervous system, etc., it often gives to our prognosis a satisfactory precision which would otherwise be wanting.

To form a diagnosis, no single sign or symptom can be depended on; the eruption, the most valuable of all, is occasionally absent. To form a satisfactory prognosis it is still more necessary that everything should be considered and weighed, and each symptom assigned its due place and importance; but, as the varying circumstances of each case give to an individual symptom, or train of symptoms, a value and importance in one case which it does not possess in the next, and as nothing but personal experience and observation can give the tact and information requisite for the elimination and proper discrimination of those symptoms and signs which are to be regarded as the most important and noteworthy, it is difficult for me to convey to those who have not used the instrument a definite idea of the exact value which I would attach to the thermometer in a given case. Its value as a sign is very variable; in one case it may be the best guide to a knowledge of the patient's condition; in a second it may be of little or no service; whilst in a third, if trusted to alone, it may positively mislead us. The same thing may be said of most of the other means by which we form our estimate of a case. They vary in value in different patients, but not one ought to be completely ignored. I have been led to regard the thermometer as one of the ordinary means of attaining a knowledge of the patient's condition, and one which I would now no more think of discarding, than I would think of ceasing to note the state of the pupil, or to listen to the cardiac sounds; but, at the same time, one which I do not use regularly in all cases, because experience has taught me to discern those in which it is likely to be of use from those in which it is not likely to convey information sufficiently important to counterbalance the expenditure of time inseparable from its employment: just as any one who has seen much of typhus can, in many instances, form a perfectly accurate idea of a patient's state without daily listening to the cardiac sounds, or looking narrowly at the state of the pupil. Though I may not be able to point out the steps by which I have been led to the above position, I may indicate some circumstances which are apt to have such an influence on a case, and so affect its nature and course, as to render it probable that the thermometer may, or may not, be of service.

The formation of a satisfactory prognosis presupposes a knowledge of the probable course of the case, and of the mode in which death is threatened. In uncomplicated typhus, the mode of fatal termination may be by coma or asthenia, or a mixture of both; death may commence at the brain, or at the heart. The former is more common in young people, the latter in those of mature years. When complicated with some pulmonary affection (as typhus so frequently is), asphyxia is apt to share in the production of the fatal result.

When symptoms referable to the nervous system exist only to a slight extent, when there is a good cardiac action, and when no complication exists, we have no hesitation in forming a favourable prognosis. When, however, the case is of such severity as to cause considerable anxiety as to the result, the first thing that the medical man has to do is to satisfy himself as to the mode in which death is threatened, to find out what is likely to be the chief source of danger; having satisfied himself on this point, he has next, by the help of all the means in his power, to calculate and estimate the amount of that danger; and here it is that the power of discrimination, and capacity for weighing and calculating the value of each particular symptom, to which I have referred, becomes of such service.

For the sake of illustrating what has already been said, and of conveying a better idea of the exact place and value of the thermometer, I shall give examples of three different kinds of cases: (*a*) uncomplicated typhus, in which the nervous symptoms ran high, and death was threatened by coma; (*b*) uncomplicated typhus, in which death was threatened by asthenia; and (*c*) typhus complicated with some pulmonary affection, in which death was threatened by asphyxia.[1]

1. A male, æt. 22, came under observation on the sixth day of illness, with the eruption out and other symptoms well marked; in

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[1] In giving these cases, I have noted the temperature along with the other symptoms in the usual manner, and have, for brevity's sake, omitted all unnecessary particulars regarding treatment. Any one interested in the matter can, in a few minutes, form a diagram which will, at a glance, show the range of temperature throughout the case.

morning the pulse was 108 and the temp. 103°; in evening they were 116 and 103·5°.

7th day.—Tongue much furred; patient is restless and has foreboding of death; pulse 108, temp. 101·8°: *vesp.* pulse 116, temp. 103·6°: *midnight,* is wakeful and restless; pulse 128, temp. 104·5°.

8th day.—Is restless and somewhat unmanageable; tongue dry in centre; pulse 132, temp. 104·5°: *vesp.* pulse 132, temp. 104·6°: *midnight,* eyes suffused; is restless, and with difficulty kept quiet in bed; pulse 148, temp. 102·5°.

9th day.—General state as last noted; tongue dry in centre; pulse 132, temp. 103·9°: *vesp.* pulse 136, temp. 103·8°: *midnight,* pulse 140, temp. 102·5°.

10th day.—Passed a restless night; bowels well moved after enema; cardiac sounds good; had an attack of convulsions about mid-day; pulse 144, temp. 103·6°; resp. 44, cerebral in character; urine contains a trace of albumen: *vesp.* pulse 120, temp. 103·6°: *midnight,* pulse 120, temp. 103·2°.

11th day.—Passed a quieter night; he had one or two slight convulsive attacks; abdomen is markedly tympanitic; urine contains rather more albumen; pulse 132, temp. 102·5°: *vesp.* 132 and 102·4°: *midnight,* 120 and 103·6°.

12th day.—Patient is much quieter; is heavy and tending to somnolency; urine contains less albumen; conjunctivæ injected, pupils rather small; cardiac sounds pretty good, impulse quite perceptible; pulse 112, temp. 102·9°: *vesp.* 108 and 103·2°: *midnight,* 116 and 103·6°.

13th day.—Sleeps a good deal; tympanitis very troublesome; bowels open; tongue dry in centre; pulse 108, temp. 102·1°: *vesp.* 100 and 101·1°: *midnight,* 96 and 100·5°.

14th day.—Tongue dry in centre; patient still heavy and somnolent; bladder distended; pulse 96, temp. 101°: *vesp.* pulse 92, temp. 100·2°.

15th day.—Tongue cleaning; general aspect improved; still requires the catheter; pulse 84, temp. 100°: *vesp.* pulse 104, temp. 100°.

16th day.—Continues to improve; urine drawn off by catheter; pulse 96, temp. 98·2°: *vesp.* pulse 88, temp. 98·4°.

Let us take the individual symptoms of this case, examine each separately, and try to give to each its proper value and significance, and so arrive at a just appreciation of the merits of each. The general aspect and manner of the patient, the injected conjunctiva, and almost wild expression of the eye were sufficient of themselves to lead one to regard the case as one in which the head-symptoms were likely to prove troublesome. The character of the respirations tended only to confirm this view. They were hurried and irregular, varying from 20 to 48 per minute; the inspirations were only such as might be looked for in a patient taking so many in that space of time. It was the expirations which gave the peculiar character to the breathing; instead of being of equal or greater length than the inspirations, they were short, hurried, and forcible, occupying only half the time taken up by the act of inspiration. This character of the breathing is always to be regarded as of grave import, more so than mere frequency, by which it is invariably accompanied; it indicates danger from the head-symptoms. The very rapid pulse showed that the case was a severe one, whilst the clearly perceptible cardiac impulse and good character of the systole showed that the danger was not from failure of the heart's action. The presence of even a trace of albumen in the urine was a symptom of grave import, but, be it remarked, this piece of evidence was not forthcoming until two days after the other symptoms had led to the conclusion that the case was likely to be a severe one. It thus seems that, without any aid from the thermometer, a perfectly just estimate could have been formed of the nature and probable course of the case. All the other symptoms showed that it was a severe one, and that the head-affection was the source of danger. What, then, was the use of the instrument in this case, which has been instanced as one tending to prove its utility? *It was the first thing which gave certainty to the unfavourable opinion which had been formed of the nature of the case.* The rise of the temperature at midnight of the 7th day, and the continued high range on the 8th, gave a stamp of precision and certainty to the prognosis which at that stage of the illness would otherwise have been wanting. On the 9th and 10th days the temperature was from half a degree to a degree lower, but there was an absence of the morning fall—a want of difference between the morning and evening temperatures—which, when persistent, is a sign of unfavourable import. This continued on the 11th and 12th days. At midnight of the former a rise of more than a degree took place, and on the 12th there was no fall; the morning range was nearly as high as the evening, and higher than that of the previous evening, whilst the midnight range was highest of all. The general state of the patient at this time was one of extreme danger, and the worst prognosis was given; his pupils were small, he was getting heavy and somnolent; the urine was scanty and contained more albumen; the abdomen was tympanitic; and though the pulse had fallen from 132° to 112°, it was doubtful whether, with such other symptoms, this could be regarded as of favourable import. On the 13th morning the temperature fell from 103·6° at midnight of 12th to 102·1°; in the evening it was 101·1°; and at midnight 100·5°; thus showing that defervescence had fairly set in; the pulse fell at the same time, and though the general aspect of the case was only slightly if at all improved, there was no hesitation in pronouncing the greatest danger to be past. From that time improvement was progressive. In this case the thermometer gave the first certain sign of impending danger, and likewise the first indication that the danger was past. True it is that, contemporaneously with the rise of the temperature, there took place an even greater rise of the pulse, and that throughout the course of the case the pulse was, as compared with the normal standard, higher than the temperature. It is also true that, when the temperature fell at the commencement of defervescence, the pulse continued to keep it company; but, nevertheless, the state of the pulse did not convey to my mind such definite and satisfactory information as was derived from the temperature, and that for the following reason:—when a patient is restless, excitable, and frequently endeavouring to get out of bed and perform other muscular feats (as this one was), the rapidity of the pulse and respirations is apt to be much increased by these means, and ceases to be an accurate and reliable guide to a knowledge of the patient's real state. The temperature, however, is not so affected. The mere muscular effort required to stand on the floor may send a typhus patient's pulse up 20 or 30 beats per minute, but has no corresponding effect on his temperature; hence it was that the fall of the pulse which took place on the 12th day, when the patient got heavy and somnolent, and ceased from his muscular efforts, was not (seeing that the temperature kept up) looked on as being of favourable import.

I shall give one more case of a similar character:

2. A female, aged 24, came under observation on 4th day of illness, with general febrile symptoms, but no eruption; pulse in morning 120, temperature 102·3: *vesp.* pulse 120, temp. 104·5°.

5th day.—Distinct typhus rash; tongue dry in centre; pulse 112, temp. 103·9°: *vesp.* pulse 120, temp. 105·1°; cardiac action good.

6th day.—Passed rather a restless night; eyes suffused; has great thirst; pulse 108, resp. 24, temp. 103·3°; urine slightly albuminous: *vesp.* pulse 114, temp. 104·3°.

7th day.—General state as last noted; pulse 120, resp. 44; temp. 103·3°: *vesp.* pulse 108, temp. 104·4°.

8th day.—Very restless; eyes suffused; urine contains more albumen, and deposits a few granular casts; pulse 128, resp. 30, temp. 103·1°: *vesp.* pulse 132, temp. 104·3°

9th day.—Very restless and noisy all night; tongue dry; rash very copious; pulse 128; temp. 102·7°: *vesp.* pulse 148, resp. 56, temp. 104·5°.

10th day.—As at last note; pulse 128, temp. 102·7°, resp. 44: *vesp.* pulse 140, temp. 103·1°.

11th day.—Passed a quieter night; seems heavy and depressed; eyes much suffused; pupils rather small; cardiac systole plain and audible; pulse 144, resp. 54, temp. 103·9°: *vesp.* pulse 144, resp. 42, temp. 104·3°.

12th day.—Rather more restless; has considerable stupor; urine still albuminous, and depositing granular casts; pulse 138, resp. 48, temp. 104·4°: *vesp.* pulse 148, resp. 38, temp. 104·6°.

13th day.—Is heavy and stupid; swallows very badly; tongue very dry, pulse 140, resp. 36, temp. 103·2°: *vesp.* pulse 156, resp. 48, temp. 104°.

14*th day.*—Still considerable stupor, but swallows better; urine albuminous; pulse 132, resp. 30, temp. 102·4°: *vesp.* pulse 136, resp. 36, temp. 103·3°.

15*th day.*—General state unchanged; pulse 132, resp. 30, temp. 101·2°: *vesp.* pulse 136, resp. 40, temp. 101·9°.

16*th day.*—Has less stupor; general aspect somewhat improved; tongue moist at edges; urine albuminous, deposits urates and a few granular casts; pulse 132, resp. 32, temp. 99·5°: *vesp.* pulse 132, resp. 38, temp. 101·1°.

17*th day.*—General improvement continues; pulse 124, resp. 30, temp. 98·8°: *vesp.* pulse 132, resp. 40, temp. 100·6°.

18*th day.*—Pulse 116, resp. 30, temp. 98·4°: *vesp.* pulse 120, resp. 28, temp. 99·9°.

19*th day.*—Pulse 116, resp. 24, temp. 97·3°: *vesp.* pulse 120, resp. 28, temp. 97·0°; tongue moist and cleaning; general aspect greatly improved.

The high range of the temperature early in this case led me to be on the look-out for head-symptoms, and to examine the urine, which was found as early as the 6th day to be slightly albuminous; as the case advanced, the albuminuria increased, and granular casts of the renal tubes were found in the deposit; the nervous symptoms ran high; the patient was restless; and had much delirium; the pupils were small, the conjunctivæ much injected, and towards the end of the case there was considerable stupor. It was not, however, till the eighth day that the general symptoms were such as to call for special notice. During the first week, the pulse was never above 120; on the eighth day, it rose and continued very high (from 132 to 156) throughout the remainder of the case; the respirations during the first six days were not high, from 24 to 32; on the 7th, they became hurried and irregular, and remained so throughout. The temperature was high *from the very first*, the evening range up to time of defervescence varying from 104·3° to 105·1°. There was thus, during the first week, nothing either in the general aspect and symptoms, or in the pulse and respirations, to induce me to regard the case with more than usual anxiety. The albuminuria was of course a most serious complication, and likely to be attended with bad head-symptoms; but it was the thermometer which, by showing a temperature of 105·1° on the 5th evening, gave the first warning note, and led to the belief that the nervous symptoms would run high, to the suspicion that there might be some renal complication, and to the formation, at an early stage of the illness, of a prognosis which the subsequent course of the case fully bore out, and which could not have been arrived at without its aid for some days later. It also gave the first indication of commencing defervescence. It was not till the 16th day that the tongue began to moisten at the edges, and that the improvement in the general aspect and symptoms of the case was such as to warrant the assertion, on these grounds alone, that defervescence had commenced: it was not till the 18th day that the pulse got down to 120; and on the 20th (on which day the observations ceased), it was still 112; whilst the temperature, which on the 11th and 12th days was high, and had shown little or no tendency to fall in the morning, gave, on the 13th day, decided signs of a decrease, which was even more marked on the 14th, and continued till the normal standard was reached. The thermometer gave indications of commencing defervescence three days earlier than any other symptom, and enabled the opinion that defervescence had commenced to be pronounced at least two days sooner than could have been done without its aid; for of course the fall of the temperature on the 13th day, unaccompanied as it was by any other signs of improvement, would have been of little or no value unless continued on the 14th.

These two examples serve to show that in those cases in which there is danger from the head-symptoms, the thermometer is a most useful aid to the early attainment of a just estimate of the patient's state: they are given as instances well calculated to show the benefit to be derived from the employment of the instrument, and as types of a class of cases which are of frequent occurrence during an epidemic.

Let us now see if it is of equal value in those in which the nervous symptoms do not predominate, but in which the chief source of danger is failure of the heart's action.

3. A female, aged 42, came under observation on the 7th day, with the eruption out, and general symptoms well marked; on morning of that day, the pulse was 112, respirations 36, temperature 102·5°: *vesp.* pulse 116, respirations 32, temperature 104°.

8th day.—Passed a restless night; tongue dirty; eruption abundant; conjunctivæ injected; pulse 120, resp. 40, temp. 102·7°: *vesp.* pulse 116, resp. 40, temp. 103·8°.

9th day.—General condition unchanged; pulse 116, resp. 36, temp. 103·2°: *vesp.* pulse 108, resp. 42, temp. 102·4°.

10th day.—Is still restless; tongue dry in centre; cardiac impulse and systole rather feeble; pulse 116, resp. 38, temp. 102·9°: *vesp.* pulse 102, resp. 40, temp. 102·8°. Wine to 6 ounces.

11th day.—General state as last noted; pulse 128, resp. 36, temp. 103: *vesp.* pulse 128; resp. 34, temp. 103·8°.

12th day.—Pulse 124, resp. 36, temp. 103·3°: *vesp.* pulse 128, resp. 40, temp. 103·3°.

13th day.—Is more sunk in bed; tongue dry and brown; cardiac systole faint, impulse scarcely perceptible; pulse 132, resp. 36, temp. 102·9°: *vesp.* pulse 148, resp. 44, temp. 103·3°. Omit wine; whisky to 6 ounces.

14th day.—As last noted; pulse 138, resp. 42, temp. 102·2°: *vesp.* pulse 132, resp. 48, temp. 103·4°.

15*th day.*—Passed a quiet night; is sunk down in bed; cardiac systole faint; pulse 132, resp. 42, temp. 102·4°: *vesp.* pulse 140, resp. 48, temp. 102·3°; has wheezing in both fronts.

16*th day.*—Skin moist; tongue moistening; has general bronchitis, causing some embarrassment of breathing; pulse 136, resp. 38, temp. 100°: *vesp.* pulse 120, resp. 48, temp. 101·2°.

17*th day.*—General state much the same; pulse 140, resp. 48, temp. 98·8: *vesp.* pulse 136, resp. 50, temp. 101·4°.

18*th day.*—General improvement continues; pulse 124, temp. 98·5°, resp. irregular: *vesp.* pulse 120, temp. 98·4°.

The general symptoms in this case were more those of depression than of excitement; the pulse was rapid after the 10th day, and so far partook of the character of the pulse in the two previous cases; but the cardiac phenomena were different. Instead of a clearly perceptible impulse and audible systole, there was a very feeble impulse and a first sound faint and indistinct; the range of the temperature was equable, and not at all high; it was not a good guide to prognosis, but was a reliable index of the declension of the fever. The general aspect of the case, and the feeble compressible pulse indicated that the danger was from failure of the cardiac action, but it was the stethoscopic examination of that organ which gave precise knowledge of the actual state of affairs, and led to a correct estimate of the amount of the danger.

This case is given in illustration of a very numerous class in which, though symptoms referable to the nervous system exist to a greater or less extent, the chief danger is from failure of the heart's action; it serves to show that in such cases the information derived from the thermometer is of little positive value, and that for the formation of a just estimate of the patient's condition, we must rely mainly on the evidence afforded by a stethoscopic examination of the heart. For the sake of further illustrating this point, I shall instance two cases of uncomplicated typhus, in which the thermometer not only conveyed no positive information on which to found a prognosis, but in which it even tended to mislead.

4. A female, aged 18, came under observation on the seventh day, with the eruption out and general symptoms well marked.

7*th day.*—Pulse 150, temp. 104·6°: *vesp.* pulse 144, temp. 104°.

8*th day.*—Pulse 150, temp. 104°: *vesp.* pulse 144, temp. 102·3°.

9*th day.*—Pulse 136, temp. 102·1°: *vesp.* pulse 148, temp. 102·6°.

10*th day.*—Is much depressed; extremities cold; cardiac impulse scarcely perceptible, systole feeble; pulse 144, temp. 100°; to have wine to 8 ounces: *vesp.* pulse 156; temp. 102·6°.

11*th day.*—Has rallied considerably, but still very low; pulse 132, temp. 100·8°: *vesp.* pulse 132, temp. 101·5°.

12*th day.*—General state little changed; pulse 132, temp. 100°: *vesp.* pulse 144, temp. 104°; cardiac systole still very feeble.

13*th day.*—Pulse 120, temp. 102·2°: *vesp.* pulse 128, temp. 103·5°.

14th day.—Has improved expression; tongue dry; pulse 120, temp. 100·3°: *vesp.* pulse 112, temp. 102°.

15th day.—General condition unchanged; pulse 108, temp. 99·8°: *vesp.* pulse 108, temp. 101°.

16th day.—Tongue moist; has improved aspect; cardiac sounds better; pulse 96, temp. 99·5°: *vesp.* pulse 92, temp. 99·2°.

17th day.—Pulse 100, temp. 99°: *vesp.* pulse 96, temp. 98°.

The pulse in this patient was very rapid, ranging from 120 to 156, never getting below the former number till defervescence commenced; she had the general aspect and signs of depression, and a cardiac systole faint and soft: the temperature, which was 104·6° on the 7th evening, fell somewhat on the next day, continued to fall on the 9th, and on the 10th morning was down to 100°. This low range, continued for two days, might, unless the other symptoms had been attended to, have been regarded as of favourable import; as it was, no signification could be attached to it, further than to regard it as the result of the great depression under which the patient was labouring, as indicated by a feeble and rapid pulse, coldness of the extremities, and a scarcely audible cardiac systole. The patient was freely stimulated, and under this treatment rallied considerably. On the 12th evening, the temperature was again up to 104°. On the 14th day defervescence began and was progressive.

5. The next case was similar in character, but even more marked. A male, aged 53, came under observation on the 6th day, with the eruption and general symptoms well marked.

7th day.—Pulse 120, temp. 103·8°: *vesp.* pulse 120, temp. 104·3°.

8th day.—Tongue moist; there is considerable delirium; has some hypostatic congestion of bases of both lungs posteriorly; pulse 124, temp. 104·2°: *vesp.* pulse 116, temp. 104·8°.

9th day.—General state unchanged; cardiac sounds fair; pulse 120, temp. 103·5°: *vesp.* pulse 108, temp. 103·4°.

10th day.—Passed a restless night; very delirious; constantly endeavouring to get out of bed; pulse 120, temp. 103·2°: *vesp.* pulse 120, temp. 103°.

11th day.—Is much sunk; pulse scarcely perceptible; eyes suffused; tongue dry in centre; is unable to swallow; apex beat not felt; first sound very feeble; pulse about 120, temp. 99·6°; to have an ounce of whisky with a pint of beef-tea as enema every hour: *vesp.* pulse 112, temp. 99·9°; can swallow; whisky to 12 ounces.

12th day.—Is considerably revived; cardiac systole still very feeble; pulse 112, temp. 99·3°: *vesp.* pulse 108, temp. 98·7°.

13th day.—Has muscular tremors; tongue dry; heart's sounds faint; pulse 112, temp. 99·2°: *vesp.* pulse 112, temp. 98·6°; continue stimulants.

14th day.—General state unchanged; pulse 108, temp. 97·2°: *vesp.* pulse 100, temp. 97°.

15*th day*.—Has still muscular tremors and great depression; cardiac sounds better, but still feeble; pulse 108, temp. 97·2°: *vesp.* pulse 108, temp. 96·4°.

16*th day*.—Passed a restless night; has still muscular twitches; pulse 112, temp. 97°: *vesp.* pulse 112, temp. 96·6°. Continue stimulants.

17*th day*.—Tongue dry in centre, pulse 120, temp. 98·3°: *vesp.* pulse 108, temp. 96·8°.

18*th day*.—Pulse 116, temp. 97·3°: *vesp.* pulse 116, temp. 98·2°.

19*th day*.—Tongue cleaning; cardiac sounds improved; pulse 108, temp. 98·2°: *vesp.* pulse 108, temp. 97·8°; whisky to 8 ounces.

As was to be expected in a man of his years, all the symptoms were well marked; he was restless and delirious at night; there was hypostatic congestion of bases of both lungs posteriorly; the pulse varied from 116 to 124, and the temperature during the first week was high (104·8° on 8th evening). On the 10th day, the pulse was 120, and the temperature 103·2°, the general condition being as above; on the 11th day, the temperature was, in morning 99·6°, and in evening 99·9°; on the 12th and 13th days, this fall continued, and on the 14th day, it was down to 97°, and remained below the normal standard till the 20th day. A fall of 3½° on the 11th day of illness, and a continued low range on succeeding days, would in most cases be proof of commencing convalescence; in this case, however, it was otherwise, for the patient's condition was one of the utmost danger; he was suffering from extreme depression; the pulse was small and scarcely perceptible, the eyes suffused, the tongue dry, the cardiac impulse not to be felt, and the first sound faint and distant; the patient was in imminent danger, could not swallow, and was only rallied by having an ounce of spirit, along with a pint of beef-tea, given as an injection every hour until he could take nourishment by the mouth. He gradually and slowly recovered, but it was not till the 19th day that the general state of depression, as indicated by the occurrence of muscular tremors, feeble cardiac action, etc. was such as to permit of a diminution below 12 ounces of the daily quantity of spirits; and yet, during the entire week in which the patient was in this critical state, the temperature was low, generally below the normal standard, and never reached 100°. The thermometer alone would have given a dangerously false idea of the state of affairs: the general aspect of the patient, and the condition of the heart as revealed through the stethoscope, were the proper guides to the formation of a just prognosis, and to the adoption of a proper mode of treatment. It may be said that the low range of the temperature was an index of the state of depression under which the patient laboured. Grant that it was so, the information derived from it could not, for a moment, be compared in value or precision with that which was derived from the other symptoms, and especially from the stethoscopic examination of the heart.[1]

To illustrate those cases in which there was danger from asphyxia, I shall choose two which were complicated with bad bronchitis.

6. A female, aged 19, came under observation on the 7th day; at that time the eruption was out, the tongue dirty, and there existed wheezing all over the chest; the respirations were hurried and irregular; pulse 132, temp. 102·9°: *vesp.* pulse 132, temp. 103·4°.

8th day.—Passed a noisy night; conjunctivæ injected; pupils of good size; pulse 132, temp. 103·5°: *vesp.* pulse 132, temp. 103·9°; is very delirious.

9th day.—Passed a better night after having an opiate; is now quieter; pulse 132, temp. 105·1°: *vesp.* pulse 132, temp. 105°; is noisy and delirious.

10th day.—Is more restless; bronchitis very troublesome; breathing greatly embarrassed; face and lips turgid; abundant sibilation, mingled with mucous râles all over chest; pulse 144, temp. 104·4°: *vesp.* pulse 132, temp. 102·7°.

11th day.—Breathing very bad, almost gasping; lips blue and face congested; pulse 132, temp. 101:1°: *vesp.* pulse 144, temp. 104·4°.

12th day.—General state much the same; cardiac action pretty good; pulse 132, temp. 103·6°: *vesp.* pulse 132, temp. 104·4°.

13th day.—As last noted; tongue dry; rests very little at night; pulse 120, temp. 101·4°: *vesp.* pulse 132, temp. 103·2°.

14th day.—Has improved expression; tongue moistening; chest symptoms still troublesome; pulse 120, temp. 100·7°: *vesp.* pulse 130, temp. 101·8°.

15th day.—Is still restless and somewhat noisy; tongue cleaning; pulse 132, temp. 101°: *vesp.* pulse 120, temp. 101·2°.

16th day.—Eruption faded; bronchitic symptoms still very marked; expectoration abundant; patient is still noisy and unmanageable; pulse 120, temp. 99·2: *vesp.* pulse 120, temp. 101·1°.

17th day.—Shows signs of general improvement in a more marked degree; pulse 108, temp. 98·6°: *vesp.* pulse 116, temp. 99·1°.

Improvement continued, but was very gradual; the temperature was some days in reaching the normal standard, and the pulse was still later in doing so; the bronchitic symptoms did not totally disappear for some weeks, and for three or four days after defervescence had really commenced the patient was still noisy and restless at night.

The first noticeable fact in the case is the high range of the temperature (105·1° on 9th morning) in connexion with the prominence of the head-symptoms; the chief reason for instancing it, however, is to show how, at the time when the patient was in the utmost danger from the bronchitis, and when great fears were entertained as to the result, the temperature fell from 104·4° (10th mo.) to 101·2° (11th mo.), more than 3° in 24 hours; had the patient been other than young and vigorous, it is probable that she would not have rallied; as it was, the cardiac action was good, and with the help of appropriate remedies, enabled the patient to struggle against the pulmonary embarrassment; by the evening of the 11th day, she had rallied considerably, and the temperature again rose to 104·4°.

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[1] In this case may also be noted the fact, that with a high temperature during the first week there were very prominent head-symptoms during the early part of the second.

Here the thermometer, if trusted to alone, would have misled; when taken in conjunction with the other symptoms, it suffers so by the contrast that its utility, so far as the estimation of the real danger from the bronchitis was concerned, seems almost *nil*. The cardiac action gave no indication of the danger, but was of use in showing that there was little or nothing to fear from failure in that quarter; the greatly embarrassed breathing, the blueness of the lips and face, and the stethoscopic examination of the chest, were the only criteria by which an exact knowledge of the patient's condition could be reached, and a correct estimate formed of the probability or otherwise of a fatal result.

The next case is one which proved fatal, and in which death was due mainly to the bronchitis.

7. A female, aged 31, came under notice on the 5th day, with the eruption out; pulse 116, temp. 103·3°: *vesp.* pulse 116, temp. 104·3°.

6th day.—Is restless; pulse 124, temp. 103·5°: *vesp.* pulse 124, temp. 103·5°.

7th day.—Cardiac sounds rather feeble; apex beat scarcely felt; pulse 124, temp. 102·6°: *vesp.* pulse 140, temp. 101·6°.

8th day.—Urine contains albumen and a few granular casts, sp. gr. 1028; pulse 112, temp. 101°, resp. 40: *vesp.* pulse 132, temp. 103·1°, resp. 48.

9th day.—General state unchanged; pulse 116, temp. 100·1°, resp. 40: *vesp.* pulse 124, temp. 102·3°, resp. 48.

10th day.—Cardiac sounds feeble, but quite audible; urine albuminous; pulse 116, temp. 101·4°, resp. 40: *vesp.* pulse 120, temp. 100·8°, resp. 40.

11th day.—Pulse 132, temp. 102·9°, resp. 44: *vesp.* pulse 128, temp. 103·2°, resp. 48.

12th day.—Passed a restless night; tongue moist; pulse 128, temp. 100·7°, resp. 44: *vesp.* pulse 122, temp. 103·6°, resp. 48.

13th day.—Passed a restless night; passes water and stools in bed; tongue dry in centre; rather feeble cardiac systole; has some wheezing in chest; pulse 128, temp. 101·1°, resp. 40: *vesp.* pulse 120, temp. 101·9°, resp. 42, irregular.

14th day.—Has general bronchitis; there is great wheezing all over chest, and considerable embarrassment of breathing;

pulse 116, temp. 100·6°, respirations irregular; pulse 112, temp. 99·7°.

15*th day.*—Is greatly depressed; abundant sibilation mingled with mucous râles all over the chest; tongue dry; urine still albuminous; pulse 108, temp. 97·8°, resp. 48: *vesp.* pulse 156, temp. 100·2°, resp. 54.

16*th day.*—Moribund; pulse 120, temp. 98°, resp. 54. Died at midday.

There are several points worthy of note in this case: (*a*) The occurrence of albuminuria was a source of anxiety, and led to the expectation of marked nervous symptoms; (*b*) the temperature, however, was not so high as to point to danger from that quarter; and the subsequent course of the case showed that the thermometer had not misled in that respect, for the head-symptoms were never such as to cause unusual anxiety; (*c*) the pulse and temperature varied much during the case; and (*d*) when bronchitis set in, the pulse and respirations kept up, whilst the temperature fell a little below the normal standard, and at time of last observation, a few hours before death, stood at 98°. Up to the time of the onset of the bronchitis, neither the pulse, temperature, nor general symptoms, indicated unusual danger; the first sound of the heart was rather feeble, but not so much so as to preclude a hopeful prognosis: with the occurrence of that complication, the general symptoms were, of course, altered by the addition of those referable to the pulmonary organs; the pulse underwent little change; the respirations became irregular and increased in rapidity; while the temperature fell, and continued to do so till it reached the normal standard. Here, as in the last case, the amount of the danger could be estimated neither by the pulse nor the temperature, but only by attention to the general symptoms caused by the pulmonary complication, and by a stethoscopic examination of the chest. In both instances, the fall of the temperature might be taken as an indication of the extent to which the pulmonary obstruction interfered with the proper oxygenation of the blood, and such, to a certain degree, it undoubtedly was, but the information so obtained was wanting in the precision and accuracy which accompanied that got from the other sources already mentioned, and would alone have been of little or no real value.

There is yet another class of cases to which I would allude, and of which I would give an instance. I refer to those not very common ones in which the eruption and general symptoms are so well marked as to leave no room for doubt as to their real nature, but in which the pulse rises little, if at all, above the normal standard.

8. A male, aged 27, came under observation on the eighth day of his illness.

8*th day.*—Abundant typhus rash; tongue dry in centre; conjunctivæ injected; pulse 68, resp. 26, temp. 103·2°: *vesp.* pulse 68, resp. 32, temp. 104·8°.

9th day.—Pulse 72, resp. 32, temp. 104·2°: *vesp.* pulse 64, resp. 34, temp. 104°.

10th day.—Eruption dingy; tongue dry in centre; passes water in bed; pulse 64, resp. 32, temp. 103·4°: *vesp.* pulse 80, resp. 32, temp. 104·4°.

11th day.—Passed a restless night; has considerable delirium; catarrhal wheeze in both fronts; tongue dry and brown; pulse 64, resp. 32, temp. 102·3°: *vesp.* pulse 78, resp. 30, temp. 103·3°.

12th day.—Eruption less dingy; otherwise as last noted; pulse 64, resp. 28, temp. 101·3°: *vesp.* pulse 56; resp. 24, temp. 102·3°.

13th day.—Tongue moist at edges; pulse 60, resp. 24, temp. 99·1°: *vesp.* pulse 56, resp. 20, temp. 99·3°.

14th day.—Pulse 40, resp. 18, temp. 97·3°: *vesp.* pulse 48, resp. 18, temp. 99·6°.

15th day.—Tongue moist and cleaning; has generally improved aspect; pulse 36, resp. 18, temp. 98·8°: *vesp.* pulse 48, resp. 24, temp. 97·5°.

16th day.—Pulse 36, resp. 16, temp. 96·2°: *vesp.* pulse 42, resp. 18, temp. 97·4°.

Here, then, was a well-marked case of typhus, with distinct nervous symptoms, and abundant dingy eruption, in which the pulse only once got up to 80, was habitually below the normal standard, and was, during the first week of convalescence, on two occasions, as low as 36, and not once above 48. The respirations were distinctly but not greatly increased in frequency. The temperature was the best, in fact the only means by which the amount of febrile disturbance could be accurately estimated.

9. A male, aged 21, came under observation after six days' illness.

7th day.—Skin hot; tongue furred; typhus eruption distinct; has a strong heaving cardiac impulse, and a slight diastolic blow at base; pulse 84, resp. 32, temp. 102·7°: *vesp.* pulse 88, resp. 40, temp. 103·4°.

8th day.—Has considerable stupor; eruption copious; pulse 88, resp. 38, temp. 102·1°: *vesp.* pulse 84, resp. 36, temp. 104·1°.

9th day.—Tongue dry in centre; pulse 88, resp. 32, temp. 103°.

10th day.—Pulse 84, resp. 28, temp. 103·2°: *vesp.* pulse 88, resp. 28, temp. 104°.

11th day.—General condition as last noted; pulse 84, resp. 32, temp. 102·7°: *vesp.* pulse 84, resp. 48, temp. 103·1°.

12th day.—Pulse 80, resp. 28, temp. 100·5°: *vesp.* pulse 80, resp. 28, temp. 102·2°.

13th day.—General state much the same; tongue not quite so dirty; pulse 76, resp. 32, temp. 102·3°: *vesp.* pulse 72, resp. 28, temp. 101·2°.

14th day.—Has improved expression; tongue cleaner; pulse 68, resp. 26, temp. 99·5°: *vesp.* pulse 72, resp. 28, temp. 99·2°.

15th day.—Improvement continues; pulse 64, resp. 18, temp. 97°: *vesp.* pulse 64, resp. 18, temp. 97·6°.

The remarks made regarding the last case are applicable to this one; the thermometer gave the best indication of the amount of "fever" which existed, the pulse being of no value in determining that point.

The cases already instanced (except the last two) are types of those in which there was danger of a fatal termination by coma, by asthenia, or by asphyxia. I shall now give the particulars of a well-marked, but mild case, which never presented any unfavourable symptoms, and which contrasts admirably with the one which I have instanced first, in which the nervous symptoms were high, and went on to convulsions. The cases were those of two gentlemen who acted as my assistants; they both had the same duties to perform, lived in the same manner, took ill about the same time, and were similarly situated in all respects, so far as external circumstances were concerned; they were, moreover, of the same age.

10. A male, aged 22, had the first observation made on the fourth evening of his illness, at which time the skin was hot and had a few elevated spots on it; the tongue was slightly furred; the pulse 104, resp. 24, temp. 103·3°.

5th day.—Distinct typhus rash; pulse 104, resp. 24, temp. 102·7°: *vesp.* pulse 104, resp. 24, temp. 103·5°.

6th day.—Had a pretty good night; is quite rational; tongue slightly furred; pulse 104, resp. 24, temp. 102·8°: *vesp.* pulse 104, resp. 24, temp. 103·3°.

7th day.—Pulse 108, resp. 26, temp. 102·7°: *vesp.* pulse 108, resp. 28, temp. 103·1°.

8th day.—Pulse 108, resp. 20, temp. 102·4°: *vesp.* pulse 116, resp. 28, temp. 103°.

9th day.—General state much as last noted; occasionally wanders slightly; pulse 116, resp. 20, temp. 102·2°: *vesp.* pulse 104, resp. 24, temp. 102·7°.

10th day.—Passed a good night; skin moist; tongue dry in centre; pulse 104, resp. 24, temp. 102·8°: *vesp.* pulse 108, resp. 26, temp. 103·4°.

11th day.—Slept well; skin moist; tongue dry in centre; pulse 104, resp. 20, temp. 100°: *vesp.* pulse 100; resp. 20; temp. 102·4°.

12th day.—General state unchanged; tongue moist; pulse 92, resp. 22, temp. 100·5°: *vesp.* pulse 88, resp. 18, temp. 99·4°.

13th day.—Expression natural; tongue clean; skin cool; pulse 80, resp. 18, temp. 97·7°; *vesp.* pulse 76, resp. 18, temp. 98·5°.

Here, along with a mild train of general symptoms, the pulse, respirations, and temperature showed a low range and marked equability throughout the case; in both respects it contrasts strongly with the one given first.

Treatment.—In mild cases, which tend to recovery, there is no occasion for any active treatment; a sensible nurse is the chief requisite. In severe cases, tending to death, prompt and judicious treatment is of the utmost importance. I have already pointed out the kind of cases in which the thermometer is of most use in prognosis. The same circumstances which teach us, at an early stage, to be on the outlook for head-symptoms, lead us also to adopt such measures as are calculated to ward off the threatened danger, and mitigate the severity of the attack when it does occur. When a high range of temperature indicates danger from this source, every care should be taken to keep the patient as quiet as possible,—the light should not fall on his face; his bowels should be moved every day by castor-oil or an enema, if necessary; his head should be shaved, and cold kept constantly applied; sleep, if not obtained naturally, should be brought on by opium (unless specially contra-indicated), with or without antimony, according as the pupil is small or large—a well-timed opiate is, I am confident, often the means of saving a patient's life; the bladder should be regularly examined to see that there is no retention—*the nurse's assurance that the patient has made water, should never be trusted to*, as incontinence and retention often go together. The bladder may be not only full, but running over; when such is the case, every care should be taken to promote the patient's comfort by keeping him as clean and dry as possible. Notwithstanding all these precautions, should he continue to get worse, and coma or even convulsions be apprehended, the scalp should at once be blistered, either by the ordinary emplastrum cantharidis, or (what I believe is better, as acting more speedily,) the acetum cantharidis,—of course care must be taken that the scalp is closely shaved, and that when the acetum is employed, it is of good quality, and not merely painted on, but rubbed in. As long ago remarked by Graves, the good effects of the cantharides are often brought about by applying it only for a sufficient length of time to produce a stimulant and rubeficient action, not pushing it to vesication, and repeating the application as circumstances may require; that is, I believe, the best mode of using it for the alleviation of comatose symptoms when they come on gradually and are not immediately alarming. In cases, however, in which the coma is deep or has come on rapidly, and in those in which convulsions supervene—in cases, in short, in which the head-symptoms are a source of immediate and urgent danger—it is better to push it on to vesication at once, and, for this purpose, the acetum cantharidis, employed as above indicated, is more prompt than the emplastrum. By the early adoption of the above measures, I feel confident that, in not a few cases, the nervous symptoms have been kept in check; and that in several of the cases in which they went on to coma and convulsions, they were so far mitigated as to be more easily controlled by the more active measures required for the treatment of these dangerous symptoms. Of eight cases complicated with convulsions, four recovered,—an unusually large proportion.

The great use of the thermometer, so far as treatment is concerned, is to point out, during the first week, the cases in which the head-symptoms are likely to be a source of danger, and so enable us to adopt proper precautionary measures sooner than they would be indicated by the other symptoms. It is useful in treatment in the same cases in which it is useful in prognosis. Where death is threatened by asthenia or by asphyxia, it is of little or no service; the general symptoms, and condition of the heart and lungs, being in such cases the best guides to the proper line of treatment.

Every case which I have instanced may be regarded as a well-marked type of a class; it must not be supposed, however, that all cases can be ranged under one or other of the heads indicated above. In the great majority, both the cardiac and nervous symptoms are well marked, though the nervous apparently preponderate, in consequence of their more obvious character. Every one who has seen much of typhus, knows that it is often difficult to say whether there is greater danger from failure of the heart's action or from coma; such cases require for their treatment the utmost nicety of judgment, and at the same time a prompt decision, which nothing but experience can give. When coma and asthenia clash (if I may use the term), the case is usually well advanced, and minutes are precious; indecision on the part of the physician is apt to be attended with the most serious consequences to the patient. Given such a case, what are we do? With the patient comatose, and the pupils small, can we act on the cardiac evidence and give alcohol freely? With a heart's first sound inaudible, and an impulse not to be felt, is it safe to create a discharge by blistering the scalp?—Yes; most undoubtedly. If you don't blister, the best means of overcoming the coma and rousing the patient are neglected; if you don't stimulate freely, the heart's action will fail. Cardiac feebleness does not contra-indicate blistering, if the head-symptoms call for it; the presence of coma is no reason for not stimulating, if the heart requires it. Spirits may be given and that freely, and their employment in many such cases beneficially supplemented by camphor and ammonia, remedies which are often of great service under such circumstances; as their action is transient, too long an interval should not be allowed to elapse between the doses. Finally, in consequence of the patient's inability to swallow, it may be necessary to administer both stimulants and nourishment per rectum. I have, occasionally, seen cases apparently hopeless saved by this means. Such cases should never be given up; treatment should be persevered in to the last; the patient may be just at the turn, requiring only to be kept alive for a few hours until the disease has run its course, and defervescence commences. At this stage of such a case, the thermometer conveys no information which can guide us in treatment; indeed, the symptoms are all so fully developed that no aid is required from it. It is often useful, however, in giving an early indication of commencing improvement.

The following statement shows that the thermometer is the best index of commencing defervescence :—

In 81 cases, in which the fact was specially noted, the fall of the temperature *alone* gave the first indication of commencing improvement in 18; the temperature and pulse declined simultaneously in 32; improvement in the general aspect and symptoms occurred simultaneously with a fall of pulse and temperature in 27; improvement in the general symptoms and a fall of the temperature preceded a fall of the pulse in 4: thus a fall of the temperature was one of the earliest indications in 100 per cent. of the cases; a fall of the pulse in 72·8 per cent.; and improvement in the general symptoms in 38·2 per cent. In making this calculation, I have omitted all cases in which there was any doubt as to the time at which improvement began. In connexion with the above statement must be borne in mind the fact already noted, that a fall of the temperature may be the result of great and alarming depression.

The average time occupied by the process of defervescence, that is to say, the time which elapsed between the hour at which a progressive fall of the temperature was first noticed, and that at which it reached the normal standard, was, in these 81 cases, 41 hours; the longest was 96 hours. In 13 cases, or in 16 per cent. of the whole, the fall was sudden, by crisis, and was completed between the morning and evening, or between the evening and morning observations; whilst in the remaining 68, or 84 per cent., it was by lysis, was gradual, requiring for its completion one, two, three, or even four days. In complicated cases, the temperature was often kept up for a length of time by the secondary affection.

Whether or not the temperature is raised by the complicating malady, seems to depend on the nature of the secondary disease, and the time at which it appears; when it springs up during convalescence, it raises the temperature if its nature is inflammatory.

The pulmonary complications which usually occur during the height of the fever, even though of an inflammatory nature, do not, so far as my experience goes, necessarily have this effect; and often, when fully developed (as in the two cases already instanced), tend to lower it. I am aware that this does not quite accord with what has been found by other observers, but honesty compels me to say, that whilst an elevation in the temperature has occasionally been noted at the commencement of an attack of bronchitis or pleurisy, I have seen several cases in which pleurisy, and many in which bronchitis occurred, without finding the thermometer give any indication of their presence. The action of pneumonia in this respect I have never had the opportunity to test. The only two affections which I have found invariably raise the temperature, are parotid swelling and inflammation of the ear; and only in the latter has the increased heat ever given the *first* indication of something wrong, and led to the discovery of the secondary affection.

Hæmorrhages (epistaxis, abortion, etc.), and diarrhœa, never have an elevating, but frequently a lowering tendency; this latter is, in great part, due to the fact that they are apt to occur towards the end of the case, when they may be of a critical character, and when the temperature, independently of their occurrence, may be expected to fall.

I do not think that age has much influence in increasing the range of the temperature in typhus; any apparent action which it has in that respect, is probably due to the fact that the blood-poison is more apt to produce acute head-symptoms in the young. The pulse is undoubtedly more frequent in children during the fever as well as during health, but this difference is not apparent after puberty has been reached. I have made a calculation of all my cases which recovered, and find that in those under 15 years the mean temperature was, in morning 102·8°, in evening 103·8°, the pulse being at same time 116 and 117; in those from 15 to 30, it was 102·9° in morning, and 103·4° in evening, the pulse being 112 and 112; in those above 30 the averages were—for the temperature 102·6° and 103·1°, and for the pulse 111 and 113.

Neither does sex have any practically appreciable influence; if it has any effect at all, the preponderance is probably on the female side, and may be accounted for by the greater delicacy of the nervous organization in that sex. In my cases (which by mere chance are exactly half males and half females), the average temperature for males was, in morning 102·6°, and in evening 103·3°; for females it was, at the same times, 103·3° and 103·4°.

I have never found the temperature of the ward have any effect on that of the body during the height of the fever.

My experience quite accords with that of other observers, when they say that the temperature invariably falls below the normal standard during convalescence, and remains so for some time. My own observations lead me to regard the generally adopted standard, 98·4°, as being, as nearly as possible, correct.

The facts given above serve to illustrate those on which are founded the inferences which I have been led to draw, after a large number of observations made on all sorts of cases. I shall try to summarize them, but would premise this by the warning, that the temperature should not be looked to alone, but should be taken in conjunction with all the other symptoms, and that any apparent neglect of reference to them in my remarks is to be attributed to a desire to be as brief as possible.

1. In the formation of a diagnosis, the thermometer gives, at an early stage of the illness, no evidence sufficient to enable us to distinguish typhus from other forms of continued fever.

2. As an aid to the formation of a satisfactory prognosis, and as a guide to a proper line of treatment, its value varies in different cases.

3. It is of most value in those in which there is likely to be danger from the head-symptoms: a high temperature during the first week being the earliest indication of danger from that source.

4. A morning temperature of 104°, or an evening temperature of 105, during the first week, and a morning range of 103·8° with

an evening one of 104·5°, after that time, should teach us to treat the case as one likely to be endangered by the severity of the head-affection.

5. The highest range occurs in cases which prove fatal by acute head-symptoms, and in which there is partial or complete suppression of urine.

6. Irregularities and abnormalities in the range often characterize the course of severe cases, and are of specially unfavourable import when occurring in those in which a high temperature has been noted at the end of the first week.

7. Equability of the temperature is one of the usual characteristics of a mild case.

8. A rise at midnight, and a *continued* absence of a morning fall, are (when the range is high) still further indications of dangerous nervous symptoms, though their absence does not denote the contrary.

9. When probable failure of the heart's action is the source of anxiety, the thermometer is of no value in enabling us to estimate the exact amount of the danger; the state of the heart, as revealed by a stethoscopic examination of that organ, being the best guide to a knowledge of the patient's condition.

10. When pulmonary complications exist to so great an extent as to threaten death by asphyxia, the thermometer conveys no reliable information regarding the amount of the risk, this being best estimated by attention to the general state of the patient, and by the stethoscopic examination of the chest.

11. In cases in which (either from greatly enfeebled cardiac action, or from pulmonary obstruction causing deficient oxygenation of the blood) the patient gets into a state of great depression, the temperature may fall to, or even below, the normal standard, indicating, not a commencing defervescence, but an extreme degree of depression, the exact extent of which, and the danger to be apprehended from it, are best estimated by a reference to the causes which produced it.

12. In ordinary uncomplicated cases which go on to a favourable termination, the temperature generally attains its maximum about the time of development of the eruption.

13. In consequence of the non-liability of the range of the temperature to be affected by various causes which have a disturbing influence on the pulse and respirations, the thermometer is the most generally accurate means of estimating the amount of febrile disturbance which exists.

14. As an indication of commencing defervescence, a fall of the temperature is earlier and more reliable than any other single sign of improvement.

15. The effect on the temperature of complications springing up during the course of the fever varies according to the nature of the secondary affection, and the time at which it appears.

16. Complications coming on during convalescence seem to act, in this respect, just as if no febrile attack had preceded them.

In conclusion, I would repeat, that the chief use of the thermometer in practice is to enable us, during the first week, to recognise and pick out with greater precision those cases in which the head-symptoms are likely to run high; later on in the case, when all the symptoms are fully developed, its chief use is to give an early indication of commencing defervescence. It must also be borne in mind, that severe head-symptoms indicate a probably protracted case; and that in elderly persons, and in young people of weak constitution, there is likely, towards the end of the second week, to be superadded to the other risks the additional one of enfeebled cardiac action, requiring its own treatment, but not interfering with that called for by the head-symptoms. As compared with those last mentioned, I believe that all the other indications of the thermometer are of secondary importance.

ARTICLE V.—*Craniotomy and Cephalotripsy Contrasted; with Cases.* By ALEXANDER MILNE, M.D., L.R.C.P. & S.

(*Read before the Obstetrical Society, 26th June* 1867).

CASE I.—On the 12th June 1865, I was called by Mr, now Dr Monro, to see a case of tedious labour in a primipara at No 9 Monteith's Close. The woman had been in labour for about thirty hours, and the liquor amnii had long escaped. The head was high up, and had made little progress for hours, and the pains were becoming irregular and weak. The os was pretty well dilated, but the soft parts were exceedingly swollen and dry, and painful to touch.

After evacuating the bladder, Mr M. put the woman well under the influence of chloroform, and I introduced my left hand, in order, if possible, to effect version. I soon found that this was out of the question, and for a triple reason—viz., 1*st*, The existence of a projecting promontory which debarred the ascent of the hand, and which, as it involved a great diminution in the conjugate diameter, would have debarred also the descent of an unreduced head; 2*d*, The presence of a tetanic condition of the uterus (a tonic contraction so obdurate that nothing short of powder and shot could have prevailed against it, and which the anæsthetic, in this instance, failed to overcome); and, 3*d*, A wretchedly dry and tumified state of the vagina. In these circumstances, there was nothing left but to perforate. This was easily accomplished, but about a couple of hours was occupied in getting home the child, or the remains of it rather, the crotchet and craniotomy forceps slipping frequently. A good deal of trouble was experienced with the shoulders. After it did come home, by the face as recommended by Burns and others,[1] it was found, as usual, that though the parietal, occipital, and other bones were considerably picked away, the base (which may be said to be the basis of the whole concern in such cases) was totally unassaulted. The woman made a somewhat slow recovery, being on her legs only in about three weeks.

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[1] Dr Braxton Hicks, whose paper on the best method of delivering the fœtal head after perforation, is well worthy of perusal.

Case II.—On the third of April last I was requested to see Mrs. S., a young married woman in labour with her first child. On arriving at ten A.M., I found that the pains were few and far between, and hardly genuine, and left, promising to call in the evening. At nine P.M. the pains were true, but moderate. I passed my finger to the os, but found it opened only so far as to admit a finger. It was somewhat rigid and unyielding. No presentation could be felt. There being a suspicion of an impacted rectum, I ordered an enema of warm water and ol. ricini, and left. I was sent for again at one A.M. The enema had done its work; the pains were pretty strong, regular, and frequent, but the os expanded slowly. No presenting part could be detected during a pain, but by pushing two fingers of my left hand well up, I could make out the head lying well up above the brim. While searching for the presenting part, I could detect a most suspicious sacral promontory, projecting and dipping as it did over the brim. I waited a little to see what the natural power might effect, but a couple of hours of excellent pains gave almost nothing in the shape of advance of the head. I knocked up Mr. Thomson, Nelson Street, to relieve me a little. I said to him on leaving, "I suspect a narrow brim here, but wait on, and encourage the woman; give her good cheer, mental and material, and now and then apply the finger, well larded, to the os." I was summoned again at seven A.M. The os was now dilated to about the size of half-a-crown; it was softer but yet undilatable. The head, notwithstanding that the pains had kept strong and regular, was still high up. I resolved to measure the brim. Mr Thomson anæsthetized the woman, and I introduced two fingers of my left hand, the fore and the middle one, into the os, pushed them well up, and separated them, and found that the extremity of the one impinged on the promontory, and that of the other on the pubis, and that without much stretching. As the tips of my fore and middle fingers, when voluntarily stretched to the utmost, only extend to a space of three and a quarter inches, I concluded that, in this case, I had a brim to deal with which antero-posteriorly measured probably not more than two and a half inches. Craniotomy was therefore (the child also being at the full time) necessitated. I was quite prepared for the disagreeable work, but subscribing, as I did heartily, to the canon that it is desirable to destroy singly as seldom as possible, I resolved on having a consultation. Sir James Simpson was communicated with, and, with that kindness and unbounded self-denial which, as is well known, characterize him in an extraordinary degree, responded to the call. On arriving, he speedily made out the nature and necessities of the case, and resolved on employing the cephalotribe. After failing to detect the pulsation of the fœtal heart by means of the stethoscope, he began the operation. The patient being placed under the influence of chloroform by my friend Mr Thomson, Nelson Street, Dr Black being also present along with Dr Suttie, the head, notwithstanding that it was very high up, and the os not fully dilated, was perforated with consummate ease and skill. One of Sir James' own cephalotribes—a pigmy beside the gigantic one of Baudelocque—was then introduced with some difficulty, but with much coolness, tact, and skill, and traction begun. Great force, after the blades were brought together, was necessary, and the instrument slipped.[1] Quickly another was brought from the inexhaustible bag, and as quickly applied, and it retained a good hold, and after a good deal of force delivery was effected. The whole time occupied was only twenty-five minutes, and less would have sufficed if the instrument had not slipped. I feel persuaded that with the crotchet or craniotomy forceps, so contracted was the pelvis, a much longer time—probably several hours—would have been required, and much more difficulty experienced. There was little discharge afterwards, and the uterus contracted well. The after progress was very satisfactory. There was a little pain over the uterus twenty-four hours afterwards, but it was effectually met by an opiate. She was able to pass water soon. On the eighth day she was moving about the room. On examining the head afterwards, the bones, including the base, were found to be effectually crushed, and the widest part was about two inches. I may just add that the distortion was due to rickets, Mrs S. being six years old before she was able to walk.

Remarks.—Instead of simply commenting on these cases, or on the more prominent features of them, I have thought that it would not be out of place were I to enter more fully into the general subject, seeing that there is a growing feeling of dissatisfaction with old tools, and a waxing desire for new ones; that we are, in fact, in the middle of a kind of transition period, when old implements are being weighed in the balance, and when new ones are asserting their claims, and struggling hard to attain the vantage-ground of general acceptance and use. My remarks, I thought, might properly take the direction or shape of a contrast of the old system and the new—the crotchet and the cephalotribe—in order, if possible, to find out on which side most merit or advantage lay; to which practice our support and suffrages should go; which instrument we should adopt when placed in the trying position of being called upon to reduce the fœtal head. I say called upon, for there is no doubt that pelves will continue every now and then to cast up through which heads unbroken will not be able to descend, any more than the Irish elk or the mastodon, if they were resuscitated, could go through the eye of a needle. Of course there is a way of escape from the perforator, viz., the Cæsarian section; but this has been found, at least in this country, to be an outlet bristling with more perils, a pathway paved with more of ill-fortune to the mother. Abroad —in France and elsewhere—they are said to manage things better (that is, save more lives) by incising earlier; but the statistics are probably not altogether reliable,—are, perhaps, in fact, like certain guides in outlandish parts of a country, fitted only to lead one into a region of chaos and doubt.

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[1] This was solely owing to the screw having given way.

In beginning the comparison, I shall take the older instruments first—the instruments in common use for the purposes of reduction and extraction. I need not go over the black catalogue of tools of destruction, nor enter into a minute description of the many instruments which have been invented; it will suffice that I notice the more common and prominent of them, and especially the manner in which they have performed their unpleasant work, even in skilful hands. There is the perforator, the crotchet, the blunt-hook, and the craniotomy and bone forceps. The first instrument, the perforator, must be excluded from the comparison, because it also precedes the cephalotribe, perforation being the necessary preamble of successful cephalotripsy. The other instruments have one function mainly—they are used chiefly for the purpose of traction. Of course, by means of the craniotomy and bone forceps, and even in pulling with the crotchet, you may gradually break down the cranial bones, and reduce the volume of the head; but then you cannot at the same time employ anything like extractive force with respect to the bulk of the head. The system, in fact, in legitimate—that is, in extreme cases, and even in the most skilful hands, is one of pulling and picking, seizing hold and letting go, like a dog with an unmanageable bone; every slip, if not every fresh seizure, being pregnant with risk to the mother. Yes, crochet, and blunt-hook, and craniotomy forceps, are all open to the objection of losing hold, and requiring re-application, over and over again. I repeat that I refer only to extreme cases of contraction, for heads in some cases have come down after mere perforation; in others the fingers have served the purpose of forceps and crochet to boot; but in these cases I should say there might be well-founded doubt whether perforation were necessary at all—whether, in fact, the grand, yet simple and generally safe, operation of turning might not have been the soundest practice on the occasion.

But I shall now go from mere assertion to evidence,—to the recorded experience of some of the worthies of the obstetric art. Every one knows that Hamilton has spent nearly half a day upon it, toiling and resting a bit, and beginning again; and that able veteran and arch-craniotomist (I do not use the term offensively), Dr Lee, has spent nearly as long a time, often sweating like a gladiator in a ring, or a mariner in a storm. Here is a sentence in corroboration from case 32,[1] in his record of difficult labours:—" I opened the head," says he, "at one o'clock, but four hours elapsed ere the delivery could be completed. After all the bones of the skull had been torn to pieces with the crotchet in extracting, I passed the crotchet up between the head and the uterus, to fix its point on the face, or in one of its orbits. Unfortunately the point of the instrument came in contact with an arm, instead of the outside of the head, and the slightest force brought the arm into the vagina, converting the case into one of arm-presentation." Case 31 is equally striking. Denman—simple-minded, yet acute and experienced, and imbued in a peculiar way with everything obstetrical—says:[1]—"Where the resistance has been very great, after making my efforts (*with the crotchet*) with all the force and skill I could exert, without success, I have desisted for an hour or longer, and then renewed my attempts." Meigs has taken an hour or two to remove as much osseous matter as would be equal in bulk to a parietal bone; and Burns, whose work is still a quarry rich in worthy material, depones also to the skill, and force, and time required.[2]

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[1] Clinical Midwifery, p. 47.

Tyler Smith bears similar testimony. He says,[3]—"With any of these instruments the greatest care is necessary in guarding the soft parts of the mother from injury, and avoiding lacerations of the vagina and perineum by the instruments, or by spicula of bone. Great force is sometimes required with the crotchet and craniotomy forceps, and the completion of delivery may occupy many hours."

Again, Dr Ramsbotham observes,[4]—"If the pelvis be considerably contracted, we may expect that much exertion will be necessary, and much time will be spent, before the head passes through the brim, and we must not be disappointed in finding the bones break constantly, and piece after piece come away."

Lastly, not to multiply references unnecessarily, Smellie says,[5]—"I have been so fatigued from the crotchet slipping its hold so often, that I have scarcely been able to move my fingers or arms for many hours after."

Experience thus testifies that with the crotchet and craniotomy forceps, the operation has been, as a rule, a tardy and prolonged one, and as "time is," as Denman observes, "equal to force," and force almost equivalent to injury, we are, I think, also justified in characterizing the system as one open to grave objections—as one, in short, injurious to the mother. Statistics bear this view fully out, for they show that about one in six of the mothers has perished. It is true that they may have died in some cases as much from the delay prior to the operation, as from the operation itself; in others from attendant complications; still, few will deny that unusual delay in cases of this kind is a decided evil, and that brevity, other things being equal, is a most blessed boon. Why, labour itself is potential enough to kill if unduly prolonged, and needs not the finishing stroke of a tedious and protracted operation.

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[1] Introduction to Midwifery, p. 310.

[2] Principles of Midwifery, p. 450.

[3] Manual of Obstetrics, p. 601.

[4] Obstetric Medicine and Surgery, p. 311.

[5] Theory and Practice of Midwifery, p. 304.

Death, suffering, sorrow, and woe, therefore, like the prophet's roll, have marked craniotomy in the past; shall there be less of this in the future, or shall it be but a repetition, a reproduction, of the past? Time alone will be able to tell. One thing, however, will be freely conceded, viz., the desirableness of doing everything in our power to render an unavoidable operation as harmless as possible, of leaving no stone unturned whereby we may, while inevitably sacrificing one life, spare the other, on which a higher value is commonly set, in relation, if not to supernal interests, at least to mundane concerns.

With this worthy view various instruments have been invented from time to time—some good, some bad, some indifferent—and, in later years, one very prominent tool has been given birth to, viz., the cephalotribe. Of it we shall now speak.

The Cephalotribe.—This instrument, the chief credit of which is given to Baudelocque,[1] has not made much progress in Britain as yet, if we except our own metropolis, where most things, at least of a decent and reasonable and feasible nature, get an early footing and fair trial; it has yet its way, as it were, to fight,—it has yet its laurels to win. When first introduced, it was viewed with suspicion and distrust, with dislike, and even dread. Obstetricians, when they viewed it, stood aghast; they shrank back as a small animal would on the approach of a lion, or as Soult recoiled when he saw the formidable lines of Wellington. The obstetric manuals, too, discoursed of it in forbidding terms, and declared that it must go to the lumber-room or limbo, where many odd and useless things rest till the crack of doom. Now, this indifferent reception and greeting was, no doubt, due in some measure to the formidable character of Baudelocque's instrument being, as it was, by far too bulky and heavy, and, in fact, more like a mill for grinding dry goods than anything else; but also, doubtless, in great part to that somewhat prevalent conservative spirit which leads those animated by it to cling to ancient ways and things, like ivy on a mouldering ruin, and almost to shut their eyes to the sight and their ears to the mention of a novelty;—a spirit under whose benighted reign many useful inventions have progressed with the tardy movement of the snail, or been strangled altogether; a spirit that, had it been general—which happily it has not, and, in these days of unrest and innovation, of searching inquiry and keen experiment, and astounding liberalism in every sphere, less likely to prevail every day—might have seen and found us yet withdrawing the life's-blood, *ad deliquium* and *ad libitum*, in all kinds of disorders; convulsing the intestines, with all the zeal and zest of a Hamilton, on the smallest occasion; loading our maniacs with heavy chains; and in ignorance of the blessings of chloroform.

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[1] The earliest known cephalotribe was invested by Assalini. It is described in a work published at Milan in 1811.

But although the cephalotribe did not escape the usual reception—did not find a ready niche in the regards of the bulk of the obstetric profession—it failed not to secure the consideration of enterprising men. These, as became men of wisdom and liberal spirit, surveyed it carefully; they segregated the good of it from the bad of it, the beauties from the defects; retaining and adding to the former, and casting aside the latter. Among these we recognise the leading obstetric men in our own "romantic" city, and chief among them our own distinguished president.

But, now, what are its merits? Is it superior to other instruments, or is it not? Probably a longer probation is yet requisite before the question can be fully or fairly answered. At all events, now that it has been cleared of some of its more formidable points —made lighter in construction by Sir James Simpson, Braun, Dr Charles, Dr Kidd, Dr Hicks, and others—it is achieving unwonted success. It is now not much more bulky than a pair of long forceps.

The principle of it is excellent; it has a two-fold function; it both crushes and extracts; it is, so to speak, forceps and blunt-hook and crotchet in one. While you are crushing you are also seizing a bull-dog hold for the purpose of extraction—a hold of ten-fold security as compared with the craniotomy forceps, or any kind of hook under the sun. The advantage here is, that you may require only one introduction and one withdrawal, and thus happily and mercifully escape the weary and painful seizing and losing hold, pulling and picking, and ceaseless twisting and tearing, until the exhaustion of the operator is nearly equal to that of the hapless and suffering patient. With it your work may be over, and done well too, in the course of a very few minutes—ten, fifteen, twenty. Moreover, its crushing power is great, and herein lies one of its most favourable features; for it breaks down swiftly the cranial bones, including also the base, which latter is hardly reducible or assailable by the other instruments even after the expenditure of much time and skill and pains. Indeed, if you survey the crushed heads in the obstetric museums, you will find that, as a rule, the base has escaped all the assaults of the craniotomist, who, mayhap, has bestowed hours on the work of reduction.

But a further advantage is that, by means of this instrument, you reduce the bones without much, if any, tearing of their coverings. The spicula then, if there are any, do not protrude, but are encased in the safe envelope of an almost unbroken scalp. Soft parts thus escape the chance of injury always dreaded under the other system from sharp edges of bone during the descent of the mangled head. Swiftness, in short, characterizes the cephalotribe in contradistinction to the interminable delay of the crotchet—a delay the evils of which are too well known to require repetition—and greater security and safety, directly and indirectly, so far as the mother is concerned.

But in case these advantages should be deemed merely fanciful or theoretical, let us inquire a little into the practical working of the instrument. Theory and practice, it need hardly be said, do not necessarily comport; nay, they are often widely different, from unforeseen elements casting up, and coming into adverse play. But abroad, Bandelocque, Champion, Cazeaux, Scanzoni, Dubois, and others have employed it very successfully; and in this country, Simpson, Keiller, Kidd, and others, have also tested its value.

The case I have detailed illustrates most strikingly the worth of the implement; it shows how quickly and thoroughly and safely it can accomplish its appointed work. One or two other cases may be briefly referred to. There is Dr Balfour's case, recorded in the *Edinburgh Medical Journal*.[1] Here there was a very contracted brim, and turning had been resorted to on previous occasions, after the induction of premature labour. The cephalotribe did its work speedily and well in the hands of Sir James Simpson.

Another is that in the same Journal[2] by Dr Keiller. Here the long forceps and turning were found quite impracticable, but complete success attended the use of the cephalotribe, and the mother again recovered well.

The last case I shall refer to is one that happened in the practice of my friend Dr Bryce, Dalkeith.[3] The case was one of extreme contraction, and Dr B., after opening the head, had "three hours fruitless effort with the crotchet." Yet Sir James Simpson, who had been sent for, succeeded in effecting delivery by means of this instrument in the course of twenty minutes. The patient was able to be out of bed on the ninth day.

Such are the merits and advantages of this instrument, then, not theoretical merely, but as exemplified by successful practical work; but it is now proper that we should inquire whether it has any demerits or drawbacks. There are two sides to most questions: the famous shield had two aspects; and the cephalotribe may not yet be a model of perfection, though vastly improved; it may have some fault or defect fitted to mar its usefulness, calculated to depreciate our estimate, to lessen somewhat our jubilation. The chief, if not the only one, I can perceive, lies in the bulk of its blades[4] (this bulk being necessary in order to give strength and crushing power), which must ever render it of difficult application in cases of high distortion. Still, though the task may seem in some cases nearly equal to that of Sisyphus, skill and tact will triumph.

Dr Tyler Smith,[5] no mean authority, avers that it is quite inapplicable in these extreme cases; but he does not add whether this view is the result of personal experience or mere fancy and conjecture. I rather think the instrument is as little in vogue in the south yet as oatmeal is in the diet of Englishmen.[1] If Dr S. means to affirm that it cannot be applied where the antero-posterior diameter is only, say, 2½ inches, then we demur; for it has succeeded well in pelves not exceeding that limited dimension; nay, Dr Kidd of Dublin has succeeded with a conjugate of 2 inches. Under this you are almost safer to go to the Cæsarian section, according to Churchill, Cazeaux, and others. You may, it is true, drag a fœtus, or the remains of it, through a conjugate of 1½ inches according to Gardien, and Hamilton and others—Dr Davis says even through one of a single inch—but what a miserable wreck must your patient be after the protracted and sanguinary work implied in the reduction of the child to an attenuated mass capable of emerging from, or rather being dragged through, a gap so contracted as this, and how small must be her chance or prospect of survival! The high distortion cases have seldom done well, at least under the old system, the mother having, in many instances, been *moribund when the remains* of her offspring were being hauled through her unfortunate pelvis; having in fact succumbed immediately after the terrible work was accomplished.[2] How much better, in such cases, might it not have been to have brought the living infant through the abdominal wall, which is now-a-days cut with an impunity that one could hardly have dreamed of, and which would startle Hippocrates were he to arise from his grave!

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[1] Feb. 1865.

[2] Ibid., Nov. 1864.

[3] Ibid., Aug. 1864.

[4] Most of the instruments are now made much lighter, and yet compress very effectually.

[5] Manual of Obstetrics, p. 597.

Such, then, are the advantages and disadvantages of the cephalotribe, imperfectly set forth it may be; and I think, on the whole, we are justified in claiming for it higher value—a more exalted platform than the crotchet and its companions. Success has crowned its employment as a rule; it has fairly realized expectation, and it has not withered confidence and hope. It is true, as before hinted, that it is perhaps not yet so perfect as it might be—that it is mayhap not the handiest and fittest instrument we shall yet get from the hand of genius (perfection, indeed, in mechanical appliance may never arrive, any more than the philosophers' stone, or a catholicon, or Utopia itself),—still it is a step in advance to be hailed with gladsomeness by the obstetrician—an impression on the obstetrical sands not to be effaced—a stand-point from which fresh and real progress may be begun.

The imperfection of instruments, it may be remarked in passing, constitutes a call upon our mechanically-inclined brethren to be up and doing, in order, if possible, to eclipse even the greatest inventors who have yet evoked our hearty thanks and praise. A really good and useful obstetric instrument is about as rare as an angel's visit, but its advent, when it does come, is seraph-like—a mission of good, a crusade against ill. Should this barrenness exist? Certainly not; there is no occasion for it in these days of wonderful invention. There are wondrous machines for cutting the mellow grain, and transforming natural products into the most beautiful fabrics, and even for the despicable work of entrapping small animals, and making sausages, etc.; and shall it be said that we are still badly off for safe and suitable means of launching children into the world—that many still land infants as men land fish, by hooks? Surely not.

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[1] My friend Dr Braxton Hicks is using it (and has his own instrument), and so also are Dr Graily Hewitt and Dr Greenhalgh.

[2] Boyer witnessed the mutilation and extraction of the child repeatedly by eminent men, but the mothers sank immediately. There are several such cases mentioned also in Dr Lee's clinical work.

In pursuing the work of obstetric invention and improvement, it is true that no pension may await our highest and most successful efforts. No, our statesman will remunerate fourth-rate poetry, and fifth-rate fiction, and puerile prose, and the fierce inventors of big guns for blowing up fellow-mortals by the score, and even now and then quietly pitchfork a dreary nephew into a snug berth—a square man into a round hole—but small gratitude awaits, and small lustre gathers around, the man who gives us a tool for the saving of those precious lives without which there would neither be statesmen to guide the vessel of the state, nor warriors to defend their country. However, there is one grand incentive to the work, apart from the bauble of renown, or even the "bubble of reputation," which are sometimes only posthumous, and then avail you not, and often indeed "interred with the author's bones," and then avail his family not (if he has any); and that is the assurance that every advance you make into the region of real improvement is an aggression upon that of mortality. Yes, every defect you remove from, or useful addition you engraft on, your instrument, goes to conserve life, to lessen the percentage of death. Persevere, then, in the high interests of life, and help to consign all imperfect instruments, including the crotchet, to the curiosity shop, where finally repose rusty swords, lances, and even lancets innumerable, alongside of all sorts of ancient-flavoured and dismal dust.

Remarks on Case II.—I find I have left but little time or space for any remarks on the cases recited at the head of this paper, and which provoked the foregoing, I fear too lengthy, observations. There are, however, one or two points in connexion with case No. II, on which I should like to remark briefly. It will be observed that in this case the brim was made out to be a narrow one at a comparatively early period, and before the full dilatation of the os. Was there any advantage in this? Or in other words, did the woman derive any benefit from the discovery? I think she did. For instance, the operation was in consequence begun only fifteen hours after labour had commenced in earnest, and before exhaustion had set in. It thus gave her a better chance of recovery, for she had more of strength left wherewith to bear the brunt of the interference. It is true that there has always been a school that propounded the propriety of delay, even on to the unconscionable period of several days: but few are the disciples of it now; they dwindle beautifully down into that significant obscurity which is so worthy of the hurtful doctrine they espouse. Why, it is almost a law that an operation undertaken during great exhaustion will be followed by a fatal result. The surgeon is loth to lop off a limb when his patient is in a state of profound debility; and he will sometimes let him perish rather than run the risk. How much more cautious require not we to be when our operation may be much more prolonged and equally severe!

Now, in many of the cases of contraction recorded, great delay has taken place; days have been spent in the almost passive work of waiting for an impossible descent—reminding one of the Arcadian sitting by the river's brim waiting for the stream to run dry; the consequence being that the patients sunk from sheer exhaustion soon after the operation. They died probably more from the delay than the interference itself. By this system the operation is discredited; the dark side of its statistics is increased; for the results are rendered less favourable than they would otherwise have been. This has been pointed out by Sir James Simpson and Drs M'Clintock and Churchill. The latter says[1]—"This delay is unfavourable to the mother, and when at length the operation is performed, her condition has rendered her much more susceptible of injury from it."

Dr Lee frequently complains of this delay. Glance at case 52.[2] This one dragged its painful length over Saturday, Sunday, and Monday, and the mother died from inflammation. Dr Lee in remarking on it says—"*She was left too long in labour.*" Case 57 is even more suggestive. It had been allowed to carry on from Saturday to Wednesday, and proved fatal twelve hours after the operation. Dr Lee says[3]—"The result of this case would probably have been very different had we proceeded to deliver twenty-four hours sooner; and I can never think of it without regret."

The great thing, then, is an early and correct diagnosis of the character of the pelvis. On this may hang the mother's existence. Whenever serious and impracticable disproportion is made out, early and energetic action should follow, and delay be swept unceremoniously from our resources as a hurtful expedient. I need hardly suggest again that for measuring the brim, the hand, especially the left, or two fingers of the same, is the best of all apparatus for the purpose.

There is just another point on which I would say a word before closing: it is in reference to the use of the stethoscope in this case. It is well known that there are not a few practitioners who shirk the perforator until the child is dead, or supposed to be so. They throw the heavy burden of killing upon Nature, feeling scarcely broad-shouldered enough to bear it, or having too little of fatalism about them to give them the necessary intrepidity. For them the stethoscope is a proper enough article to resort to, affording, as it does, some little negative evidence of the death of the child; but for those who deprecate delay, who set the highest price on the mother's life, it seems to hold no suitable place, to subserve no useful function. Their doctrine is this, and I agree with it: in a case of distorted pelvis, where the child unreduced cannot pass, the sooner delivery is accomplished the better for the mother, and the fact that the child is living is no bar. Dabbling with the stethoscope, then, is all very well for the procrastinating school, for the so-called conscientious and humane stock—and the feelings of such individuals are worthy of all respect—but it is useless, so far as the other and opposite side is concerned; for, according to their views, and consonant with their practice, they would perforate even though they heard the ticking of the fœtal heart. Living or dead, the infant's bones must be crushed within a reasonable time, that the mother may not perish. I said "humane stock," but a qualification is necessary; for the system of sparing the perforator until the child is dead is in reality a cruel method in reference to the mother. You may hang on a day or two ere the infant succumb, the patient all the while suffering the utmost distress. However, while condemning the measures, let us yield all due homage to the men, for the motives by which they are animated are most worthy.

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[1] Theory and Practice of Midwifery, p. 372.

[2] Clinical Midwifery, p. 52.

[3] Ibid., p. 53.

I fear I have dwelt too long on this subject; but I felt it to be one of the most interesting and important in the whole range of obstetrics. I trust, if I have not thrown any fresh light on the matter, I have at least not imported into it any unwonted darkness; and if I should contribute in the smallest degree to further the great cause of maternal safety, my intentions will be fully met, and my remarks will not be in vain.

Part Second.

REVIEWS.

On Diseases of the Lungs and Air Passages; their Pathology, Physical Diagnosis, Symvtoms, and Treatment. By HENRY WILLIAM FULLER, M.D. Cantab., Fellow of the Royal College of Physicians, London; Physician to St George's Hospital, etc., etc. Second Edition. London: John Churchill and Sons: 1867. 8vo, pp. 523.

WE are glad to observe that Dr Fuller's work has so soon reached a second edition. Under such circumstances, it seems hardly necessary to repeat the favourable opinion we formerly expressed. The book may now be considered to have taken its place among the standard treatises which represent the state of our knowledge of the subject at the present day. Though less minutely elaborate than Dr

Walshe's manual, the work of Dr Fuller contains a full account both of the physical methods of diagnosis, and of the symptoms and treatment of the diseases of the lungs; and what is a chief recommendation, the author's statements are based upon or confirmed by his experience as an accurate clinical observer in a large hospital. Some of his views are novel and original, and many of them differ in important particulars from those commonly received. In short, the work is one which deserves to be read and studied both by practitioners and students. There are a few points, however, to which we desire to call attention.

In the present edition the chapters relating to diseases of the heart and great vessels have been omitted, having been published separately; and the present treatise is therefore strictly confined to diseases of the lungs and air-passages. With this exception, there has been no material alteration in the plan or arrangement of the book.

The subject-matter is divided into two parts. The principles of physical diagnosis in their application to the lungs are explained in Part I.; and the description of the symptoms, course, and treatment of the different diseases of the lungs is reserved for Part II. The former occupies one-third, the latter two-thirds of the volume.

In the first part the methods for the exploration of the chest are carefully laid down, and the various physical phenomena not only described, but explained. To some of these explanations, especially Dr Fuller's theory of the production of ægophony, we formerly called attention, and need not now return to it. We find the author opposed to the suggestions made in recent times—proposing to refer the fine crepitation of pneumonia to the "sudden unfolding of the walls of the air-cells, rendered dry and crackling by disease," or to "the giving way of exudation matter which has been effused between the vesicles in the substance of the lung." Dr Fuller considers these views untenable, and adheres to the older opinion, which ascribes the fine crepitation to the passage of air through the fluid effused in the pulmonary air-cells. The author also criticizes, with some severity, Skoda's theory of consonance as a cause of highly developed vocal resonance. On this point, however, the opinions of the Viennese professor have not met with much acceptance in this country. It must be admitted, nevertheless, that several acoustic phenomena revealed in percussion and auscultation, are not satisfactorily understood. For example—the clear percussion occasionally observed over solidified lung in pneumonia, and the resonance in the upper part of the chest when the pleura is nearly full of fluid, and the lung compressed, have been a source of difficulty to most modern authorities, and have been variously accounted for by Hudson, Stokes, Graves, Walshe, Williams, Skoda, etc. Dr Fuller's discussion of the subject is very ingenious; but we doubt that his explanation, by the presence of air in a portion of the lung in the immediate vicinity of consolidated tissue, will not account for the phenomena in all cases. In many instances the lung has appeared completely condensed, without any portion containing air being observable. The question requires further investigation. Altogether, Dr Fuller's description of the physical signs, and their explanation, is clear and satisfactory; and though the details are necessarily somewhat dry, may be read easily and with interest. We have noticed a few inaccuracies. At p. 32, the "duration of sound' is stated to be "dependent on the frequency with which the vibrations are repeated," which is not what the author can have intended to express. The well-known German physician who invented the percussion hammer has his name modified to Winterlich; and in fig. 2 of the frontispiece, it must be a misprint to state that "the lungs are supposed to be unexpanded." The distended stomach, in the same figure, has an unnatural appearance.

Part II., which treats of the diseases of the lungs, possesses, of course, more direct practical interest. In any treatise of the kind there must necessarily be a great deal repeated which is commonly known. It is sufficient that ascertained facts and principles be clearly and pointedly stated; but on disputed or doubtful points Dr Fuller has a decided opinion to offer, founded on the record of his own observation. Our own experience would lead us generally to agree with his statements, which are at once judicious and practically important. Occasionally, however, both on controverted topics and others which are usually assumed to be settled, we find ourselves at variance with the author. Thus, in regard to the resolution of pneumonia, if we understand the following paragraphs rightly, we are obliged to dissent. Speaking of the stage of red hepatization (p. 251), the author says:—

> "When resolution takes place at this stage of the complaint, a serous fluid is poured out, which leads gradually to the separation and liquefaction of the fibrinous matter previously effused; and as the liquefied matters are expectorated, the lung becomes more pervious to air.
>
> "Ultimately the exuded matter may be thoroughly got rid of by expectoration, the engorgement of the pulmonary capillaries may pass off, the colour and sponginess of the lung return, and its tissue may resume its normal appearance."

In this passage, the removal of the exuded matters is described as taking place entirely by means of the expectoration. In point of fact, this can, we think, be proved to be incorrect. Thus, in not a few cases of pneumonia, the expectoration is extremely scanty or altogether absent; and yet, nevertheless, a large portion of lung in a state of hepatization is found to undergo resolution. It is by absorption, and not by expectoration, that the exudation is chiefly, if not exclusively, removed in pneumonic consolidation.

Another statement which appears to us to be questionable, occurs at p. 254. "In some few instances," says Dr Fuller, "especially when pneumonia arises in connexion with acute rheumatism, tubercular breathing is rapidly induced, but crepitation never occurs," a fact which he believes to be "attributable to the occurrence of exudation into the interlobular cellular tissue, and immediate occlusion of the air-cells." It does not appear to us that sudden consolidation requires us to resort to such a strained hypothesis as the one suggested, which is not based on any substantial evidence.

The treatment of pneumonia, although a subject trite and tedious, is too important for us to pass over altogether. Dr Fuller quotes his statistics with evident satisfaction. "Although patients," he says, "are admitted into the hospital, of all ages, and in all stages of the complaint, I have lost only four out of 78 cases; and of these one died the day after admission, and the other three were in an advanced stage of the disease before the treatment was commenced."

This success must be regarded as very complete, for no treatment could be responsible for failure when commenced in an advanced stage of the disease. "Whether this favourable result," he adds, "is attributable to my supporting the patient and avoiding depressing treatment, it is difficult at present to decide; but inasmuch as the statistics from which the more unfavourable results are deduced relate to cases in which copious venesection and other actively depressing treatment was employed, it seems not improbable that such may be the case." In plain English, Dr Fuller ascribes his success to avoidance of the depressing, and adoption of the supporting plan of treatment. This is well; but we do not quite reconcile this statement, *first*, with the foot-note at p. 139, where, speaking of the pulmonary congestion which occurs in fevers, etc., and of pulmonary œdema, Dr Fuller remarks,—"It is obvious that the active and depressing treatment which might be appropriate in combating inflammation of the lungs, would be productive of mischievous results in cases such as as these" (*i.e.*, of congestion or œdema); and, *second*, with the details of treatment which Dr Fuller recommends. The following are the remedies he advises:—Venesection, when clearly indicated (which, we presume, means when there is great congestion, and the disease is at an early stage); tartar emetic in $\frac{1}{4}$ to $\frac{1}{8}$ grain doses, at intervals of three or four hours, occasionally in combination with salines and moderate doses of digitalis; and, in some instances, when tartar emetic fails to exert a curative action, or is not well borne, calomel and opium in repeated doses, together with digitalis and full doses of alkalies and neutral salts. "Speaking generally," says Dr Fuller, "no single remedy is so useful as tartar emetic;" and he adds afterwards—"If my views are correct, the efficacy of mercury is especially conspicuous in those cases of pneumonia in which tartar emetic is of least avail," namely, "in those instances of the disease which are accompanied by a more than ordinarily plastic exudation—where crepitation either does not exist at all, or is replaced in a few hours by intense tubular breathing." Dr Fuller also thinks that the external application of ice, which certain German physicians have recently advised, may prove a "most valuable ally." And of the veratrum viride, so much in favour with American authors, he says, "I have seen enough to convince me that it deserves a further trial." We willingly believe that in Dr Fuller's hands all these means are judiciously, temperately, even cautiously used; but is this what any one can call avoidance of depressing treatment? Because the patient is "allowed good beef-tea, and is encouraged to drink freely of milk and whey, in which an egg may be beaten up," are we to regard the whole plan of treatment as directed to support the patient? Do the beef-tea and milk outweigh all the tartar emetic, digitalis, and calomel? We are often not a little surprised at the powerful restorative virtues ascribed to beef-tea and milk in acute diseases by recent writers; but however this may be, and admitting their tonic powers, it cannot be questioned that the tendency of tartar emetic, digitalis, veratrum viride, and ice, is to depress, and that calomel enfeebles rather than invigorates. Dr Fuller's treatment must therefore be regarded as of a mixed kind; the diet tonic, the remedies depressing, but not pushed to excess. And we cannot therefore concede that the success which he claims is due simply to "supporting the patient, and avoiding depressing treatment;" nor can we express our approval of the remedies which he recommends.

In the pneumothorax, which occurs in tubercular disease of the lungs, it was deduced by Dr Walshe, from the statistics which he collected, that the perforation took place far more frequently on the left than on the right side, in the proportion of 55 to 29. Dr Fuller's experience is different. Out of 18 cases of his own, the pneumothorax was found on the right side in 10, and on the left in 8. He quotes a similar conclusion from the statistics of St George's Hospital, as given in Dr T. K. Chambers' *Decennium Pathologicum.* Of 21 fatal cases there enumerated, in 12 the perforation had occurred in the right side, and in 9 on the left. Dr. Fuller is therefore inclined to believe that there is probably little or no difference in the liability of the two sides to suffer from this complication of phthisis. These authoritative statements reopen the question. Our own experience has been entirely confirmatory of Dr Walshe's results as to the greater frequency of left pneumothorax.

The subject of pulmonary consumption occupies deservedly a large space in the latter part of the volume. The record of Dr. Fuller's experience, and the opinions which he entertains in regard to what he justly calls the commonest and most fatal malady to which, in our climate, the human race is subject, will be perused with interest. There is a very good clinical history of the course of the different varieties of tuberculosis, and the chances of prolonged life, and the possibility of cure, are impartially discussed. The various kinds of treatment are appreciated by the results of practical experience; and although a certain vagueness is sometimes disappointing, it is perhaps impossible, at the present time, to find scientifically accurate data in regard to the real effects of remedies or particular methods of treatment. We must often rest satisfied with the general impression of success or failure which is created in the mind of the practitioner by the results obtained within the sphere of his personal observation. It is unnecessary, and would be tedious, to particularize the many points in which we consider the author's observations valuable and judicious; as, for example, his remarks on the advantages of fresh air, as opposed to confinement in-doors, and the suitableness of cold countries for some, and of warm climates for other cases of consumption. Some of the statements made, however, provoke criticism. We expected that the author would, in this edition, have omitted his objection to the terms "pulmonary consumption" and "tubercular diseases of the lungs," as ignoring the constitutional nature of the affection. Surely the term "tubercular" implies something more than a local mischief to every educated medical mind. The single terms, "consumption," "phthisis," or "tuberculosis," which Dr Fuller proposes to substitute for pulmonary consumption are quite inadequate. There are many kinds of consumption, and there may be tuberculosis of many organs besides the lungs. The whole symptoms and clinical history differ according to the locality which the tubercular disease chiefly attacks. There is a tubercular phthisis of the lungs, and a tubercular phthisis of the kidney, and so on. Indeed, so unsatisfactory would the term tuberculosis be by itself, that it would indicate the general pathology of tubercle rather than any special disease which is its effect. And we find, accordingly, that Dr Fuller himself retains "pulmonary consumption" as the heading of his chapter. If he added "tubercular," the name would be sufficiently accurate, distinguishing the particular disease meant from other forms of pulmonary consumption which are not tubercular,—which is the chief distinction required. A considerable omission in Dr Fuller's treatise is the absence of a description of the non-tubercular forms of phthisis pulmonalis.

Dr Fuller is decidedly opposed to the ordinary belief, and considers it a mistake to suppose that youth is the "harvest time of consumption," and that "middle age is the extreme limit of the period within which the whole crop is garnered." "Nothing," he says, "can be further from the truth, or more utterly at variance with modern statistics." We admit that the unqualified statement which Dr Fuller gives as the popular belief is exaggerated, and we doubt if any well-informed physician holds it to the extent stated. But to maintain, as Dr Fuller does, that the mortality from consumption does not vary materially between the ages of 15 and 70, appears to us to be rushing into the opposite extreme of error. The statistics he quotes from St George's Hospital and the Registrar-General (for 1857) are not shown to be free from obvious fallacy. The mere post-mortem presence of tubercle in the lungs is a very different thing from death by tubercular pulmonary consumption; and we certainly should never look to the returns of the Registrar-General for scientific proof in regard to any pathological question whatever. We confess that we remain sceptical as to the equal distribution of the mortality from pulmonary consumption over nearly all the periods of life, and hold to the old belief that youth possesses the melancholy privilege of excessive proclivity to scrofulous disease of the lungs.

As the author has included the "pathology" of diseases of the lungs among the titles of his work, we must express regret at the absence from his pages of any account, indeed of almost any notice, of the more recent advances in the pathological investigation of tubercle. The experiments of Villemin on the inoculability of tubercle are only cursorily referred to by way of footnote or interpolation. And when an author tells us at the present day that, "when the blood has become vitiated up to a certain point, an unorganized material, which has been termed tubercle, is deposited in various parts of the body," we perceive, only too plainly, how far his pathological knowledge has been left in arrear by the actual progress of science. This defect, however, does not, for the present, impair the value of the clinical and practical observations, which make the chapter on pulmonary consumption one of the best in the book.

Clinical Illustrations of various forms of Cancer, and of other Diseases likely to be mistaken for them, with especial reference to their Surgical Treatment. By OLIVER PEMBERTON, Surgeon to the General Hospital, Birmingham. London: Longmans, Green, Reader, and Dyer: 1867. Folio, pp. 128, with numerous wood-engravings and lithographic plates.

AS surgeon to one of the largest, oldest, and most celebrated provincial hospitals in England, Mr Pemberton has enjoyed a very extensive field of observation, which he has cultivated with much zeal and success. Already well known to the surgical world, through many important contributions to its literature, especially on the subject of the melanosis and excision of the knee-joint, he has now, in this most beautiful work, added greatly to his well-deserved reputation.

The scope of the work is admirably described on his title-page. It does not pretend to be an elaborate account of the varieties and relations of cancer, or even a treatise on its pathology, but it consists of an extensive and valuable series of clinical illustrations of the various forms of cancer.

The method of arrangement is a double one, and very convenient in a practical point of view,—the well-known separation of scirrhus, encephaloid, melanotic, and epithelial growths being preserved as in a *systematic* course; while the various cases of scirrhus are further subdivided into three chapters, and grouped together according to their local position in mamma, rectum, etc., thus preserving the *clinical* character of the work. In like manner, seven chapters are given to the illustration of the various positions in which encephaloid cancer is most frequent, one to melanoma, seven to epithelial sores, and one to rodent ulcers.

A great part of the work consists of the actual cases of the patients, as noted at the time; when it is possible, the previous history and result are also given. They are given simply, and yet with sufficient fulness to be very interesting and instructive. Some of them are selected on account of their rarity; others from being typical cases. Among the comparatively rare ones is a very well told one of scirrhus of the thyroid (p. 27), and a magnificent specimen of encephaloid of the cranium (p. 31). Mr Pemberton was fortunate enough to obtain the complete skull of the latter case, and in a most masterly woodcut figures the macerated skeleton of the tumour (fig. 3). A case of encephaloid of the mamma (p. 58) is remarkable from the extreme rapidity of its growth and reproduction after excision.

One practical lesson, we may take the liberty of pointing out to Mr Pemberton, is well taught by several of the cases of encephaloid cancer—viz., the very great dangers of the experimental punctures he so often practises. Rarely necessary or instructive as a means of diagnosis, a puncture often seems very markedly to hasten the rupture, protrusion, and even sloughing of encephaloid cancers—(pp. 72, 73)—and ought always to be avoided.

A very good woodcut (fig. 14) is taken from a section of the thigh of a young girl, amputated by Mr Pemberton in 1866 for well-marked encephaloid cancer. The girl is still alive and well, with no recurrence of the disease. This result is sufficiently rare to add great interest to a distinction which Mr Pemberton endeavours to make out between the encephaloids which infiltrate the whole substance of a leg bone, and those less dangerous ones growing merely from the outer lamella (pp. 75, 76).

A case of deep-seated abscess in the head of the tibia seems to have presented unusual difficulties in its diagnosis from encephaloid cancer, but eventually was trephined, and with the most perfect success. The peculiar paroxysmal toothachy pain seems to have not been well marked.

The question as to operative interference in cases of encephaloid cancer is very fully and carefully discussed. That disarticulation should always be preferred to amputation, in every case except in those affecting the lower third of the thigh-bone, is now generally agreed upon. Unfortunately that is the most frequent seat, and it is still *sub judice* whether amputation through the upper third of the thigh, with its greater risk of recurrence, should or should not be preferred to the most dangerous but more thorough removal gained by amputation at the hip-joint. The fortunate case alluded to above very naturally gives Mr Pemberton a leaning to the less severe operation.

The chapters on epithelial growths and rodent ulcers are very well illustrated by typical cases, and numerous ugly but expressive woodcuts.

Few medical books have ever been so lavishly adorned. Massive binding, thick paper, wide margins, copious illustration by woodcuts and lithographs, show that the eminent firm by whom it is published have had full powers given them to make it externally one of the handsomest books of the kind ever given to the public.

Some of the plates (those from preparations) are remarkably well executed; others (from patients) are comparatively sketchy, and occasionally out of drawing.

Such works, the cream of the experience of large hospital practice, are always welcome and invariably instructive; would there were more of them! Few men are qualified to write a great systematic work, still fewer to condense their knowledge into a little one; but every surgeon worthy of the name has observed cases, and gained experience, which are unique and original, and by publishing them he may add to the mass of facts from which alone science can advance.

Contributions to Medicine and Midwifery. By T. E. Beatty, M.D., etc. Dublin: 1866.

On the Signs and Diseases of Pregnancy. By T. H. Tanner, M.D., etc. London: 1867.

These two works bring to mind two other excellent works. Dr Beatty's book is a collection of his papers, such as his eminent townsman Dr M'Clintock had previously published. Dr Tanner's volume recalls the classical work of Montgomery on the Signs and Symptoms of Pregnancy.

Dr Beatty's volume is a handsome one. The papers in it are highly respectable productions. Most of them are now out of date; some of them mere addresses to societies, scarcely demanding republication. The book will form an ornament of the library shelves of the author's friends, reminding them of an agreeable professional colleague, and one of the most successful of modern Irish medical practitioners.

We have much pleasure in calling the attention of our readers to the volume produced by Dr Tanner, the second edition of a work that was, in its original state even, acceptable to the profession. We recommend obstetrical students, young and old, to have this volume in their collections. It contains not only a fair statement of the signs, symptoms, and diseases of pregnancy, but comprises in addition much interesting relative matter that is not to be found in any other work that we can name. It suggests strongly to us, as we read it, how desirable it is that we should have a good encyclopædia of obstetric literature. The topics now comprehended under such a designation are so numerous and diversified that none of our regular works includes them. They have to be sought for in year-books, or in still more voluminous journals, and run great risk of being entirely forgotten. Numerous valuable researches should be embalmed in an encyclopædia of this kind, not merely because they are the results of labour, and are statements of difficultly acquired truth, but because they are destined some day to be turned to practical account, and should be made easily accessible to all who desire to advance the science and art of obstetrics.

Several inaccuracies have crept into Tanner's work which we trust he will exclude from the third edition.

On Ringworm: An Inquiry into the Pathology, Causes, and Treatment of the several Diseases to which this term has been applied. By WILLIAM J. SMITH, M.B. Lond., etc.

THIS is a small treatise devoted to the elucidation of that much misapplied term ringworm, which the author has used in the widest possible acceptation; as under this head he describes no less than twelve distinct affections of the skin, which either are called ringworm, or at least have some resemblance to it.

We should have much preferred that the author had confined himself to a description of the true ringworm, the tinea tonsurans, and merely introduced those other affections with which this one might by any possibility have been confounded in his differential diagnosis. He would thus have had more space to devote to a full and clear enunciation of all the main facts connected with the subject. We might not then have had to complain of that carelessness in composition, that want of method in the arrangement, that total absence of anything like definition, and that persistent ignoring of some of the most characteristic symptoms of the affections treated of (and in this we refer more particularly to the description given of tinea tonsurans), which pervades this production. Moreover, if this brochure be intended for the enlightenment of the medical profession, as we suppose it to be, what physician of any pretension to education would be at all likely to mistake scabies, or herpes iris, or eczema, for tinea tonsurans?

We repeat that Mr Smith has failed to present us with a good, reliable, trustworthy monograph, such as the subject demands, simply because he has attempted to give us too much.

On the Principles of Æsthetic Medicine, or the Natural Use of Sensation and Desire in the Maintenance of Health and the Treatment of Disease, as demonstrated by Induction from the common Facts of Life. By JOSEPH PEEL CATLOW, M.R.C.S. London: 1867.

THIS book is but the fragment of a work which the sudden death of the author has unfortunately put an end to. Mr Catlow seems to have been a man who, amid his own daily round of duties, had thought out for himself a method of treating disease apparently based upon the relations of vitality to surrounding nature, adopting the views of John Brown in regard to the dependence of life upon "intrinsic capacity and extrinsic influence." The author is, however, aware of Brown's errors, and the injurious tendency of many of his doctrines, adopting and giving him due credit only for the theory of life above referred to,—a theory which John Brown was the first distinctly to propound, and which is yet destined to bear good fruit in regard to the maintenance of health and the treatment of disease. It is singular that, while Brown's practical errors, developed into Brunnonism, for many years more or less guided the practice of all Europe, except, perhaps, the country of his birth, where that phase of practice was never much in vogue, it is only in these latter days that the scientific truth contained in his propositions seems likely to have due justice done to it. We regret that the author's death has prevented the complete publication of his views in regard to therapeutics, of which we have in this fragmentary volume scarcely any intimations at all, the bulk of it being taken up with such questions as "The laws of social life and the relation of man to his Creator," "The relation of self-denial to the development of individual and social life," and "On the influence of vital agents on the organs and functions;"—all subjects of the utmost importance, even in relation to therapeutics, but difficult to handle so as to bring out that relation intelligibly. It is, however, curious to have the influence of vital agents upon the economy treated of with scarcely a word as to the existence of a nervous system at all, but wholly according to the principles of "æsthetic and orexic" biology as developed and modified by instinct in the earlier years of life, and by mind at a more advanced period. This book is in many respects an interesting volume; we regret that its fragmentary nature, and the sad cause of that imperfection, both place it beyond the pale of criticism.

Die Nerven der Gebaermutter und ihre Endigung in den glatten Muskelfasern. Von Dr F. FRANKENHAEUSER. Jena: 1867.
The Nerves of the Uterus, and their Termination in the Smooth Muscular Fibres. By Dr F. FRANKENHAEUSER. Jena: 1867.

DR FRANKENHAEUSER is not unknown to us. He has contributed to obstetrical science some valuable and elaborate researches. His present imposing large folio monograph, with eight plates, he styles a "Contribution to Anatomy and Gynækology." It is truly a splendid addition to the obstetric library, as well as a valuable contribution to anatomy and physiology.

With such investigators as Frankenhaeuser, the science and practice of midwifery cannot fail to make strides. Before saying something more particularly regarding the work, we would attract attention to the beautifully executed plates, drawn by the author and lithographed by Schütze, of Berlin. They leave nothing to be desired, for elegance.

Frankenhaeuser begins at the beginning, and goes to the farthest visible end of his subject. First, he considers the writings of Vesalius, Eustachius, De Graaf, Willis, Haller, W. Hunter, Tiedemann, Walter, Lee, Snow-Beck, and many others, and traces the nervous supply of the uterus as high as the solar plexus in the sympathetic system, and from the sacral nerves of the cerebro-spinal system. He follows the nerves through the great uterine plexus and the cervical ganglia of R. Lee, and through the ovarian nerves into the tissues of the uterus. Then, in connexion with the minor subdivisions of the nerve-fibres and the termination of the minutest fibrillæ, he takes up the writings of Remak, Klebs, Henle, Beale, Arnold, His, and others, and traces the minutest knot-bearing fibre into the very nucleolus of the nucleus of the muscular fibre. Lastly, he enters upon the most recondite questions of the structure of smooth muscular fibre, criticizing the views of Frey, Kölliker, Margo, Moleschott, Kehrer, Heidenhain, Meissner, and others. The book is required for consultation by all anatomists, physiologists, and obstetricians who wish to go to the bottom of the subject.

It is evident, from a perusal of the work, that the nerves of the uterus have been the subject of investigations far more searching and laborious than they have ever been subjected to, and by a man possessed of more varied resources than his predecessors in the same special department.

The history of every topic is carefully given; and it is rather painful to see how easily the author finds errors in the works of previous writers, especially of British,—at least their names strike the British reader most readily. For instance, while he points out many errors and imperfections in Lee's researches, arising especially from the want of histological verification as they proceeded, he is still more astonished at Snow-Beck's denial of the existence of the enormous cervical ganglia, and of the lesser vesical ganglia which Lee has the honour of discovering. The subperitoneal ganglia of Lee, on the other hand, he shows to be a mere delusion, arising from the neglect of histological assistance.

The great cervical ganglia are the chief sources of the uterine nerves.

Lectures on the Progress of Anatomy and Surgery during the present Century. By Sir William Fergusson, Bart., F.R.S., etc., late Professor of Human Anatomy and Surgery to the Royal College of Surgeons of England, etc. Pp. 302, with upwards of a hundred woodcuts. London: John Churchill and Sons: 1867.

The twelve lectures which constitute this work have been already before the notice of the profession, having been published in the *Lancet* soon after their delivery in the summers of 1864 and 1865. They were delivered at the Royal College of Surgeons of England during the two years in which their author held the position of Professor of Human Anatomy and Surgery to that distinguished body. The eminence of the author, and the importance of the position he then held in the College, demand a more full and critical notice than generally falls to the lot of reprints. The principle on which the author has acted in his choice of subjects for lecture also adds to the interest of the work; for, in the introductory lecture, he tells us that he has selected "subjects in which I have myself taken special interest or had peculiar opportunities of studying."

The twelve lectures may be taken, not in the order in which they were delivered and in which they are printed, but in groups according to their subject. The first group of four includes the first and second lectures of the first year and the first and last lectures of the second year. Written in a peculiarly conversational style, they wander from topic to topic with a pleasing variety of illustration. The history of the schools of Edinburgh and London during the last forty years, Sir Kenelm Digby's sympathetic powder, the merits of lithotrity and subcutaneous tenotomy, Dr Williamson's case of excision of the ulna, anæsthetics, and the number of medical societies, form the salient topics in the first and introductory chapter. The second, on Conservative Surgery, is discursive, with some repetition, as if written only from notes, taken in shorthand; but, nevertheless, it gives excellent illustrations of its subject, drawn from cases of fingers preserved from amputation, joints removed by excision, cases of removal of the scapula and arm, and Mr Syme's case of excision of the scapula and a portion of the clavicle, preserving the arm, which Sir W. Fergusson justly characterizes as the *ne plus ultra* of conservative surgery. The first of the second series on Minor Surgery almost of necessity is, to a slight extent, a repetition of the others already mentioned, but is full of excellent observations on the powers of nature, the merits of simple water-dressing as compared with various astringent lotions, the advantages of early removal of tumours, the necessity of having all the instruments in the room before an operation, the disadvantages of too tight bandaging (wonderful to relate, *without* John Bell's *in* *terrorem* diagram), etc. The last lecture, on operations, contains many precepts about the preparation of instruments, assistants, sponges, etc., prior to an operation; enforced by a repetition of the story of the absent saw, and the equally classical tale of the flaps made in the wrong direction. The woodcut, representing an operating table, is curious in its pre-Raphaelite attention to details of costume and indifference to the laws of perspective.

Turning from the generalities, the next group of lectures, three in number, is devoted to the subjects of lithotomy and lithotrity. The first gives the results of the author's experience in lithotomy on children under fifteen; the average is very good—fifty cases and two deaths; and then a brief view of his experience of lithotrity, which he repeated, with additions, in another lecture on lithotrity in the second series. The third, on lithotomy, will be read with much interest by all who have seen the author operate. His results are as follows:—He has cut 162 cases; of these 35 died—a mortality of 1 in 4·6, or 21·6 per cent. The apparently large mortality is explained by the fact that only 50 were below 15 years, and that the operation was refused only once to a hopeless case. Still, after deducting the 50 children and then 2 deaths, a mortality of 33 in 112 seems large, and may serve to remind us of the fact, that even in the most skilful hands lithotomy in the adult is a most dangerous and formidable operation. Hence we turn with pleasure to the author's lithotrity statistics: he has had 109 cases, of which 12 only have died. These statistics, so fairly and candidly given, are a very valuable contribution to the subject, as comparing the two operations in the truest way. The lecture was illustrated by the author's large collection of calculi. The next group consists of two lectures on excision of the knee-joint. They treat rather of the propriety of the operation than of the method of its performance, and argue from the point of view that a good limb after a successful excision is better than a stump, however strong, and a wooden leg, however ingenious. This will be granted by many surgeons who may have grave doubts as to the more general question of the propriety of excisions as a rule, when contrasted with other forms of local treatment in the milder cases, and amputation in the more severe. The author's statistics of forty cases, with fifteen deaths, do not disprove the assertion that the operation of excision of the knee is more fatal than amputation of the thigh for disease of the joint. The opinion of one who, though to a certain extent a partizan, still has had a larger experience of the operation than any other surgeon, must carry its own weight. To Sir W. Fergusson, too, is owing the immense impetus which the whole subject of excision of the knee-joint has received, and it is to his pupils, Price, Smith, and Swain, that we owe the chief English contributions to the literature of the question.

The chapter on hare-lip and cleft palate is the best in the book. Whatever may be the differences of opinion as to whether the author's method of dividing the muscles has any special advantage over the incision of Dieffenbach, and the separation of the soft palate from the hard of Mason Warren, there can be no doubt that Sir W. Fergusson has had an enormous experience of the operation of staphylorrhaphy, and an equally large measure of success. To have operated on 134 cases, with only five failures, speaks volumes both for the dexterity and patience of the operator. The honour of priority in the suggestion of the plan of the operation for closing fissures of the hard palate is gracefully conceded to Dr Mason Warren, whose untimely death has deprived American Surgery of one of its brightest ornaments. The anatomical details of harelip are carefully given; indeed, this chapter is the only one which contains an allusion to the "progress of anatomy." The author states that in man the fissure in harelip, whether of bone or soft parts, never occurs in the middle line but always at one or both sides. A case (so far as known to us, the only one on record) has since been very fully and carefully recorded by Dr Mason Warren,[1] in which there can be no doubt that the fissure both of bone and soft parts was in the mesial line.

The lecture on operations on the jaws is chiefly remarkable for its advocacy of a principle of treatment, which, to say the least of it, is liable to misinterpretation; it is, that a total excision of the upper jaw is not always required for tumour. This will at once be granted; certainly it is not needed, if only we can cut wide of the disease, and remove it entire by incision through healthy parts. But Sir W. Fergusson goes further:—"My view is, that instead of attacking disease of the upper jaw at its circumference, as has been the almost invariable practice since Gensoul's proposal to excise the whole bone, it is better to get into the disease, as it were, and cut from the centre to the circumference, making sure that in doing so that circumference shall be thoroughly encompassed." This appears to us a return to the dark age of Surgery, and to the practice of Akoluthus of Breslau, except that, instead of alternating "the cautery and cutting instruments," as the practitioner of the seventeenth century did, Sir William, in the nineteenth, recommends forceps and gouge.

The last chapter, on amputation, is partly historical. The author concludes by giving in his adhesion to the prevailing fashion of the present day, which we owe so much to Teale of Leeds, and Carden of Worcester, and in which we avoid the presence of a cicatrix over the bone, by the use of a long anterior and a short posterior flap.

In concluding this brief analysis, we may advert to the interest these lectures present as a sketch of contemporaneous Surgery, by one who has taken his place among the most eminent surgeons of the period whose history and progress he records. They are not stiff or formal prelections; but the natural recollections and reflections of one who was a witness and participator in the events which he describes. On this side of the Tweed, it affords pleasure to find that the book is full of affectionate remembrance of his old school, which could not have been shown more gracefully than by the dedication of the work to his old friend of forty years' standing, Dr John Burt, late President of the Royal College of Physicians of Edinburgh.

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[1] Surgical Observations. Boston, 1867. Pp. 139, 140.

A Practical Treatise upon Eczema, including its Lichenous and Impetiginous Forms. By Dr M'CALL ANDERSON, Lecturer on Practice of Medicine in Anderson's University, Physician to the Dispensary for Skin Diseases, etc., Glasgow. 2d Edition. London: John Churchill and Sons.

WE are glad to see that this excellent work has reached a second edition. The first edition we reviewed some years ago; and in the new we find that a considerable amount of additional matter, principally of a practical character, has been introduced. The former edition has been well received by the profession; we confidently anticipate for the present one a similar or even increased popularity.

Journal of Cutaneous Medicine and Diseases of the Skin. Edited by ERASMUS WILSON, F.R.S. November 1867.

WE have much pleasure in directing the attention of those of our readers who are interested in the study of this department of medical science to this publication. It is surely fitting that "the country of Willan and Bateman, which has taken the largest share in what may be termed the Reformation of Dermatology," should continue in the van, and not permit the zealous workers of Germany and France entirely to outstrip it, and eclipse the fame that Britain has already won by the brilliant exertions of those fathers of dermatology. And no more certain method of attaining this object could be devised than that of collecting together the researches of the many labourers in the study of cutaneous medicine and preserving them from oblivion. Aware how much might be accomplished towards the perfection of the science and the management of diseases of the skin by the united labours of the many, it occurred to the mind of Mr Wilson, the editor, that this end might be greatly furthered by the publication of a separate Journal for skin diseases.

"We are desirous," says the editor in his introduction to the first number, "that every step made towards the progress and perfection of cutaneous medicine, should find its record in our pages, and that the volumes of our journal should become the archives of every matter of interest appertaining to the cutaneous system; and we trust that, by pursuing this object conscientiously and honestly, by representing the combined information of a number of co-labourers and observers, we may accomplish much towards simplifying the complexity of cutaneous diseases, and subject many, at present regarded as inveterate and indomitable, to a certain and successful principle of treatment." Such are the aims and purposes of this Journal; and we heartily wish it that success which it bids fair to achieve when it numbers among its contributors such distinguished men as Hebra, Wilson, M'Call Anderson, Tilbury Fox, Hutchinson, etc. This is already the third number of the Journal, which appears quarterly.

Principles of Chemistry founded on Modern Theories. By M. A. NAQUET. Translated from the second edition by WILLIAM CORTIS. Revised by Dr STEVENSON. Renshaw: London: 1868.

THIS is a treatise on chemistry from a theoretical point of view, and is obviously intended for advanced students. It is full and clear and well adapted to the class of readers to which it is addressed, and might be studied with great advantage by all who wish to understand the remarkable development of chemical ideas which has taken place within the last ten years. It is carefully and intelligently, though somewhat too literally translated. As an instance of the curious effect produced here and there by too close an adherence to the letter of the original, we may mention that the Master of the Mint is referred to as "an English chemist, Mr Graham." Such an explanation may be required in France, but is certainly out of place in an English version. We observe occasionally the barbarism "substituted by" instead of "replaced by," and biatomic appears throughout instead of dyatomic.

It is to be regretted that more complete references have not been made to original papers. A student who is able to read the book before us ought to be encouraged to go to the sources of information, and draw for himself. These are, however, only slight blemishes, and we can cordially recommend the book to all who are able and willing to read so large and so thorough a treatise on theoretical chemistry.

A Treatise on Natural Philosophy. By Sir WILLIAM THOMSON and Professor P. G. TAIT. Vol. I. Oxford: Clarendon Press: 1867.

Elements of Natural Philosophy. By CHARLES BROOKE, M.B. New Edition. London: Churchill and Sons: 1867.

THOMSON and TAIT'S Natural Philosophy is a model of what a scientific treatise ought to be. It is characterized by a rigidly consistent use of carefully defined terms, and by a straightforwardness of method which often leads into difficulties, and generally through them, but never evades them. The reader will find the book difficult, because the subject is so; it would be no praise of a treatise on any branch of science to say that it could be read without constant, close attention. We are glad to see that the authors are preparing a smaller volume containing the results and geometrical illustrations (many of which are very beautiful and original), without the analytical details.

Mr Brooke's work is so well known to our readers that we content ourselves with announcing the publication of a new edition.

Part Third.

MEETINGS OF SOCIETIES.

PROCEEDINGS OF THE EDINBURGH OBSTETRICAL SOCIETY.

SESSION XXVII.—MEETING I.

13*th November* 1867.—Sir JAMES Y. SIMPSON, Bart., *President*, in the Chair.

I. CASE OF HYDATIGENOUS DEGENERATION OF THE OVUM. BY DR R. LORD OF CREWE. COMMUNICATED BY DR STEPHENSON.

H. P., æt. 29, was married on the 25th of December 1866. She was a big strong, healthy woman, and had always enjoyed excellent health. She menstruated naturally twice after her marriage, and dated her pregnancy from the 1st of March 1867, at which time she became sick and vomited everything she took. The sickness continued in a severe form almost incessantly for two months. During the night, and while asleep in bed on the 1st of May (two months from the supposed date of the conception), she was awoke by a flooding, which she told me was great, but unaccompanied by pain. This hæmorrhage continued, with intervals of an hour or two, for a month before she consulted a neighbouring practitioner, who prescribed for her, and told her to take rest, or she would miscarry. On the 18th of June I was sent for, and found her very anæmic, weak, and ghastly-looking. She could only walk across the floor by laying hold of things. On making digital examination, I found the vagina full of clots, the os dilated about the size of a shilling, but nothing could be felt in the uterus. My impression was that she was threatened with abortion, and I told her so. I continued to attend her for a month, the treatment being directed to the arrest of the hæmorrhage, and if possible to save the child. Opium, sulphuric acid, and ergot were variously tried. She was better and worse at times, being free of hæmorrhage for a day, and then worse again. She generally spent a few hours per diem on the sofa reclining, but did most of her own house-work and even washing. During my attendance she stated that she distinctly felt the movement of the child, but I regret I cannot ascertain the exact date. During the month of August she went to Liverpool, a distance of 45 miles. She went about and enjoyed herself very much that day and the day after; then sickness returned, which had been absent for two months. At this time she could not lie on one side for the child hurting her with kicking. She frequently spoke to her husband and others about the child being such a rough one, and kicking so much. On one occasion, when going along the street, she stopped suddenly, as if struck, and leaned on some iron railing close to. Her husband asked her what was the matter, when she answered, "It is the little fellow kicking."

A short time after this, she came under the care of my brother, Dr John Lord. He was called to her in the night, or early morning, of September 19, on account of severe flooding and fainting. He prescribed for her, with the effect of relieving the general symptoms, but not the hæmorrhage. This was repeated the following night with a similar effect.

On Saturday morning, September 21, about five o'clock, he was sent for, she being, as she stated, in labour. He went, and found that she had regular labour pains, which were rather strong, and there was as usual flooding. In reply to questions, she said that she had had pains for some hours, but that they got very much worse. She said afterwards that she was sure she most distinctly felt the child moving and kicking. During the next pain, my brother made an examination, per vaginam, and felt the os dilated about the size of a two-shilling piece, and inside the uterus a fleshy mass of what he believed to be placenta. His diagnosis was soon made, and his mind made up that he had to deal with a case of placenta prævia.

On reflection, he decided to pass his hand through the placenta and extract the child. He did so, but to his utter chagrin and surprise, no child could be found. After waiting a time, he made another search for the child, but with no better success. This latter time, however, he brought away a portion of the placenta, which, on inspection, presented the curious appearance of a fleshy mass studded with innumerable vesicles, or little bladders, somewhat resembling small grapes, and which, when squeezed, exuded a clear limpid fluid. He was baffled; so he sent for me to consult with him. I went. This would be about 10 A.M. He told me all the foregoing, and remarked that he was puzzled to know what had become of the fœtus. Two explanations offered themselves to his mind, viz. :—

1. That the uterus had ruptured, and the fœtus become extruded amongst the intestines.

2. That there was no child. But as no case had ever been witnessed by him of this description, this was said half-jestingly.

We decided that my brother should administer chloroform, and that I should make a thorough exploration of the uterus. This was done accordingly. On passing my hand into the uterus, I could feel nothing but a fleshy mass of some description completely filling the uterus. Going further, I passed my fingers gropingly along the fundus, until I came to the left superior apex of the uterus, when I found an opening which I believed was the ruptured part through which the child had escaped, as surmised above. But on further exploration, I ascertained that it was a sort of patent corner or cul-de-sac, and filled with a portion of the fleshy mass similar to that contained in the uterus. That portion found in the corner is the specimen we forwarded to Edinburgh.

While she still remained under chloroform, I emptied the uterus of all its contents, which, although we did not weigh, was, as near as I could guess, a wash-hand basinful of a fleshy mass studded all over with little bladders similar to the specimen sent; but there was no child. We immediately came to the conclusion that it was a case of degeneration of the ovum. We then treated the case *secundum artem*. She soon began to show signs of inflammation of the uterus, also peritonitis and nervous irritation, the latter being evidenced by an extraordinary amount of palpitation of the heart, until the very bed shook under her, although up to this time she had not felt anything wrong about the cardiac organ.

Opiate fomentations and a calomel powder succeeded in greatly relieving the pain over the pubis; but the palpitation continued to such a degree as to prevent all possibility of sleep. Mustard cataplasms were applied externally, and opium internally.

On the third day after confinement, she showed marked signs of "sinking"

—viz., a pulse at 160 per minute, very weak, and easily compressible, and the "facies Hippocratica." She did'nt complain of any pain, only of sinking and sleeplessness from palpitation of the heart. She died calmly and quietly on the fifth day after confinement.

Sir James Simpson questioned the propriety of giving ergot at first in such cases, as that drug was not generally exhibited unless premature labour was to be brought on.

Dr Peter Young had had several cases of hydatigenous degeneration where there was no fœtus.

Sir James Simpson said the fœtus might be small, but almost always was found present. One patient went on for thirteen months. The arrestment in the growth of the child generally takes place at six or eight weeks, and it may become broken up. Madame Boivin has given some cases of this kind. The uterus, as in this case, is often found irregular in shape.

Dr Burn said one patient of his went to the full time; died suddenly from hæmorrhage; and on examination, hydatigenous degeneration of the ovum was found.

Dr Keiller had several similar cases—one like Dr Lord's. In one case he had been able to diagnose its nature from discovering some of the vesicles in the discharge. Another point of importance in the diagnosis was, that the discharge was not always pure blood, but a watery fluid, derived no doubt from the rupture of some of the vesicles.

Dr Bryce had one case at the full time, when there was no hæmorrhage—one mass of hydatids was the result; and the woman had another child in 18 months.

Dr Cappie had one case where there was no hæmorrhage, with recovery, but the patient had no more children. In a second case, there were hydatids, with no hæmorrhage.

Sir James Simpson suggested, as the treatment for such cases, when their nature has been determined, ergot of rye to induce labour, and the use of the plug.

II. RETAINED PLACENTA. BY DR BELL.

This paper will appear in the next number of the Journal.

Dr Sidey asked whether there was no such thing as hour-glass contraction?

Dr Bell answered he did not believe it; in which opinion Dr Pattison and others agreed.

Drs Cappie, Stevenson, Cairns, and *P. Young* had had each *one* case of hour-glass contraction.

Dr Keiller said that retained and adherent placentæ might be confounded, and what might have been thought hour-glass contraction might be merely the contraction of the whole uterus round the os.

Drs Bryce and *Bruce* coincided with the opinion of Dr Keiller.

MEETING II.

27th November 1867.—Dr CHARLES BELL, *Vice-President*, in the Chair.

Dr R. Lord of Crewe was admitted as a Corresponding Fellow, and Dr Aitken, 52 Queen Street, as an Ordinary Fellow of the Society.

I. UMBILICAL HÆMORRHAGE AND TUMOUR. BY DR RITCHIE.

This paper will appear in a future number.

Dr Keiller said that cases of hæmorrhage from the umbilicus were generally associated with a diseased condition of the children, which showed itself in feebleness of the circulation, and jaundice. Post-mortem examination usually revealed some congenital deficiency or obstruction of the hepatic ducts. In one case which had come under his own observation some time ago, obstruction was found to exist. In other cases, again, the gall-bladder was absent. In a case published many years ago, by Dr Alexander Campbell, the child had hæmorrhage from the bowels, as well as from the umbilicus, but there was no deficiency in the ducts. Dr Keiller thought that citric acid might be tried; and he had seen its application to the cheek in a case of cancrum oris followed by some benefit. He would recommend it to be used both internally and locally. In cases of umbilical hæmorrhage, associated with jaundice, etc., no treatment was of much avail; but in cases of bleeding produced by violence, the application of pressure, the perchloride of iron, and other styptics, might be employed, or transfixion of the cord by needles, as had been done in his own case, might be tried.

Dr Pattison asked how the application of sulphurous acid would do?

Dr Andrew Inglis had a patient in whom the hæmorrhagic diathesis was manifested by the frequent occurrence of menorrhagia and a spongy condition of the gums, and two of her children had bleeding from the funis within a month after birth, thus showing the tendency to hereditary transmission.

Dr Thomson, Dalkeith, stated that in the course of his practice, which extended over a period of twenty years, he had not met with any fatal case of umbilical hæmorrhage. In most of the children he had been called to attend for bleeding from the funis, he found that the cause of the hæmorrhage had been that the cord was not properly tied, or there was a fungoid growth present. He concluded, therefore, that fatal cases of hæmorrhage from the umbilicus were rare.

Dr Murray mentioned a case he had seen in the Maternity Hospital, in which the oozing was arrested by means of the perchloride, but melæna subsequently came on, and the child died. An examination of the body revealed enlargement of the liver and obstruction of the hepatic ducts.

Dr Peter Young suggested that in the table of cases drawn up by Dr Ritchie the age ought to be mentioned, as by that means it might be possible to determine whether the disease was congenital or not. One case had occurred in his own practice, in which the hæmorrhage came on eight days after birth; and it was found after death that the bile ducts were obstructed. In cases in which the bleeding was due to hepatic disease, the hæmorrhage, he thought, would likely occur a short time after birth, while bleeding resulting from other causes would come on later. In the case alluded to there was considerable loss of blood, both arterial and venous, which would not coagulate. The perchloride and pressure were had recourse to, but as the patient only survived six hours, no internal remedies were used.

Dr Fraser thought that the phosphate of soda given to the mother might have a good effect upon the child in such cases, along with citric acid.

Part Fourth.

PERISCOPE.

MATERIA MEDICA AND THERAPEUTICS.

By Thomas R. Fraser, M.D.

THE "VAPORARIUM" AS A MEANS OF TREATING PHTHISIS PULMONALIS.

Dr Felix Bricheteau strongly recommends to the attention of physicians the application of the "vaporarium" in the treatment of phthisis pulmonalis. The honour of first proposing and testing this method appears to be due to Professor Trousseau, whose recent death we all unite in deploring. Drs Galliet and Henri Henrot have since employed it in several cases of phthisis, as well as in various chest and throat affections, and from their experience the greater portion of the evidence in its favour is derived. It is scarcely necessary to explain that the "vaporarium" is a chamber filled with the vapour of water, in which the patient constantly resides for a more or less extended period. To obtain a warm and moist atmosphere, two plans are recommended. The first is applicable to hospitals, and to such establishments as possess the convenience of steam-pipes. To one of these a tube is attached, that leads to a chamber in which is placed a trough of considerable depth, and with a large exposed surface. This tube is made to enter the bottom of the trough, and the steam thus passes through the contained water, and causes the required evaporation.

The second method may be used in the absence of steam-pipes. It consists essentially of a small boiler, heated by a gas or other lamp, and having a tube to conduct steam into the lower part of a trough containing water, from the surface of which a rapid evaporation of warm vapour is thus caused.

For a room of ordinary size, a trough one yard and a half long, and about ten inches deep, is sufficient; and with this a temperature of between 74° and 80° Fahr. may be readily maintained.

As yet the experience with the "vaporarium" has not been extensive. Three cases of confirmed phthisis have been perfectly cured; two inveterate (opiniâtre) cases of the same disease have been relieved; one case of croup was rapidly cured; and one of œdemia glottidis, which had resisted all other means, was cured in a few hours. Details are given of several of these cases, and from the description of the physical signs, there can be no doubt as to the genuineness of the phthisical affection.

In a few instances, patients derived no benefit from their abode in the "vaporarium." Among these were sufferers from very advanced phthisis, and the greater number of those treated for croup.

Dr Bricheteau, with apparent justice, ascribes the value of the "vaporarium" to the convenience that it affords of imitating a warm and moist climate. It places at our disposal a constant temperature and an atmosphere loaded with moisture; and a room may easily be constructed in large dwelling-houses or in hospitals where such a warm and moist atmosphere as is so advantageous to a large class of patients may be constantly maintained. In addition, the air of the chamber may be impregnated by medicinal substances, such as sulphurous acid, tar, carbolic acid, or iodine, if these be placed in the trough of water through which the steam passes.—*Bull. Gén. de Thérap.*, July 30, 1867, p. 49.

ON THE EMPLOYMENT OF NUX VOMICA IN DYSENTERY AND IN DYSENTERIC PARALYSIS. BY DR DE SAVIGNAC.

In an interesting paper Dr D. de Savignac discusses the value of nux vomica in the treatment of dysentery, and of the general paralysis that frequently accompanies the chronic forms of this disease. He has had ample opportunity to study the disease in L'Hôpital Maritime de Toulon; and he has already advanced the theory that the symptoms of dysentery are due, in the first place, to an affection of the spinal cord, which causes paralysis of the motor nerves of the muscular coat of the large intestines, and of the vaso-motor nerves that supply its blood-vessels. He regards the general paralysis of the chronic form as another effect of the central nerve lesion. In support of this theory, several cases are given and others referred to, in which pathological changes in the spinal cord, of the nature of softening, were found at the autopsies. Nux vomica possesses many therapeutical effects that admirably apply to this view of the causation of dysentery. It stimulates the motor nerves of the intestines, and removes the paralysis of this viscus; it increases the tone of the intestinal blood-vessels, and diminishes or stops the abnormal excretion from the mucous surface; it re-establishes the normal neurility of the constrictor ani muscle, and control over defæcation is gradually reacquired: and from these effects there result cessation of colic and tenesmus, diminution in the frequency of the stools, and a modification in the character of the defæcations. At the same time, the action of the remedy on the spinal cord removes the paralysed condition of the limbs. In such chronic cases of dysentery Dr Savignac prefers the powder of nux vomica (a form not recognised in the British Pharmacopœia) to all other preparations of the remedy. He administers from 3 to 7 grains in the twenty-four hours, and usually combines it with canella powder, whose aromatic and tonic properties are useful in diminishing the intestinal flux.—*Bull. Gén. de Thérap.*, Sept. 15, 1867, p. 193.

APHONIA OF NEARLY TWO YEARS' DURATION CURED BY ELECTRICAL STIMULATION OF THE INFERIOR LARYNGEAL NERVE. BY DR PHILIPPEAUX.

Various methods of treatment had been unsuccessfully tried in a case of aphonia which had originated two years previously, and which was supposed to be due to paralysis of the nerves of the vocal cord. The patient was a healthy female twenty years of age. It was ultimately decided by Dr Philippeaux to try the effect of electrical stimulation, applied in such a way as to directly influence the inferior laryngeal nerves. For this purpose, one metallic pole was inserted into the lower and posterior portion of the pharynx, and the second was placed on the skin over the crico-thyroid muscles. A current of considerable strength was passed between these two points: almost immediately after the closure of this current the patient started, uttered a loud cry, and began to speak with a facility equal to that which she had possessed before the commencement of the aphonia. Dr Philippeaux remarks that he has frequently met with success in treating aphonia by electricity, but never before had he the good fortune to obtain so instantaneous and perfect a cure.—*Révue de Thérap. Médico-Chirurgicale*, No. 16, 1867, p. 430.

OINTMENT FOR PRURIGO

Dr Charvet has found the following a nearly infallible cure for prurigo, after an experience of twenty years:—

Axunge, simple or camphorated, .	60 parts	Mix.
Citrine ointment, ...	3 ,,	

A small quantity of this should be spread in a thin layer over the affected surface, and it gives almost instantaneous relief. The dilution of the citrine ointment in this preparation prevents the inconvenient and permanent staining of linen which is otherwise caused.—*Bull. Gén. de Thérap.*, Aug. 30, 1867, p. 180.

PATHOLOGY.

ON INFLAMMATION AND SUPPURATION. BY DR J. COHNHEIM, ASSISTANT IN THE PATHOLOGICAL INSTITUTE OF BERLIN.

THE author investigated the phenomena of inflammation in the cornea of the frog, and found (1.) that it is not true that the stellate corpuscles enlarge and form pus cells within themselves, either from their nuclei or cell contents; (2.) that corneal opacity depends upon the presence of pus corpuscles within it; (3) that the pus corpuscles are situated between the cornea corpuscles, and that the former alter their form or position, while the latter do not. From these observations he concludes that pus corpuscles must either be derived from the pre-existing movable lymph corpuscle-like bodies in the cornea, or have travelled into it from without. The latter view he deems correct. He found that simple traumatic keratitis always begins at the margin of the cornea and spreads inwards; also, by adopting Recklinghauser's plan of colouring the white corpuscles by the introduction of coloured solutions (Cohnheim used aniline blue) into the lymph sacs, the veins, or the arteries, he satisfied himself that "the pus corpuscles in the inflamed cornea had formerly been white blood corpuscles, and had passed into the cornea from surrounding vessels."

In order to ascertain how these structures escaped from vessels, he experimented on frogs, which he first paralysed by means of curara (a poison which, given in small doses, does not influence the circulation). He selected male frogs, and, by a particular arrangement, placed the mesentery under a microscope in such a way as not to interfere with the circulation. He found (1.) that the arteries become dilated; (2.) that more slowly, but to a greater extent, the veins undergo the same change; (3.) that the capillaries also become widened; (4.) that with these alterations the circulation becomes slower; (5.) that after a time, in the veins a peculiar change occurs. The layer of the blood stream, next the walls of the blood-vessels, instead of presenting the natural clear appearance, becomes filled with white corpuscles, which gradually accumulate, and move more and more slowly, until at length they stop; while the red column continues to move in the centre. But presently another change appears. "On the outer contour of the venous wall there appear small, isolated, colourless, bud-like elevations, as if the wall itself were forming small outgrowths; gradually these enlarge. After a time, the half of a spherical body of the size of a white blood corpuscle appears. This, after a time, becomes pear-shaped, the broad end from the apex pointing towards the vessel. From the rounded end numerous little processes project, and the corpuscles assume various forms. Above all, however, the mass separates more and more from the vessel, and at last we see a colourless, somewhat shining, contractile corpuscle with one long and several short processes;" in fact, a white blood corpuscle. This process goes on·on many parts of the venous wall, which nevertheless remains intact.

When the capillary current is much impeded, a similar change occurs, but the red as well as the white corpuscles pass out.

The slowness of the circulation, and the accumulation of the white corpuscles, depend upon the dilatations of the vessels. How, in what way, and by what power, do the corpuscles pass out? Cohnheim finds that in the interior of the veins. capillaries, and arteries, there are stomata similar to those demonstrated by Rechlinghauser and Oedmanson in the walls of serous cavities, and that these communicate with canalicular spaces. It is by these openings that the corpuscles pass out. As to the power, he believes that it is by the peculiar amœba-like contractility that they escape.

During these processes he finds the rest of the tissue unchanged; the cells covering the peritoneum are unaltered, even when they are covered with a layer of lymph and pus.

These results, obtained by experiments on frogs, he has verified by examination on young rabbits and cats.

He would thus transfer the seat of pus-cell formation from the connective tissue to lymphatic glands.

In the conclusion of his paper he remarks that it will be necessary, in some degree, to modify the prevalent theories of acute inflammation—at least of that form which is accompanied by suppuration. "For this species of inflammation the vessels again come into the foreground. Without vessels no inflammation, dilatation of vessels, injection, and hyperæmia, the necessary first stage. In vascular parts it is their own vessels, in non-vascular, those of surrounding structures, that supply, as in ordinary nutrition, the plasma; in inflammation the pus corpuscles, as a second requisite for the occurrence of suppuration, necessitate the presence of spaces which permit a moving forward, and an accumulation of colourless corpuscles"—and such spaces are formed by the connective tissue.—*Virchow's Archiv*, Sept.

ON CEREBRAL HÆMORRHAGE. BY PROFESSOR BÉHIER OF PARIS.

In general, cerebral hæmorrhage has been referred either—1*st*, to excessive pressure on the arteries of the brain; or, 2*d*, to diminution of the normal elasticity of the arterial walls. Thus, hypertrophy of the heart was long considered one of the principal causes of apoplexy. But when it exists as a compensating lesion, cardiac hypertrophy can have no influence in producing apoplexy; although, if there exist peripheral obstacles to the circulation, from atrophy of the kidneys or diffuse induration of the arteries, the excessive vascular tension may possibly give rise to hæmorrhage. Béhier thinks that atheromatous incrustations of the larger arteries play only a secondary part in the production of cerebral hæmorrhage when no other morbid alterations exist; and that the same is the case in regard to mechanical obstructions, obliteration of vessels, compression of the jugular veins, and cerebral atrophy. He also controverts the opinions of Pariset and Rochoux, who considered softening of the brain as preliminary to hæmorrhage, the vessels rupturing from weakness of their walls (*ramollissement hémorrhagipare*). Cases of apoplexy so caused are excessively rare. The granular (fatty) degeneration of the cerebral capillaries, believed to be the principal cause of cerebral hæmorrhage by Robin, Paget, and others, is stated to have been proved by M. Bouchard to be not a primitive, but a consecutive lesion, frequently the consequence of softening; and, in cases of cerebral hæmorrhage, instead of preceding, results from the morbid process which follows the extravasation of blood. After rejecting all these presumed causes of sanguineous apoplexy, Professor Béhier states that a new cause of cerebral hæmorrhage has been discovered by Drs Charcot and Bouchard. The alteration consists in small *aneurisms* of the capillary arteries of the brain, far different from those which occupy the larger branches, and which have been accurately described by Dr Gougenheim. The aneurisms alluded to are situated on vessels of a minute size. They consist in small ampullary dilatations, sometimes laterally developed, but generally fusiform. They are visible to the naked eye under the shape of dark red granulations, about the size of a pin's head, disseminated over the surface and in the interior of the brain. In the very centre of the clot one or more of these little aneurisms are found to exist, and their cavity communicates freely with the extravasation by an aperture, around which float the broken fragments of the external membrane. This opening resembles in every respect the ordinary ruptures, which allow an aneurismal sac to pour its contents into the neighbouring cavities. Dr Bouchard has observed that the blood sometimes collects between the external and the middle coat of the artery; at a later period the external coat itself gives way, and hæmorrhage is then produced. The coagula contained in the aneurismal cavity are continuous with the clot which fills the cerebral focus. Since the time when his attention was first called to this subject, M. Béhier has discovered similar aneurisms in all the cases of cerebral hæmorrhage in which he has been able to perform the post-mortem examination. This alteration, therefore, may be viewed as one of the principal causes of apoplexy. He does not pretend to say that it exists in every case, but he fully believes that, on proper investigation, it will be discovered in the great majority of cases.

PROFESSOR BÉHIER ON RIGIDITY OF PARALYSED LIMBS IN CASES OF CEREBRAL HÆMORRHAGE.

In certain cases of hemplegia, the primary symptoms of the disease—viz., loss of motion and sensation—far from disappearing altogether, or even remaining stationary, are not unfrequently aggravated by the supervention of an entirely different class of phenomena. Violent pains and muscular rigidity invade the palsied limbs. How is this singular change to be explained? Formerly, it would have been supposed that inflammation had arisen in the immediate vicinity of the clot, or that the encephalic membranes were affected. But the clinical observer finds neither inflammation nor softening of the brain present; there is no fever, no acceleration of the pulse, no loss of appetite, vomiting, or coma. The brain, therefore, has nothing to do with the pains and rigidity. Are we to suppose that the spinal cord is affected? In old cases of cerebral hæmorrhage there undoubtedly exist alterations of the medulla spinalis, consisting chiefly in a kind of descending atrophy. But in such cases there is inertia and flaccidity of the palsied limbs, not intermittent pains and spasmodic contractions.

"Dr Charcot and his pupil, Dr Cornil, by the results of their investigations, laid down in a paper read before the Biological Society in 1863, have thoroughly elucidated the difficulty. The muscular rigidity of hemiplegia affects various forms. In certain cases, the limbs are entirely contracted; the arm is pressed against the wall of the chest; the fingers are bent inwards, the nails leaving their mark in the palm of the hand; and the leg is half bent; while in others the limbs remain flaccid, the fingers and toes being alone contracted.

"We have now to inquire what the anatomical alteration is which corresponds to this series of phenomena. Both muscles and nerves have been carefully examined. The muscular tissue is partially atrophied. It is of a dirty yellow or brown colour; it has become soft, and its natural elasticity has greatly diminished. There exists, therefore, an important change in its nutrition; but this is the consequence, not the cause of the disease. But the state of the nerves has principally attracted the attention of Drs Charcot and Cornil. Their size has visibly increased; they are often twice as large as the corresponding nervous trunks on the opposite side. At the same time, their toughness and density are much greater than usual. Their colour is reddish, and their surface exhibits a fine network of capillary vessels. Lastly, when the neurilemma is examined, it is found to have grown very thick, and to adhere strongly to the subjacent nervous fibres. The partitions which separate the bundles of nervous filaments are equally grown very thick, and the nuclei of the sarcolemma have considerably increased in number. On the other hand, the bundles of nervous filaments have grown thinner than usual; but although disseminated in a mass of connective tissue, which compresses them, they retain their normal characteristics, and remain free from granular degeneration. It is therefore evident that, in cases of painful muscular rigidity supervening *after* the restoration of sensibility in the paralysed limbs, the nerves are swollen, if we may use the expression, by the undue thickening of their sheaths, while the nervous tubes remain perfectly sound, and therefore capable of receiving sensorial impressions and of exciting motion. If in the circle of morbid anatomy we look for a parallel, we shall find it in cirrhosis of the liver. The progressive hypertrophy of the connective tissue compresses and separates the nervous tubes in the first case, while it dissociates and strangles the glandular elements in the second. No doubt these alterations are carried to a higher degree in the liver than in the nervous trunks; but we must bear in mind that the causes which lead to hepatic disease—*e.g.*, chronic intoxication—are generally constant and uninterrupted in their action, while in hemiplegia, whether connected with hæmorrhage or with softening of the brain, the blow, once struck, is not immediately renewed, and the consecutive alteration of the peripheral nerves remains circumscribed within much narrower limits.

"Such are the facts of the case. It now remains to inquire how the sensation of pain is produced at a moment when the brain does not seem to be capable of perceiving sensorial impressions. Without entering here into a physiological discussion, it is sufficient to remind you that in general, at that stage of the disease, sensibility has been restored to the affected parts, although the power of motion still remains abolished. It is therefore easy to understand that pain may be felt, though motion is impossible. As to the muscular rigidity, it is no doubt principally connected with the local pressure exerted upon the nervous filaments, which, still retaining their vital power, are excited by the application of this permanent stimulus.

"In a preparation which exhibits the alterations just described, a transverse section of a sound nerve is placed by the side of a similar preparation in a diseased one, taken from a subject affected with hemiplegia. Both have been treated by a solution of carmine in ammonia. A deep red colour shows the place occupied by the nervous tubes, while the connective tissue which separates them remains white. It is seen that in the second nerve the bundles of filaments are merely separated by thin white streaks, while in the diseased one the neurilemma is exceedingly thick, as well as the internal partitions which separate the fascicles from each other; and yet the nervous tubes, placed under the microscope, are found to be in a perfectly healthy state. The alteration therefore consists in an undue hypertrophy of the connective tissue, and may be viewed as *cirrhosis of the nerves*."—Abridged from *Med. Times and Gaz.*, 1867.

SURGERY.

CAROTID ANEURISMS.

The following is a brief *resumé* of a clinical lecture by M. Vanzetti of Padua on this interesting subject, on which he is already known as a high authority; it is too long for translation entire :—

"The annals of surgery do contain certain cases in which spontaneous cures, or cures by the treatment of Valsalva, have been obtained in carotid aneurisms. I have lately seen an aneurism of the right carotid cured in this manner. A patient lately consulted me on account of swelling of his legs. On asking him about the previous state of his health, he mentioned, among other details, that, at the age of thirty-three, he had an aneurism of the neck, which had been cured without an operation. His own account of it was as follows :—He was a sausage-maker; one day, when lifting a pig from the waggon on his back, he made a great effort with his head to throw it backwards, and in a day or two another similar effort to throw off a large block of wood on which he chopped his sausages. To these efforts he attributed a pain soon after noticed in the neck, and at the spot of pain a tumour as large as a filbert. He consulted a doctor, who was also one of his patrons, and who alarmed him much by telling him that he feared he would chop no more sausages for him. He then consulted Dr Malago, professor of surgery, who told him, after consultation with many other professors, and in the presence of the pupils, that it was an aneurism of the carotid, and that an operation would be necessary. By this time the tumour pulsated strongly, was painful, and as large as a hen's egg. He refused to submit to an operation, consulted other surgeons in Verona and elsewhere, who all confirmed the diagnosis. He then returned home to the Tyrol, and put himself under the care of Dr Solizzoli, who confined him entirely to bed for eleven months, and allowed him to eat only a small loaf and a little soup each day, and that at three meals. He bore the torments of hunger with resignation, was reduced to skin and bone, but, at the end of nine months, the tumour ceased to pulsate, gradually diminished in size, and at length completely disappeared."

From the history it is almost absolutely certain that he had a carotid aneurism, and that, by Valsalva's method, it had been cured. No vestige of the tumour remained; both carotids pulsated, the right being perhaps the strongest.

Such cases of cure of carotid aneurisms, by Valsalva's method, are so excessively rare that we cannot rely on such treatment. Digital compression, however, has succeeded in three cases: one by Gioppi of Padua, in an aneurism of the ophthalmic artery; another by Vanzetti at Verona, in a similar case; and a third, recorded by Dr Sheppard of Worcester, in which an aneurism as large as a hen's egg, involving the bifurcation of the right carotid, was cured by intermitting digital compression, performed by the patient himself.

The following case is an example of an aneurism situated at the bifurcation of the right carotid, of only three months' standing, which, having become rapidly diffuse, had already filled up almost the whole side of the neck, rendering digital compression impossible, and obliging us to perform ligature of the vessel close to its origin.

J. G., æt. 60, peasant, three months before admission, felt a pain in the right side of the neck, and, placing his hand on the spot, discovered a pulsating tumour. The size of the tumour, and the pain in it, increased in equal proportion. He felt pain in the ear, as if caused by *blows from a hammer.* He became hoarse, and felt an irritation in his pharynx, which made him cough perpetually. On admission, the side of his neck projected in a large tumour, which had a visible pulsation, and a distinct arterial bruit. Its limits were ill defined, extending transversely from the spinous processes of the vertebræ to the larynx, which it forced across the middle line, and vertically from the lower border of the jaw and the external ear down to about a finger's-breadth above the clavicle. Tumour was hot, and skin above it slightly red. Pulse 80. Nothing to be noticed in the heart or great vessels. Intense headache, constant drowsiness, with the eyes half-open. Right pupil more contracted than the left. Intellect unaffected.

Next day patient was delirious, endeavoured to rise; the tumour is enlarged, hotter, redder, and at its most prominent part glistens.

In such a condition, as from want of room digital compression was out of the question, ligature of the carotid artery was determined on. There was little room for this, as the sterno-mastoid was displaced, its sternal extremity being stretched and projected as a tight cord. Two incisions were made, one along the inner edge of the sterno-mastoid past its sternal insertion, another cutting through both skin and platysma along the upper border of the clavicle, thus marking out a triangular flap. The flap being raised, the sternal insertion of the sterno-mastoid was then divided, and the muscle also thrown up. The cellular tissue was exposed and cautiously divided, till a large vein (probably the anterior jugular) was exposed. This was secured with a double ligature, and divided. The sterno-hyoid and thyroid muscles were also exposed and divided. The deep fascia of the neck was then shown, and was carefully detached till the vessel was exposed; a small portion of it was carefully cleaned, and the needle passed from within outwards, the internal jugular being there pretty safe, having left the artery on the right side. The thread was tied tightly. The internal and middle coats were felt to give way. No cerebral phenomena occurred. The patient, who was not anæsthetized, bore the operation well.

The tumour diminished in size, but the patient gradually became more delirious, passed into a state of profound coma, and died at the close of the seventh day after the operation. No hæmorrhage or any trouble from the wound. At the post-mortem examination considerable vascularity and softening of the cerebral substance was found. The ligature, which was only an inch above the origin of the vessel, was ready to separate; and excellent clots sealed up both ends. The aneurism had begun to consolidate.—*Gazette des Hôpitaux*, No. 128.

M. Vanzetti also made a communication to the Imperial Society of Surgery of Paris, in which, after thanking the Society for the honour they had conferred upon him by giving him the title of corresponding member of the Society, to fill the place of Valentine Mott, he reported the following cases:—1. A case of traumatic aneurism of the superficial palmar arch, cured by digital compression of the brachial artery in thirty minutes—by Dr Vecelli, of Treviso. 2. An aneurism of the femoral artery, cured by digital compression at the pubis, continued for ten hours—by Dr Vigna, of Venice. 3. An arterio-venous aneurism at the bend of the arm, cured by pressure on the brachial artery and basilic vein—by Dr Franzolini, of Sacele.—*Gazette des Hôpitaux*, No. 127.

ON THE DANGERS RESULTING FROM THE PRESENCE OF FOREIGN BODIES, SEQUESTRA, FRAGMENTS OF CLOTHING, AND THE LIKE, IN GUNSHOT WOUNDS.

THE above is the title of a very interesting paper by M. Cabasse of the Military Hospital of Arras. It is too long for quotation, but is well worth perusal, and contains one or two interesting cases. The author insists very strongly on the importance of the early and thorough examination of the wound, and removal of all foreign bodies, if we wish a rapid and permanent cure. He reports one foreign body, which he believes to be unique. A wound in the foot was kept open till the extraction of a round piece of the hard, horny skin of the sole of the foot, which the ball had driven in, allowed it to close. Another patient had profuse suppuration and extensive redness and swelling of the neck and face after a wound of the neck by a musket-ball, which, however, had escaped by an aperture of exit. A careful examination found about fifty bristles in the wound, which had been driven in before the ball out of the patient's stiff military stock, and which, from their elasticity, had remained doubled up in the wound, and created irritation.—*Gazette des Hopitaux*, 1867, No. 117.

REPORT ON OPHTHALMOLOGY.

By Dr Argyll Robertson.

ON AFFECTIONS OF NUTRITION OCCURRING IN THE EYES OF RABBITS AFTER SECTION OF THE TRIGEMINUS. BY M. MEISSNER AND BY M. SCHIFF.

Schiff and Meissner combat the view maintained by Sneller and Büttner, that ulceration of cornea, and inflammation of the eye, following division of the 5th pair of nerves, depends upon loss of sensibility. Meissner, in attempting to cut within the cranium of a rabbit, the trunk formed by the two superior branches of the 5th, cut only, as was proved by post-mortem examination, the inner part of the trunk. Sensibility was not affected, but the cornea ulcerated, and the eye inflamed. He also refers to an experiment by Büttner, in which sensibility was lost, but the eye was not inflamed, and in which the inner part of the trunk was found intact, while the outer had been cut. Schiff relates four experiments on cats and rabbits, in which the nutritive changes resulted from incomplete section of the 5th, or even of the Gasserian ganglion. One experiment corresponded to that of Meissner recorded above. Thus they refer the affections of nutrition to section of the internal part of the common trunk of the two first branches of the trigeminus. Schiff refers to a former observation in the case of a man whose sensibility was unimpaired, but whose eye was inflamed, and cornea ulcerated, and in whom a partial alteration of the trigeminus was found on post-mortem examination.—*Annales d'Oculistique*, tome lviii., p. 172 (extracted from *Zeitschr. für Ration. Med.*, vol. xix. 1867, and *Gaz. Hebd.* 1867).

DR VON HIPPEL ON DEFECTIVE NUTRITION OF THE EYE FROM ANÆSTHESIA OF THE FIFTH PAIR OF NERVES.

The views of Schiff and Meissner, above narrated, are to a considerable extent confirmed by three cases occurring in the practice of Professor Jacobson, recorded by Dr von Hippel.

The first case was that of a woman aged 36, very nervous and very hysterical, who had been subject to frequent attacks of hæmorrhage, severe headaches—during which she sometimes became unconscious—and other nervous symptoms prior to the occurrence of any affection of the trigeminus. On one occasion, after driving in a sledge and being exposed to a dazzling reflection from the snow, she had a sudden attack of almost complete amaurosis, which, however, disappeared in the course of a few months.

In 1864 she suddenly remarked, while reading, a twitching of the lids and eyeballs, and the appearance of a black cloud before the eyes, especially the right one. She applied for advice in June 1865, and remained under observation ten months.

On admission, there was evident anæsthesia of the sensitive nerves of the left side, which, during the ten months, gradually spread to the right side, so that on the 30th of April 1866, the whole of the left side of the body was destitute of sensibility, with the exception of the adductor region of the thigh, and some parts of the sole of the foot, where the deep insertion of needles was indistinctly felt. On the right side, the forehead, temples, gums, and cheek were quite anæsthetic, while the sensibility of the mucous membrane of the nose, the ear and auditory passage, the conjunctiva and cornea, was diminished; otherwise the condition of the right side was normal. The following were the symptoms attending the development of the partial anæsthesia of the trigeminus:—The patient was first affected with excessively severe pain in the course of the different branches of the nerve, followed in the course of a few days by spontaneous bleeding under the conjunctiva or ear with total anæsthesia of the painful branches. Eventually the sensibility slowly returned. This course of symptoms recurred eight or ten times, the sensibility of the nerves diminishing after each attack. The temperature of the body was found to be lower on the left than on the right side. Sensibility for temperature on both sides normal. The senses of smell and taste were unaffected. After each bleeding from the ears, a degree of deafness was observed, which soon disappeared.

As regards the eyes. Sometimes after an attack of pain in the course of the trigeminus, a slight cloudiness of the cornea, with abrasion of the epithelium was observed, accompanied by diminution of the anterior chamber, contraction and irregularity of pupil, great injection of the conjunctival and sub-conjunctival vessels, and diminished tension of the eyeball. Occasionally, when effusion of blood occurred below the conjunctiva, there was excessive photophobia, giving rise to spasm of the orbicularis to such a degree as to cause inversion of the lids. Notwithstanding the continued use of a compress, applied to the lids by means of collodion and sticking plaster, the same process occurred. The attacks appeared only at intervals, and between them the eyes returned to their normal state, excepting the anæsthesia. If, during an attack, the eyes were left uncovered, the affection of the corneal epithelium increased, but under the use of atropine, and a pressure-bandage, subsided without proceeding to ulceration. On examination with ophthalmoscope, the interior of the eye presented a normal appearance. Vision was much impaired (right eye $S = \frac{1}{33}$, left eye $S = \frac{1}{18}$). The field of vision, which was normal on admission, was found to be very circumscribed on the 30th April.

The second case occurred in a young woman, 21 years of age, in January 1865, followed by unconsciousness for three days, on recovering from which, she found she was completely blind. The amaurosis entirely disappeared in the course of eight days. From February to October 1865 she had attacks of very severe headache, recurring every eight to fourteen days. On every occasion, there was either complete abolition or great diminution of sight, but within twenty-four hours sight returned as clear as before. From October to December she had no headaches, but the affections of sight occurred. In January the headaches returned with increased severity, accompanied by pain in the left eye, great injection of the conjunctiva, and diminished secretion of tears. After this condition had persisted three days, sight in this eye was lost and never returned. In the right eye, the sight became gradually impaired. On examination, it was found that on the left side of the face sensibility was almost lost; on the right side somewhat diminished. Hearing and smell were both impaired on the affected side, and there was no sense of taste on the left side of the tongue. There was prominence and complete anæsthesia of the *left* eye, the pupil dilated, sluggish, and complete amaurosis. The sensibility of the *right* eye was diminished, the pupil slightly dilated, and vision very defective ($S = \frac{1}{50}$). The field of vision was excessively contracted. No pathological change was visible by means of the ophthalmoscope.

When the headaches disappeared, sight in the right eye improved, and thus the visual power varied greatly (from $S = \frac{1}{100}$ to $\frac{1}{6}$) as did also the field of vision.

The third case was also that of a woman. She was 48 years of age, and had enjoyed good health till May 1865, when, in consequence of a fright, severe pain suddenly occurred in the course of the whole of the left trigeminus, accompanied by partial anæsthesia of the left cheek, which gradually in a year spread, while the neuralgia recurred with greater intensity, often accompanied by giddiness. In April 1866, complete paralysis of the left abducens, motor oculi, and auditory nerves occurred, but in a month the function of these nerves had returned to a considerable extent, although the movements of the left eye remained limited and the hearing impaired. About the middle of July, a great discharge of pus, and occasionally of blood, occurred from left eye, accompanied by cloudiness of the cornea and loss of sight. On examination on the 28th of July, the teeth, gums, jaws, tongue, and nostril, on the left side and left eyeball, were found completely anæsthetic, while the sensibility of the forehead, scalp, cheek, and chin was much impaired. Taste, sight, and smell on the left side were lost, and hearing much dulled. The conjunctiva of the left eye was much injected, and a zone of deeper vessels was observed round the lower circumference of the cornea. A flat ulcer was seen in the lower third of the cornea, spreading from which there was an infiltration of whitish-yellow matter affecting a fourth of the cornea. The aqueous humour was muddy, and pus existed in the anterior chamber. Notwithstanding the application of a pressure-bandage and the instillation of atropine, the ulcer deepened, and perforation occurred in three days. Ten days afterwards the ulcer of the cornea was healing, and a leucoma, with adherent iris, was being formed.

The symptoms in the third case correspond most exactly with those occurring after division of the 5th in the lower animals—viz., opacity, ulceration, and perforation of the cornea, injection of the conjunctiva, with discharge of a whitish purulent secretion and turbidity of the aqueous humour. In one symptom this case differed from the results of division of the nerve—the pupil was dilated, while in the latter cases it was usually contracted. The dilatation of the pupil is readily accounted for by the paralysis of the motor oculi which co-existed. Von Graefe's view, that the corneal affection depends on the exposure of the eye to external influences, thus inducing an abnormal dryness, receives some confirmation from the fact that the ulceration of the cornea (which, when induced by defective nutrition, usually occurs in the centre) was seated in the lower part. The upper portions of cornea were in this case constantly covered from external influences by the upper lid, which the patient was unable to elevate from paralysis of the motor oculi.

The second case is an illustration of the occurrence of anæsthesia of the 5th, with scarcely any of the ordinary nutritive changes in the eye.

The first case demonstrates that the corneal affection may occur although the eye be kept constantly covered; but that such a covering mitigates the affection. The sudden accessions and recessions of the symptoms are a striking peculiarity of this case.

A similar case was related by Mr Dixon in the *Med.-Chir. Trans. for* 1845. —*Archiv für Ophth.*, band xiii. abth. 1.

ROGOW ON THE ACTION OF THE EXTRACT OF THE CALABAR BEAN AND OF NICOTINE ON THE IRIS.

Authorities have hitherto ascribed the action of nicotine and the Calabar bean on the pupil to the sympathetic or to the 3d pair of nerves. Rogow directs attention to the 5th pair as an agent in the contraction of the pupil. He cites the experiments of Bernard, Budge, and others, who found, after division of the trigeminus, contraction of the pupil to follow excitation of the peripheral extremity. With the view of ascertaining the mode of action of the Calabar bean and nicotine, he performed a number of experiments on rabbits, in which he introduced these substances by the mucous membranes, injected them under the skin or applied them directly to the eye, and investigated their action under various circumstances—as while the animal was poisoned with curara, or had its pupil dilated with atropine, after excision of the cervical sympathetic, etc. As the result of his experiments, he is of opinion that the action of the Calabar bean is due to contraction of the circular fibres of the iris, dependent on irritation of the peripheral extremities of the motor oculi, and that there is no paralysis of the sympathetic. The action of nicotine he considers less simple, and that different causes lead to pupillary contraction according to the mode of its introduction. When applied directly to the conjunctiva the contraction is due to irritation of the 5th alone, or combined with excitation of the muscular fibres of the sphincter. On the other hand, when nicotine is absorbed through the nasal mucous membrane, or after subcutaneous injection, paralysis of the sympathetic branches may co-exist, but the chief agent in the myosis is the 5th pair of nerves.—*Annales d'Oculistique*, tome lviii., p. 105 (*from the Zeitsch. für Ration. Med.*, b. xxix., h.1, 1867).

REPORT OF 100 CASES OF CATARACT, IN WHICH VON GRAEFE'S NEW EXTRACTION OPERATION WAS PERFORMED. BY PROF. KNAPP OF HEIDELBERG.

The operations were performed exactly as recommended by Prof. von Graefe, except in those cases in which, in the 4th stage, the evacuation of the lens cannot be effected by pressure. To assist the exit of the lens, Knapp had recourse to a broad scoop, as he found that with the hook recommended by von Graefe he could not get a secure hold of the lens. In a few cases he removed the capsule and lens together—a procedure which he recommends in all cases where the zonule of Zinn is fragile or injured, as is frequently the case in over-mature cataracts, and in those in which the lens substance has become atrophied, and in which earthy deposit and thickening of the capsule have occurred, as also where there is a loose hard nucleus in a softened cortical mass.

We give a few extracts from the statistics of his operations.

As regards the performance of the operation no accident occurred	in	71	cases.
Escape of vitreous humour to a slight extent, so as scarcely to affect the process of cure	,,	7	,,
Portions of lens remained behind	,,	2	,,
Blood remained in anterior chamber to a considerable extent without, without having any injurious effect on the result	,,	1	,,
Blood and portions of lens were left in anterior chamber, but in which the eye recovered with good vision . .	,,	1	,,
Loss of vitreous humour occurred, and portions of lens were left in anterior chamber	,,	15	,,
Loss of vitreous humour occurred. and portions of lens and blood were left in anterior chamber	,,	3	,,

The large proportions of cases in which loss of vitreous humour occurred he attributes in the early cases to his want of practice of a new operation, and to the fact that all cases, even the most unfavourable for the operation, are included in the statistics. Particularly unfavourable as regards result he considers those cases in which loss of vitreous *preceded* the evacuation of the lens, as then the use of a traction instrument was required. He lays great stress on the removal of all portions of lens substance, even at the expense of some vitreous humour.

His after treatment resembles that ordinarily employed. In no case, however, did he apply leeches, as he considers that any pain which may occur on the day or evening of operation is best relieved by sub-cutaneous injection of morphia.

The process of resolution was quite normal in 74 of the cases. In those cases in which no accident occurred during the course of the operation, the healing process was normal in 92 per cent.; while of those in which accidents did occur in the course of the operation, normal resolution occurred in only 22 per cent. He divides his cases, as far as the nature of the cataract is concerned, into the ripe (70 cases), unripe (8), over-ripe (9), and complicated (13), and found that accidents occurred in the course of the operation in

65	per cent.	of the	unripe	cataracts.
23	,,	,,	complicated	,,
22	,,	,,	over-ripe	,,
20	,,	,,	ripe	,,

The resulting visual power in these classes of cases he found to be as follows :—

In ripe Cataract—			
Incurable blindness	in	$1\frac{3}{7}$	per cent.
Curable blindness	,,	$4\frac{4}{7}$	,,
Imperfect vision (enabling patients to guide themselves)	,,	11	,,
Good sight (sufficient for reading)	,,	83	,,
In unripe Cataract—			
Incurable blindness	,,	12½	,,
Imperfect sight	,,	25½	,,
Good sight	,,	62	,,
In over-ripe Cataract—			
Imperfect sight	,,	11	,,
Good sight	,,	89	,,
In complicated Cataract—			
Curable blindness	,,	38	,,
Imperfect sight	,,	54	,,
Good sight	,,	8	,,

The general result as regards vision was :—

Incurable blindness	in	2	per cent.
Quantitative perception of light	,,	6	,,
A decided improvement in sight, enabling patient to guide himself	,,	14	,,
Sufficient sight to enable patient to perform ordinary work and to read large-sized print	,,	18	,,
Good sight, fitting patient for any employment, and enabling him to read small print	,,	62	,,

The age of the patient did not appear to exercise any particular influence on the healing process. The average period of recovery after operation was 18½ days.

The chief advantage of this method of operating consists in the only exceptional occurrence of purulent deposit in the cornea. He considers the operation perfect so far as the position and form of the incision is concerned, but thinks that a means of removing the anterior capsule of the lens without injury to other parts is much to be desired, and that the instruments at present employed to assist the evacuation of the lens are imperfect.—*Archiv für Ophth.*, band xiii. abth. 1.

NEW OPERATION FOR OPAQUE STAPHYLOMA CORNEÆ. BY PROFESSOR CASTORANI.

The advantages claimed for this operation are :—1st, Simplicity, facility, and celerity of procedure, with only slight infliction of pain. 2d, That it removes the deformity and restores almost the normal curvature of the cornea ; and 3d, That it, with the aid of an artificial pupil, restores the power of vision to the patient. The operation is performed as follows :—The surgeon makes an incision with a cataract knife at the distance of 3 or 3½ mm. from the outer circumference of the cornea, the edge of the knife being directed forwards towards the summit of the staphyloma. The knife, held horizontally, is pushed on till counter-puncture is effected at a point near the inner circumference of the cornea corresponding to the point of entrance. The incision is then completed, so as to divide the staphyloma into upper and lower halves. A similar vertical section is then made, and thus four flaps are obtained, which are in succession removed with a pair of curved scissors. The lens is evacuated by rupturing the capsule, the eyelids closed, and a bandage applied for twenty-four hours. After three or four days the wound is cauterized with sulphate of copper. By the eleventh or twelfth day the opening is closed by a new tissue of a grayish white colour. After a time sight begins to return, and as soon as the circumference of the cornea clears sufficiently, an operation for artificial pupil is performed.—*Annales d'Oculistique*, tome lvii. p. 261 (extracted from *Giorn. d'Oftalm. Ital.*, 1866).

[This operation can only be applicable to cases of partial staphyloma, and does not appear to possess any advantage over simple abscission.—D. A. R.]

RUPTURE OF THE CAPSULE AND SUBSTANCE OF THE LENS, THE RESULT OF HANGING.

Dr Ezra Dyer (Philadelphia) examined the eyes of a criminal who had been hanged, and found rupture of the capsule of the lens in the one eye, and rupture of the substance of the lens in the other. He was also able to produce the same result in several large dogs which he subjected to hanging.—*Annales d'Oculistique*, tome lvii. p. 289.

TRAUMATIC FISSURE OF THE IRIS AND RUPTURE OF THE RETINA.

Dr Dohenen (Bonn) relates a case of the above injury, which derives its chief interest from the fact that the choroid remained entire while the retina was the seat of detachment and several ruptures, accompanied with considerable extravasation of blood. Hitherto it has been supposed that if rupture of any of the internal coats occurs as the result of injury, the choroid is the one affected, or if the retina is ruptured, the choroid must be ruptured as well, and cases hitherto narrated have supported this view. This case shows that the retina alone may be the seat of the lesion.—*Klin. Monatsbl. für Augenheilk.*, 1867, p. 160.

DR HAASE ON A CASE OF SATURNINE AMAUROSIS.

The patient was a landscape painter who used chiefly water colours, and particularly one containing red lead. He enjoyed good health till 1860, when he had an attack of lead colic, which was cured in eight weeks. He was again attacked with lead colic in 1866, and for three days the pain was very severe. On the ninth day, as the pain diminished, he was suddenly seized with dimness of sight, which rapidly increased, so that next morning he was blind in both eyes. He was examined on the afternoon of that day, when it was found that he could merely distinguish light from darkness. On ophthalmoscopic examination nothing abnormal was found beyond a slight congestion of the retinal vessels. The movements of the eyes were perfect. The gums were of a bluish colour, and the patient complained of a sweet taste in the mouth. He was subjected to local depletion by means of Heurteloup's artificial leech, and he was put on a course of opium. Under this treatment the sight gradually improved, so that in six days from the commencement of the attack he was able to read very large print. A sixth of a grain of morphia was now injected into the temples, and daily repeated, and at once rapid improvement ensued, so that in a week his sight was nearly restored ($S. = \frac{2}{3}$). Several months later vision remained the same, but he had insufficiency of the internal recti muscles, so that, on attempting to work, fatigue very soon obliged him to desist. He was also unable to distinguish colours so well as before, and thus had to give up his occupation as an artist.

In most cases of saturnine amaurosis the sight quickly returns. Relapses, however, are frequent if the patient subjects himself to the poison. Tanquerel puts great faith in the use of opium in the treatment of this disease. In the present case the beneficial effects appeared more rapidly and more decidedly from subcutaneous injection.—*Klin. Monatsbl. für Augenheilk.*, 1867, p. 225.

CASE OF SUDDEN MONOCULAR AMAUROSIS, RELATED BY DR SANDS (NEW YORK).

A doctor dressing one morning, felt his left eye all at once dazzled by a ray of sunlight. He immediately observed that his sight was dimmed, and in fifteen minutes he stated it was entirely lost. No pain was felt. Till then his sight had been excellent and his health good. Examination five hours after the accident right eye was normal. In left eye the pupil was dilated even when the other eye was open, and to a greater extent when it was shut. There was scarcely perception of light with it. The tension of the globe was normal and the media transparent. The retinal arterial branches were very small, scarcely visible beyond the optic disc; the veins were scarcely diminished in volume. In the centre of the macula lutea there was a deep red-coloured spot about one-third the size of the disc, well defined and circular, and another similar spot, about twice as large, between the disc and the macula lutea. The papilla was anæmic. There was apparently no cardiac affection. The eye was again examined seven hours later.

Leeches had been applied, whereby amelioration of the symptoms was produced, but still there was no central vision. The arteries were of natural size, the veins engorged. The retina presented a grayish cloudy appearance about the macula lutea, and the red spots still existed.

Eight days later the sight had improved, and the field of vision was enlarged; the retina was more transparent; the papilla was œdematous and infiltrated, of a dirty red colour, and had ill-defined margins.

The treatment had consisted in the administration of iodide of potassium and the application of a bandage. Instead of the bandage, the patient was desired to wear smoked glasses.

After three weeks the symptoms of neuritis disappeared, but the papilla appeared white and excavated. The field of vision again contracted, and the vessels diminished in calibre.

Dr Sands regards this case as probably one of extravasation of blood into the sheath of the optic nerve, giving rise to compression of it and occlusion of the blood-vessels, thus explaining the empty condition of the arteries and the distension of the veins. The blindness in the first instance was due to compression of the nerve and anæmia of the retina. By degrees the circulation became restored and the retina regained its functions, except where it was affected by the hæmorrhagic patches; but the presence of the blood-clot gives rise to optic neuritis, leading to atrophy of the nerve substance.—*Annales d'Ocultisque*, tome lvii. p. 286.

PECULIAR STATE OF VISION AFTER SOLUTION OF CATARACT.

Dr Rainy has recorded the case of a woman aged 65, the subject of cataract, who had the operation of discission performed four times, with the effect of eventually causing the absorption of the opaque lens. With a suitable bi-convex lens (+ 2½) she could read ordinary type, but no lens appeared to render her vision of distant objects relatively distinct. She was sent to Mr White, the Glasgow optician, who found that a plano-convex lens (+ 4½), with the convex side turned towards the eye, enabled her to read signs at the other side of a wide street with ease.—*Ophthalmic Review*, No. xi. p. 265.

FOREIGN BODY IN THE UPPER PUNCTUM LACRYMALE.

Mr Taylor has recorded the case of a brass-finisher, from whose left upper punctum-lacrymale he removed a fine brass spiral turning which had flown from the man's work as he was using the lathe. Foreign bodies are occasionally found impacted in the lower punctum; this appears to be the only instance in which such has occurred in the upper punctum.—*Ophthalmic Review*, No. xi. p. 273.

MR WILSON ON INVETERATE PANNUS TREATED BY INOCULATION.

A marked intractable case of granular lids was subjected to inoculation with the effect of removing the pannus and much of the attending opacity of the cornea. After the lapse of about two months the sight in the better eye was much improved by the performance of iridectomy. The same operation, performed at a later date on the other eye, also produced beneficial effects. His sight so improved that, in the course of a few months he was able to earn his living, while previous to the inoculation he was merely able to distinguish between light and darkness. The purulent ophthalmia induced by the inoculation was principally treated by compress bandages, which application Mr. Wilson recommends in the treatment of this as well as other forms of purulent ophthalmia.—*Ophthalmic Review*, No. 11, p. 273.

PECULIAR CASE OF COLOUR-BLINDNESS.

Mr B. J. Vernon relates the case of a medical man, the subject of congenital myopia, who one day discovered that in one eye he had a peculiar form of colour-blindness. Examination with the ophthalmoscope, three years ago, did not discover any marked change in either eye; but now, in the right eye, there is a distinct myopic crescent, and in the left some irregularity of the choroidal pigment at the entrance of the optic nerve. The faulty perception of colours is limited to one portion of the retina of his right eye, situated between the macula lutea and the entrance of the optic nerve. With this portion of the retina he cannot distinguish between blue and green, or between magenta and scarlet; while with other parts of this retina, and with the other eye, he can readily distinguish different shades of colour. His sight otherwise is better in the right than in the left eye. The patient himself is sure that, until lately, one eye could distinguish colours as well as its fellow.

Mr Vernon is inclined to ascribe the functional imperfection of the retina to the commencing choroidal changes ordinarily associated with myopia.—*Ophthalmic Review*, No. 11, p. 285 (extract from St·Barthol. Hosp. Rep. ii. p. 93).

ON THE DOUBLE VISION IN CASES OF LOCOMOTOR ATAXIA.

M. Galezowski attributes the double vision, in cases of locomotor ataxia, not to a momentary paralysis of the muscles, but rather to ataxia or their incoordinate action. This form of double vision may be distinguished from that due to paralysis of the muscles by the fact that in ataxia the right-hand image sometimes corresponds to the right, sometimes to the left eye in the same patient; and the double images coalesce by turning the head at one time to the right, at another to the left hand side; while in paralysis of the muscles the right-hand image constantly corresponds either to the right or to the left eye, and the images *always* coalesce by turning the head either to the right hand or to the left—but not sometimes to the one and sometimes to the other side.—*Annales d'Oculistique*, tome lviii. p. 101 (extracted from *Gazette des Hôpitaux*, 1867).

OPERATION FOR DIVERGENT STRABISMUS.

Dr Agnew (New York) describes his method of operating for divergent strabismus as follows:—

He lays bare the internal rectus muscle and passes a ligature under it, which he ties round the tendon close to its insertion into the sclerotic. The external rectus is now divided. He then returns to the internal rectus, and divides its tendon between the ligature and its insertion. By drawing the eye inwards he is now able to calculate what extent of the muscle requires to be removed to correct the divergence, and cuts off that portion, having previously introduced two fine sutures immediately posterior to the line of incision. By these sutures the extremity of the shortened muscle is attached to the conjunctiva close to the cornea. Thus the restraining band at the outer side (rectus externus), which prevented rotation inwards, is divided, and the shortened internal rectus so implanted in the globe as to enable it to act most effectually.—*Annales d'Oculistique*, tome lvii. p. 290.

STONY OR CALCAREOUS CYSTS OF THE EYEBROWS.

These cysts are of rare occurrence, and have been completely overlooked by the authors of most of the recent ophthalmic treatises. Dr Sichel of Paris removed one from a girl 17 years of age, in whom it had existed from infancy. As is usually the case, the cyst was intimately adherent to the periosteum.

On examination three years after its removal, the tumour presented a yellow colour, had a higher specific gravity than water, and on section exhibited in some parts a horny aspect. The application of nitric acid produced slight effervescence. On dissolving a portion of the tumour in nitric acid, and analysing the fluid, abundant indications of lime and magnesia were obtained.

Tests for sulphuric, hydrochloric, and phosphoric acids gave only negative results. On calcining a portion of the tumour with caustic potash, a small piece of the tumour, examined under the microscope, was found to consist of irregular cells. The cyst, therefore, was composed of particles of carbonate of lime and carbonate of magnesia, deposited in the irregular cells of an organic substance abounding in nitrogen.—*Annales d'Oculistique*, tome lvii. p. 211.

Part Fifth.

MEDICAL NEWS.

JURY TRIALS AND INSANITY.

By Professor Laycock.

Some trials have lately taken place in England which are worthy the very serious notice of the profession:—

1. Dr. Henry Pearson, a medical practitioner at Ely, was tried at the Cambridge Assizes in March last, on a charge of feloniously poisoning, by means of strychnine, two valuable horses, and convicted. No motive for the crime was discovered. In a letter published in the *British Medical Journal* for 7th December, Dr G. B. Mead calls the attention of the profession to this case, as being a "fearfully terrible miscarriage of justice." Dr. Pearson's name was erased from the medical register. The sentence passed upon him was equivalent to death by slow torture, for his health has irretrievably broken down. It appears that, in Ely, for some time previously, a number of domestic animals had been found dead, which had been secretly poisoned. Detectives were employed in consequence of the panic which arose, and Dr. Pearson was, in Dr. Mead's opinion, made the victim of it. The evidence against him was entirely circumstantial, and so manifestly inaccurate as to the facts, that it seemed hardly possible a jury could convict. Dr. Pearson protested his innocence, and courted every inquiry; but it appears that his friends (?) considered whether he should plead absolute innocence or insanity, the latter plea being, of course, an acknowledgment of the crime. By this false step they virtually concurred with popular prejudice against him, and (as it appears) on no other grounds than that his previous conduct had been marked by considerable eccentricity. It appears, too, that Dr. Pearson's counsel failed in his duty. He took no account of the prejudices raised against Dr. Pearson by the mysterious poisonings, and relied so entirely upon the manifest defects in the evidence for the prosecution for an acquittal, as to decline calling in the witnesses in defence. A conviction was the result. Dr. Pearson is expected to die, and will leave a family of four innocent children behind him. It is worthy of note that the mysterious poisonings at Ely still go on.

2. The next case is that of a man named Wiggins, a coal-porter in London who was convicted, in September last, of the murder of a woman with whom he cohabited. His case is made the subject of an article in the December number of *Macmillan's Magazine*, by Mr. Dicey, headed "An Unsolved Mystery." Wiggins took the girl from service at a public-house to live with him. She, however, proved to be of drunken habits, and pawned his things for drink. He resolved to get rid of her, and fixed the day for her departure. Early in the morning of the day, Wiggins appeared in the street with his throat cut, but not dangerously; she was found with hers also cut, and dead. Wiggins said that she had first attempted to murder him, and failing, had committed suicide. This was his defence at the trial. His defender seems to have failed to bring the evidence for the defence that might have been brought, for it appears that the girl had declared that, if he sent her away, she would commit suicide. The evidence was entirely circumstantial, and the verdict turned almost wholly upon the question, whether she could have cut her throat herself under the circumstances? Medical evidence was given to the effect that she could not. Wiggins was thereupon found guilty. He protested his innocence to the last moment, and frantically resisted the efforts of his executioner to hang him.

3. On the same day that Wiggins was executed, Louis Bordier, a Frenchman domiciled in London, was hung for precisely the same crime. The difference, however, in other particulars was complete. Bordier not only did not deny the act, but described how he did it to Mr Simpson, a physician and surgeon called to his victim, with the manner of a man who was satisfied with what he had done. The plea in his case was insanity, and again the counsel was so satisfied that the evidence for the prosecution would be sufficient for acquittal that he called no witnesses in support of the plea. The court had, however, no doubt of his sanity, and the jury decided accordingly, and the man was duly hung, much, apparently, to his own satisfaction. This case is one of peculiar interest, because it has given rise to an animated controversy, extending over several weeks, in the *Medical Times and Gazette*, as to the course and the plan of procedure which it behoves practitioners to follow in a case like Bordier's, and in those of alleged criminal lunacy generally. It appears that Bordier had lived with the woman as his wife for several years, and had had a family; the eldest was a girl twelve years old. In Scotland, she would have been held legally as his wife, but the editor of the *Medical Times and Gazette* chooses to call her his "paramour." He was a currier, and had been ill for several months with fistula and phthisis, and in consequence both he and his family had suffered much from poverty. He had been under treatment in St. Mark's Hospital for the fistula, where it was observed that he was deeply despondent. Shortly before her death, his wife had threatened to leave him in his distress and poverty, with his helpless young family on his hands, and go and live with another man. Whereupon his deep despondency seems to have changed in character, and he wrote a letter to his brother, and also a testamentary paper, announcing that he could endure his misery no longer; that it had become a "necessity" that he should kill his wife, whose conduct he praised highly, and blamed in no one particular; also, that, as he intended to commit suicide, he wished it to be understood that he was not insane, and the jury, on the expected inquest, were to bring in their verdict accordingly. But then he could not bear the thought that his children should survive

him, to be called the children of a murderer, and he had consequently resolved to kill them also. Somewhat in the Hamlet style of soliloquy, he also discussed the question of his future state in regard to the crimes he was about to commit, "if they were crimes," and hoped God would receive him nevertheless. The papers were written late on a Sunday night, and early on the following Tuesday morning he cut his wife's throat. He went also to his children's bedroom, and held his daughter's head back with the intent of cutting her throat, but her innocent sweet smile, he said, disarmed him. Now, if a practitioner had been called in before the tragedy was completed, the facts would have led him to diagnose the case as one of *phthisical insanity* (a variety elucidated in the Royal Edinburgh Asylum), with suicidal and homicidal tendencies, and he could have certified that Bordier was of unsound mind, and a proper person to be placed under restraint and treatment. Probably the editor of the *Medical Times and Gazette* would have agreed with Mr Simpson on this point, and if Mr Simpson, having so certified, had subsequently become the subject of prosecution by Bordier for damages in consequence, would have pointed out the injustice and impropriety thereof. But, according to the same authority, the commission of the contemplated act changed the whole character of the case. Mr Simpson was in error in giving the opinion that Bordier was insane. He was evidently "an atheistical dastardly Frenchman; there was absolutely no evidence of insanity whatever;" and hanging was his just doom. Not content with the expression of these opinions, the editor undertook the public duty of severely reprimanding Mr Simpson for differing in opinion from him; deduced from that circumstance conclusions which must have been at least very painful to Mr Simpson's feelings, inasmuch as he attributed to him both "incapacity" and presumption, and seriously admonished him that for the future he do not meddle with matters of which he was profoundly ignorant. Mr Simpson's position as a witness was peculiar, and he seems to have fulfilled his duties in a manner becoming that position. He did not volunteer his opinion, as the editor in question implied; neither was he incompetent from education and experience. But in his capacity of a citizen, being called by the prosecution to prove that the murder was committed by Bordier, he was converted, on cross-examination, into an expert, giving his evidence for the defence. Now, he was solemnly sworn to "tell the truth, the whole truth, and nothing but the truth;" if, then, it was his opinion that Bordier was insane at the time he committed the act, having been with him for thirty-five minutes immediately after it was done, was Mr Simpson to swear that, in his opinion, he was not? The final result of the long rambling arguments through which the editor led me as a dissentient, was practically to the effect that that was Mr Simpson's duty. In other words, he ought to have committed perjury. And the editor did not fail to advance grounds for this conclusion, drawn from popular prejudice and legal dicta. It was "very dangerous" to society to allow the plea of insanity to hold good in such cases like Bordier; to admit into the courts the "couldn't help it" theory (as he termed the facts of experience which clinically differentiate impulsive and emotional insanity from the raving which is the popular idea of madness) would "be subversive of the great laws of religion and morals," and all the usual clap-trap which characterizes an entire ignorance of the question. Unfortunately the plea of ignorance and inexperience, however available or explanatory of the editor's erroneous views, cannot apply to Dr Maudsley, who also criticized unfavourably Mr Simpson's conduct in court, and expressed his entire concurrence with the editor. Thus a worse state of things than ever has arisen as to the plea of insanity in criminal cases. No medical practitioner can expect to escape abuse if his opinion as a witness, even although compulsory and given on oath, do not coincide with the prejudices of the public or the doctrines or private ends of self-constituted critics.

It is common to all these cases that a miscarriage of justice appears to have arisen from the circumstance that strong prejudices influenced the juries. The forms of courts have been carefully maintained by the presiding judge with that impartiality for which, as a body, our judges are remarkable, beyond, perhaps, those of any other nation, but they have been perverted to the infliction of injustice. Such a state of things is in truth revolutionary; for when scepticism as to the just administration of the law is established, that moral force in which its whole power for good rests is lost. I need only instance the sympathy which has been created for the murderers of the policeman Brett at Manchester by a miscarriage of this kind. A man of the name of Maguire was tried for the same crime, and convicted, according to judicial phrase, on the clearest evidence, and sentenced to be hung. Yet he was not only wholly innocent, but had served his country faithfully, and was serving it, as a marine. Nevertheless it required much effort to save him from the gallows. From such facts the public not unnaturally conclude that like injustice may have been done to those who were hung, and who also protested their innocence; and thus the moral influence of the solemn proceedings at Manchester was neutralized. The pecuniary loss to the country in these abortive cases is not a trifle in lawyers' fees alone. The Attorney-General had a fee of 500 guineas at Manchester (it is said), with 50 guineas each morning as consultation fees. Two Queen's Counsel would divide between them (it is said) at least 1000 guineas, and two junior counsel from 250 to 300 guineas each. How many ruffians escape, and how many innocent men are punished, in accordance with these expensive methods of administering justice, cannot be calculated; it is certain not a few. Whether diseased wretches like Bordier and others should be destroyed or not in the interests of society, may be a proper matter for discussion, for the question is mooted by men of high social position and intelligence.

At all events, the profession has a duty to perform in this department of justice, namely, to grapple with the questions involved in the legal and moral responsibility of the insane. And it must be the whole profession, for if practitioners not specially experienced in the diagnosis and treatment of insanity take up the matter, they will be charged, as is proved by the case of Mr Simpson, with incapacity and presumption; or if they be specially experienced, they will be repudiated as "mad doctors," with their "fanciful theories," after the example of the *Medical Times and Gazette*, Lord Westbury, and numerous publicists. I would add, also, that the questions ought to be treated by the profession at large in a common-sense, practical way, so that public opinion shall be enlightened. Neither should time be lost, for they are ripe for discussion. The facts of experience are as well established as ever they are likely to be, and the need for the application of them to the due administration of justice, it must be admitted by all, is pressing.

How far the evidence in panics and under strong prejudices partakes of the character of hallucinations it is difficult to say; very recent experience points to the conclusion, that statements of detailed facts may be sworn to which are purely imaginary, and neither more nor less than insane delusions.

DEATH OF M. FLOURENS.

This distinguished physiologist has died at the age of 75. His experimental examination of the functions of the brain and medulla oblongata, and his researches on the growth of bone, are well known, and form his chief title to fame. He was an eloquent writer, and his biographies, *éloges*, history of the discovery of the circulation of the blood, treatise on human longevity, etc., are held in high estimation. He was born at Béziers, in the south of France, in 1791; when 16, he became a medical student at Montpellier, and took his doctor's degree at the early age of 19. The celebrated De Candolle, then Professor of Botany at Montpellier, was struck by his talents, and sent him to Paris, introducing him to Geoffroy St Hilaire and Cuvier. Through their powerful influence, Flourens obtained a Professorship of Comparative Physiology at the Jardin des Plantes, where he continued during the whole of the active period of his life, and a chair at the College dè France. He was also, by the same powerful patronage, chosen a member of the Academy of Sciences in 1827, of which he became one of the two perpetual secretaries. In 1840 he had the distinguished honour of being elected one of the forty members of the Académie Francaise. His rival for the seat in the Academy was Victor Hugo, but doubtless Flourens was preferred for other reasons than literary merit. Flourens may be considered fortunate in his early and high promotion. His researches are valuable, but have been eclipsed by those of succeeding physiologists, more especially by the discoveries of M. Claude Bernard, who is spoken of as his probable successor in the French Academy.

UNIVERSITY OF EDINBURGH.

The number of students matriculated in the University during the present Session is as follows:—Arts, 658; Medicine, 399; Law, 334; and Divinity, 61.

ROYAL COLLEGE OF PHYSICIANS OF EDINBURGH.

At the Annual Election Meeting of the Royal College of Physicians of Edinburgh, held on 5th December, the following office-bearers were elected for the ensuing year:—*President*—Dr John Moir. *Vice-President*—Dr John Smith. *Council*—Dr Robert Bowes Malcolm, Dr Alexander Wood, Dr John Graham Macdonald Burt, Dr Robert Paterson, Dr J. M. Duncan, Dr W. R. Sanders. *Examiners*—Drs Wood, Douglas, R. Paterson, Wright, Keiller, Pattison, Cumming, Matthews Duncan, Haldane, Sanders, G. W. Balfour, T. G. Stewart, J. Struthers, R. P. Ritchie, J. Linton, A. C. Brown, C. Muirhead, A. Dalzell. *Treasurer*—Dr Samuel Somerville. *Secretary*—Dr Rutherford Haldane. *Curator of Museum*—Dr William Henry Lowe. *Librarian*—Dr J. W. Begbie. *Registrar of Applicants for License*—Dr Rutherford Haldane. *Clerk*—Christopher Douglas, W.S. *Auditor*—Kenneth Mackenzie, C.A. *Under-Librarian*, John Small, M.A. *Officer*—Thomas Marshall.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

The following gentlemen were elected office-bearers for the ensuing year on December 18, 1867:—*President*—Robert Omond, M.D., F.R.C.S. *Vice-Presidents*—J. D. Gillespie, M.D.; A. Halliday Douglas, M.D.; W. R. Sanders, M.D. *Councillors*—J. A. Smith, M.D.; Joseph Bell, M.D.; Arthur Gamgee, M.D.; Charles Dycer, M.D.; Thomas Annandale, M.D.; James Sidey, M.D.; John Linton, M.D.; Andrew Smart, M.D. *Treasurer*—George W. Balfour, *Secretaries*—D. Argyll Robertson, M.D., Thomas Grainger Stewart, M.D.

EDINBURGH OBSTETRICAL SOCIETY.

The following office-bearers were elected for the next two years at the meeting held on 27th inst. :—*President*—Dr Burn. *Vice-Presidents*—Dr W. Bryce, Dr J. A. Sidey. *Treasurer*—Dr James Young. *Secretaries*—Dr R. Peel Ritchie, Dr G. Stevenson Smith. *Council*—Dr Menzies, Dr Keiller, Dr Stephenson.

ROYAL MEDICAL SOCIETY.

At a meeting of the Society, held in its Hall, 7 Melbourne Place, on the evening of November 29th, the following gentlemen were elected Presidents of the Society for the ensuing session :—Messrs William Hogg, F.G.S.E., Jamaica; Richard Caton, M.B.C.M., Yorkshire; Ch. P. Nicolson, M.A., B. Sc., F.G.S.E., India; A. Christy Wilson, M.B., C.M., L.R.C.S.E., Roxburghshire.

HOW MUCH ARSENIC WILL KILL A FLY?

Orfila, the celebrated toxicologist, being examined on a capital trial, was asked by the President whether he could tell what quantity of arsenic was requisite to kill a fly. The doctor replied, "Certainly, M. le President; but I must know beforehand the age of the fly, its sex, its temperament, its condition and habit of body; whether married or single, widower or maiden, widow or bachelor. When satisfied in these points, I can answer your question."

GOODSIR FELLOWSHIP—SUBSCRIPTION LIST *continued.*

D. Macpherson, Esq., Commissioner of Customs, Newchang,	£3	0	0
James Watson, Esq., M.D., Newchang,	10	0	0
Per Robert Bryson & Sons, Watchmakers :—			
Robert Bryson, Esq.,	1	1	0
James Newlands, Esq., Liverpool,	1	1	0
Charles Stevenson, Esq.,	1	1	0
James Ritchie, Esq.,	1	1	0
John Clapperton, Esq.,	1	1	0
Per Bank of Scotland :—			
Dr T. G. Stewart,	2	2	0
Per Duncan, Flockhart, & Co., North Bridge :—			
A. K. Drysdale. Esq., Surgeon, 79th Regiment,	1	1	0
W. Johnston, M.D., Army Medical Staff,	1	1	0
Messrs Duncan, Flockhart, & Co., Princes Street,	1	1	0
G. E. Steuart, Esq., M.D.,	1	1	0
Dr Prance, Plymouth,	1	1	0

CORRESPONDENCE.

PROFESSOR MACLAGAN ON SUGAR IN THE URINE AS A SYMPTOM OF DEATH FROM COLD.

To the Editor of the Edinburgh Medical Journal.

28 *Heriot Row, Edinburgh*, 17*th December* 1867.

My dear Sir,—Dr Bence Jones, in the Proceedings of the Royal Society of London, 14th December 1864, narrated some experiments which showed that when small animals (rabbits) were killed by subjecting them for some time to a freezing temperature, the urine was rendered diabetic Dr Jones' experiments were repeated and confirmed by Professor Brücke, who found that the sugar, of which traces can always be found in the urine of rabbits, was "incomparably greater" when they were frozen to death. When lecturing to my class of Medical Jurisprudence on death from exposure, I have mentioned these results of Jones and Brücke as possibly furnishing an additional diagnostic of death having taken place from cold in the case of persons found dead in wintry weather. It is true that, in the majority of instances, such cases occur under circumstances which render any scientific inquiry into the cause of death unnecessary, and consequently the authorities do not order a post-mortem examination. The urine, however, could easily be drawn off by a catheter in cases where neither the authorities order, nor the relatives permit a dissection.

As almost every year some such cases occur in this country, I venture to ask the favour of your inserting this note, so that if it meets the eye of any of our brethren in whose neighbourhood such a case occurs, he would do me the favour to procure and forward to me a sample of the urine, that the presence, and, if it be present, the quantity, of sugar might be determined.—Yours faithfully, DOUGLAS MACLAGAN.

PUBLICATIONS RECEIVED.

Braithwaite's Retrospect of Medicine. London, 1867.
W. Morrant Baker,—Kirkes' Handbook of Physiology. Sixth Edition. London, 1867.
Catalogue of United States Army Medical Museum. Washington, 1867.
William Cortis,—Naquet's Modern Chemistry. London, 1867.
William H. Dickinson, M.D.,—Pathology and Treatment of Albuminuria. Lond., 1867.
Dr Esmarch, — Chronische Gelenkentzündungen. Kiel, 1867.
Graily Hewitt, M.D.,—Diseases of Women. London, 1868.
L. Ollier,—Traité expérimental et clinique de la Régénération des Os. Paris, 1867.
J. I. Mackenzie, M.B.,—On the Climate of Sidmouth. London, 1867.
Henry W. Rumsey, M.D.,—On State Medicine. London, 1867.
J. B. Sanderson, M.D.,—Handbook of the Sphygmograph. London, 1867.
G. J. Symons,—Rain. London, 1867.
Edward J. Waring, M.D.,—Cottage Hospitals. 1867.
A. W. Wemyss, M.D.,—Remarks on the Lunacy Acts for Scotland. Edin., 1867.
Morrill Wyman, M.D.,—On Corporal Punishment in Public Schools. Cambridge, U.S., 1867.

PERIODICALS RECEIVED.

Archiv für Ophthalmologie,—Berlin, 1867.
Annales d'Oculistique,—Sept. and Oct.
American Journal of the Medical Sciences, —October.
Berliner klin. Wochensch.,—Nos. 43 to 51.
Births, Deaths, and Marriages,—for October and Nov.; and Quarterly Return, Sept.
Bulletin Général de Thérapeutique,—Aug., Sept., and Oct.
Medical Press,—Nov. 6 to Dec. 25.
Dub. Quarterly Journal of Medical Science, —Nov.
Gazette des Hôpitaux,—Nos. 126 to 149.
Gaz. Hebd. de Médecine, etc.,—Nos. 44 to 51.
Gazette Médicale de Paris,—Nos. 43 to 51.
Glasgow Medical Journal,—Nov. and Dec.
Journal of Cutaneous Medicine,—Jan. 1868.
Journal de Médecine Mentale,—No. 11.
Journal of the Scottish Meteorological Society,—October.
Journal of Anatomy and Physiology,—Nov.
Klin. Monatsbl. f. Augenheilkunde,—July, Aug., and Sept.
Medical Times and Gazette,—Nov. and Dec.
Medizinische Jahrbucher,—Nos. 5 and 6. Vienna, 1867.
Monatsschrift für Ohrenheilkunde,—No. 1.
New York Medical Journal,—Oct. and Nov.
Rev. de Thérapeutique Médico-Chirurgicale, —Nos. 21 to 24.
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Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Therapeutical Action of Medicines in Dilated Conditions of the Bloodvessels.* By ARCHIBALD REITH, M.D., Physician to the Royal Infirmary of Aberdeen.

(*Read before the Medico-Chirurgical Society of Aberdeen.*)

IT is admitted by all practical physicians, that the science of therapeutics is in a most unsatisfactory state. A brief glance at the immense variety of conflicting opinions entertained regarding the best mode of treating disease, is sufficient to convince every intelligent mind that the whole subject is in utter confusion. It is only necessary to compare the various recommendations for the treatment of but a single disease, made by those authors who have written on it, in order to be satisfied that very little is known of the remedies for our bodily infirmities. It is still a standing reproach on the profession, that after centuries of careful observation and experiment so little progress should have been made towards those great objects of medicine,—the relief of suffering, and the prolongation of human life. Chemistry, physiology, and pathology have made most rapid advances, but therapeutics seem to be at a stand-still. It is true, progress of a certain kind has been made. A school of men has arisen, of which Professor Bennett of Edinburgh is one of the principal exponents, who trust almost entirely to the powers of nature in the cure of diseases. Founding their opinions on the well-known teaching of experience, that medicines, in their present mode of administration, do not, as a rule, exert any remediable influence on disease, they conclude that diseases always run a natural course, and cannot be modified or controlled by any medicinal agent whatever. Among the immense number of powerful agents found in nature, they believe that few have yet been discovered which will exercise a curative effect on the maladies of the human frame. These views are rapidly gaining ground, and are embraced by perhaps the greater number of physicians at the present day. The drugging system has been abandoned, and the natural system resorted to,—of feeding nature, and allowing nature to look after itself. There is thus progress made, in a way, towards the better treatment of disease. I say, in a way, because it is not true scientific progress. It is merely the abandonment of the system of doing too much—the *nimia diligentia*—for the adoption of the system of doing little or nothing. Of the two, certainly the natural system is to be preferred. It allows nature to exercise her own undoubted powers unchecked, and thus contributes much more surely to the welfare and recovery of the patient. Should the case terminate fatally, the patient dies at least a *natural* death. But still, it is not scientific progress. It is a reaction from over-credulity to over-scepticism, from the belief in the absolute necessity of medicines to a conviction of their absolute uselessness. If we contrast the position of our forefathers, and their vast array of drugs, with our own present helpless position—standing with folded arms at our patient's side, doing little more than announcing the name of his disease, and giving daily bulletins of its course—we will be little inclined to consider our progress very satisfactory.

The conclusion on which the followers of the natural system seem to proceed is, that there are few agents yet discovered which will exert a true remediable influence on disease. The oft-repeated, almost invariable failures met with in practice, and the discrepancy among observers regarding the action of drugs, seem to give colour to that opinion. It is impossible to look back over the recorded experience of the last fifty years without being in a considerable degree inclined towards more or less complete disbelief in medicinal agency. That period has witnessed the rise and fall of a whole dynasty of medicines, each one more vaunted and promising more fair than its predecessor, but all destined more or less to fall into disuse. It cannot be wondered at, then, if the rising generation of medical men should declare themselves sceptics. Yet, notwithstanding the great uncertainty of medicinal action, it has long appeared to me that the conclusions on which the natural system of therapeutics is based, are premature. They proceed on the assumption that our knowledge of disease and of medicinal action on the living body, is sufficiently perfect to warrant an accurate determination of the influence exerted by the one over the other. To be sound, they ought to be based on a thorough acquaintance with Pathology on the one hand, and of Materia Medica on the other. It is needless to say how far short we are of this perfection. Rapid as has been the advance of pathology, and clear as is the light it has shed on many obscurities, we must, nevertheless, confess our ignorance of disease in its true nature and causation. The state of the Materia Medica is not much better. Although many drugs have been familiar to the profession for centuries, and although an infinite number of experiments of almost every kind have been made with them, both on man and on the lower animals, there is still great discrepancy regarding their properties. The diversity of opinion is not so much in regard to their physiological or toxic properties; on the contrary, most writers are at one on the symptoms usually manifested in the living body by each medicine. But it is when their application to therapeutics is considered, when their medicinal virtues come to be judged of, that great diversity exists. On this point there is scarcely any agreement. And moreover, old and apparently settled opinions on the properties of medicines are every now and then changing. Agents, which have long been prescribed in accordance with a given theory, are suddenly found to be possessed of different qualities, and their administration varies accordingly. This is remarkably so at the present time. Quinine, so long the fashionable tonic, the universal panacea for all sorts of debility, is already regarded with doubt. Digitalis, belladonna, iron, even opium itself, are the objects of suspicion, and the subjects of new trials. And it would not be surprising if the fabrics raised by time-honoured belief should crumble to pieces under the rude shocks of an age, the characteristic of which is to believe nothing and doubt everything.

Now, it may be asked, with good reason, When and where is this to end? Are we always to oscillate from side to side, without fixed principles, driven to and fro like the waves of the sea? Are we never to have decided opinions, but to go on as before, disbelieving or doubting to-morrow what we believe to-day, rendering ourselves and our profession the laughing-stock of an intelligent public? It is to be hoped not. There are even now signs of a change; and I venture to predict that before long, very many of the questions which now disturb and distract us will be definitely settled on a fixed basis. But whatever may be the future of therapeutics, it is certainly premature, in the face of present diversities, to accept and act upon the conclusion that disease is irremediable by medicinal agency. The proper course is evidently to undertake a careful re-examination of the whole subject—to begin, as it were, at the beginning, and, with the additional knowledge afforded by the progress of the collateral sciences, endeavour to procure accurate results. A long-continued course of persevering investigations could not fail to be productive of very important practical benefit.

In endeavouring to afford some little help towards the elucidation of this very important subject, I may premise that observers, in studying the action of medicines, appear to have fallen into two mistakes, both of them insignificant at first sight, but leading to serious fallacies. The first mistake consisted in drawing inferences as to the action of medicines on man, from the effects observed by experimenting with them on the lower animals; and the second, in regarding the symptoms produced in the human economy as the only and invariable physiological phenomena characteristic of the substances ingested.[1] It is scarcely necessary to bring forward proofs of the many fallacies to which the first error leads. The want of correspondence between the properties manifested by certain substances when administered to animals, and those exhibited in man, is admitted on all hands. The habits and mode of life of animals being so diverse from those of man, no proper comparison can be made between them. Some animals are uninfluenced by agents which destroy human life. The information, therefore, derived from this source, however valuable in itself, is by no means a trustworthy indication of medicinal action. The second error is a very natural one, and, in the circumstances, almost impossible to avoid. That the physiological symptoms of a medicine, repeated invariably whenever it was administered, should be regarded as its only action, was merely what might be expected. It could scarcely be otherwise. Yet it was an error, and has proved itself to be so, by the very fact, that there still exist conflicting theories as to the therapeutical value of those medicines, regarding whose physiological properties mostly all are agreed. It is my object, in this and following papers, to show that medicines have another and a very different action on the economy from what their physiological symptoms would lead us *à priori* to suppose they had.

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[1] I find that I am anticipated in this remark by Brown-Séquard, in his admirable lectures.—*Lancet*, 10th March 1866, p. 248.

If we carefully examine the fundamental nature of diseases in general, we cannot avoid noticing the singular uniformity as to origin which prevails among them. Widely different as are the ultimate results, and varied as are the mechanical and chemical changes induced in the body, it is possible, nevertheless, to trace upwards the various stages of disease, until we arrive at what appears, so far as we know, to be its common centre, from which it derives its origin. It matters not what names we give to diseases. We may call them by as many names as we choose, until Nosology be a complete dictionary; but all arise, if our pathology be correct, from the same centre. This centre is the joint cerebro-spinal and vaso-motor nervous systems. The latest physiological and pathological researches warrant us, I think, in laying down the proposition—that most diseases are due to a morbid state of the nervous system, which morbid state either arises in the nervous system itself, or is induced by the presence of a noxious element in the blood. Few, I suppose, will question this proposition. Disease has many forms and produces many changes, but the first step in the downward direction is derangement of the nervous influence, whether cerebro-spinal or vaso-motor. It is, then, of the greatest importance to determine the nature of this derangement; for if we can do so, we make great progress towards a proper understanding of the treatment to be pursued.

In following out this subject, I intend to confine myself on the present occasion to the consideration of those morbid conditions which are attended by dilatation of the blood-vessels. I shall enter into some details regarding the origin of inflammation, a condition occupying so prominent a place in the production of disease, and hope to demonstrate that the course of the inflammatory process is precisely analogous to the effect of medicines on the system. The therapeutical action of medicines will then be more clearly understood.

Inflammation is, then, as I understand it, primarily and essentially a disorder of the nervous system. The stagnation of blood, the effusion of lymph and serum, the suppuration, etc., which are commonly identified with inflammation, are themselves only the result of deeper seated morbid processes affecting conjointly the cerebro-spinal and vaso-motor nervous systems. The researches of Bernard and Brown-Séquard on the nervous system have brought to light the very important part which nervous influence plays in the production of inflammation. These writers, as well as others, have shown that if the cervical sympathetic be irritated, certain changes take place in the parts to which it is distributed. These changes are summed up by Brown-Séquard as follows:—

1. Contraction of Bloodvessels.
2. Diminution of Blood.
3. Decrease of Vital Properties.

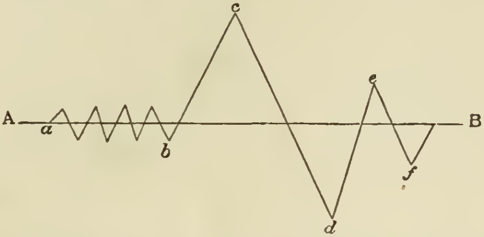
On the other hand, if the same nerve be simply divided, the opposite condition takes place, namely—

1. Dilatation of Bloodvessels.
2. Afflux of Blood.
3. Increase of Vital Properties.

In the first case, the vaso-motor nerve has been excited or stimulated; in the second, it has been paralyzed. Here is, then, a miniature portrait of inflammation. It will be seen that the effects of paralysis of the sympathetic bear a close resemblance to the state of parts in the inflammatory process. In that morbid condition there exist enlargement of the bloodvessels, with consequent increase in the quantity of blood, and greater activity of the vital properties of the tissues, all of which are closely similar to the results of division of the sympathetic. There can be little doubt, therefore, that this nerve exercises considerable influence in the production of inflammation. But this is not all. There seems, in order to constitute a true phlogistic action to be another influence at work, that of the cerebro-spinal system, which operates in a manner hitherto unexplained; and, moreover, Lister's experiments prove that, along with derangement of the combined nervous influence, there are local changes in the affected part. There is what is called a suspension of the concurrent exercise of function among the minute elements of the tissues of the organs involved. Thus, three factors enter into the composition of inflammation. There is first, paralysis of the vaso-motor nerves; secondly, derangement of the cerebro-spinal influence; and, thirdly, suspension of molecular function. Among them, however, there seems good reason for concluding that the chief place is taken by the vaso-motor nerve. Whatever part may be played by the cerebro-spinal nerves, I am inclined to think that it is secondary in point of importance to that of the sympathetic. It is true that the two systems are mutually dependent on each other. The vaso-motor derives its influence from the cerebro-spinal, while the latter, on the other hand, is dependent on the vaso-motor for the due transmission of its supply of blood. It would appear, also, from the experiments of Claude Bernard, that while the sympathetic system regulates the supply of blood to the various organs of the body, the cerebro-spinal determines the character of the nutrition. For example, when the skin is wounded, repair seems to be effected by the combined influence of the two great divisions of the nervous system which we are considering. Blood is sent to the wounded part by the vaso-motor nerves, while the particular organization formed from the blastema is determined by the cerebro-spinal nerves. I am aware that an opinion has lately been started to the effect that the cerebro-spinal and vaso-motor systems are antagonistic; but I think it has been advanced without good cause. It rests only on the apparent anomaly that, in Bernard's experiments section of the cervical sympathetic was followed, not by an increase of the salivary secretion, as would *à priori* have been expected, but by a diminution; and that, conversely, irritation of the nerve was followed, not by diminution, but by increase of the saliva. On these experiments rests the theory of the antagonism of the two great nervous centres. The anomaly, however, is more apparent than real; and, had I time, I might give a satisfactory explanation of it, as well as of another, which is, that the paralyzing influence of belladonna on the cervical sympathetic is attended by dilatation of the pupil, when we would have expected contraction. These anomalies, however inexplicable, are insufficient to establish any proof of antagonism between the cerebro-spinal and vaso-motor systems; and, in the meantime, we are forced to regard both these systems as mutually dependent, acting simultaneously in health, and affected conjointly in disease. In inflammation, they both suffer, the vaso-motor probably being first involved.

Now, it is of the highest importance to determine, as far as possible, the first step in this nervous derangement. And here it is that, in my estimation, a serious oversight has been made, leading to very erroneous practice. It is commonly supposed that the paralysis of the sympathetic on which inflammation in great measure depends, is the *primary* effect of the exciting cause; whereas, in truth, it is its *secondary* effect. The first action of all those causes which tend to produce inflammation in the body is to stimulate and excite the vaso-motor nerves, the result of which is the contraction of bloodvessels, decrease of blood, and of vital properties, which, as we have seen, follow galvanization of this nerve in animals. This state of excitement is followed, as a natural and necessary consequence, by a *reaction* to the opposite state of paralysis, with those local changes which result therefrom, as seen also in animals after division of the sympathetic. Thus, inflammation, as observed in the body, is a reaction from a previous and often invisible stimulus upon the vaso-motor system. Let me explain this a little more clearly.

The vaso-motor system in health may be compared to a balance moving gently upwards and downwards, never rising too high, nor falling too low. To illustrate this, let the straight line A B, in the accompanying diagram, represent the vaso-motor system when exactly midway between excitement and paralysis; in other words, the state of the bloodvessels when midway between contraction and dilatation. The zig-zag line *a b* will represent the variable condition of the vaso-motor system. The alternation of rising and falling represents the alternate state of excitement and depression which constitutes the healthy condition. This variation is essential to health. In fact, all the functions of the body depend on it. When food is taken into the stomach, its presence there produces excitement of the vaso-motor nerves, followed almost immediately by reaction and determination of blood to the viscus, whereby, along with cerebro-spinal influence, the secretion of gastric juice is effected. When the body is thrown into active exercise, there is a certain amount of heat generated by friction and other causes, and this heat acting primarily as a stimulant, induces reaction to what, for convenience sake, I shall call a healthy paralysis of the sympathetic, creating that activity of the circulation and comfortable feeling of warmth which are so familiar to us all. A cold bath acts in a similar way. And numberless instances might be mentioned, showing that upon this singular alternation of stimulus and reaction the wellbeing of the economy is greatly dependent. If the vaso-motor system were fixed invariably in the middle position indicated by the line AB, it would no more resemble an elastic band adapting itself to circumstances, or a watch-spring in its regular oscillations, but would be rigid and motionless. In such a case, digestion would be at a stand-still. The quantity of gastric juice poured forth in a given time would be insufficient for the solution of the food necessary to nutrition. We should be unable to endure the heat of summer, or the cold of winter, much less the hourly vicissitudes of temperature to which we are exposed. But nature provides for these changes by endowing the vaso-motor system with a degree of elasticity which, within certain limits, enables it to adapt itself to the various conditions of the body. Now, suppose the stimulus to be too strong. Suppose the line *a b* to be acted upon by a force stronger than usual, such as intense cold, and is pulled higher than is consistent with health, say to a point *c*, which represents spasm



of the bloodvessels, the necessary consequence is, that, when the force has expended itself, the reaction will be proportionally excessive, and the line will fall lower than usual, say to *d*. The point *d*, then, will represent inflammation as manifested to our senses by its ordinary symptoms. The condition indicated by the point *c*, that of excitement, is often invisible, or at least unnoticed, and can only be presumed to have existed from the appearance of the opposite state of reaction. It has not received the attention it deserves, but it is, nevertheless, the primary and essential step of the whole morbid process. Inflammation cannot take place without it, for, as I shall hereafter show, all these causes which induce the inflammatory state, act primarily as stimulants of the vaso-motor system. A very simple experiment will place this in a clear light. If ether spray be directed on the skin by means of Dr Richardson's apparatus, the effects of the intense cold will be seen by the naked eye. All the symptoms of galvanization of the sympathetic which I mentioned a little ago, are at once observed, namely, contraction of bloodvessels, diminution of blood, and decrease of vital properties, manifested by whiteness and insensibility of the part. When the stimulus is removed, reaction gradually takes place till the parts resume their natural appearance; but it does not stop there. It goes beyond health, and approximates disease. The surface is congested—there is dilatation of the bloodvessels, more blood, and more vital energy, as increased heat and sensibility, than are found in the ordinary state. And if the cold were applied for a sufficient length of time, stagnation, with its consequences, would doubtless follow. I need scarcely refer to the familiar example of what takes place in the web of the bat's wing or frog's foot under the microscope, when pricked with a needle or touched with a drop of alcohol. It has long been known that the first effect of the stimulus is to contract the vessels, to be followed by their dilatation and stagnation of blood. Such is exactly the course of inflammation. Cold, or some other agent, first of all stimulates the vaso-motor system, causing spasm of the bloodvessels. Simultaneously with this, and perhaps in consequence of it, there is derangement of the cerebro-spinal influence, and suspension of the concurrent exercise of function among the minute elements of the tissues. Then follows reaction, proportionate in intensity to the previous stimulus, paralysis of the vaso-motor system takes place, causing dilatation of the bloodvessels, with all those local changes in succession which constitute the inflammatory state.[1]

In thus endeavouring to explain the origin of inflammation, it is not to be understood that I am underrating the influence of the cerebro-spinal system by giving so much prominence to the vaso-motor. On the contrary, I do not think that true *active* congestion can take place without previous disturbance of the cerebro-spinal influence. Paralysis of the vaso-motor nerves seems to go no further than passive congestion, at least, if the continuity of the nerve be gradually or gently interrupted. This was well seen in a case which I published in the *Medical Times and Gazette* of Nov. 11, 1865. In that patient, interruption of the functions of both cervical sympathetics was followed by intense *passive* congestion in the organs to which they were distributed, but there was no inflammation. I am, therefore, inclined to believe that, while the vaso-motor system takes the chief part, a concurrent derangement of the cerebro-spinal is requisite for the production of true inflammation. Perhaps, also, a good deal depends on the force and suddenness of the original stimulus; which, if severe, might tend to cause active congestion by the intense sudden reaction consequent.

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[1] "When bloodvessels in the ear and face of a dog are made to contract considerably by a very powerful galvanic excitation of the cervical sympathetic nerve, we find that, after a short time, they become exhausted and paralyzed, and that, consequently, they dilate as much as when their motor nerve is divided and paralyzed."—Brown-Séquard, *Lancet*, 10th March 1866, p. 248, *note*.

Though not bearing directly on the object of this paper, I may here touch upon the febrile state which so often accompanies inflammation, and is called inflammatory fever. This febrile state is not due, as is often supposed, to an effort of the heart to overcome the obstacle offered by the local lesion to the passage of blood. If it were so, we should have inflammatory fever in every disease where there was an obstruction to the sanguineous current, which is not the case. We have, moreover, a condition bordering on fever, where we know there can be no such obstacle. During active exercise, the circulation is in a state closely allied to inflammatory fever. There must be another cause, therefore, for the febrile condition. That cause is the same stimulus which originated the inflammation. Let us look for a moment at the peculiar anatomical arrangement of the cardiac nerves. Between the brain and the abdomen extends a nervous circle, of which one segment is formed by the pneumogastrics, and the other by the cervical and thoracic sympathetics, the great and small splanchnic nerves. The two segments are connected above with the brain, and below with the chylo-poietic viscera. In the centre of this circle, which may be denominated the cerebro-ganglionic circle, lies the heart, and from the circumference are sent off a number of branches for distribution to that organ. The heart's action is thus directly controlled by the cerebro-ganglionic circle. Every impression communicated to the latter is felt by the former. Now, if a strong stimulus be directed on the nervous circle, such as cold from without, or a poison from within, it is thrown into a state of temporary excitement, causing those rigors and general depression which usher in a fever, and constitute its cold stage. The reaction from this is its hot or febrile stage. If the morbid action go no further, the fever subsides, and its subsidence is attended with perspiration, which is looked upon as a successful effort of nature to throw off the offending cause, inasmuch as it is attended with relief to the symptoms. But it is rather a coincidence, and should be regarded as a concomitant sign that the nervous circle is recovering its equilibrium. Now, in the case of inflammation, if the original stimulus be comparatively weak, it will affect only the local nerves of the part involved, as, for instance, in coryza or tonsillitis, where there is often no attendant fever. But if the stimulus be powerful, it will not only act on the nerves of the inflamed organ, but will at the same time excite the nerves of the heart, causing decrease of the vital properties of the organ, and of blood in its tissue, manifested by chilliness, paleness of the surface, and general depression. Then comes reaction, proportionate to the intensity and duration of the previous stimulus, more blood is sent to the tissue of the heart in consequence of the paralysis of its nerves, there is an increase of its vital properties manifested by the greater frequency and force of its contractions, the functions of the economy are momentarily exalted, rendering the blood richer in fibrin—in a word, there is inflammatory fever. Thus, what was long considered an indication of increased power in the heart, is, in reality, only a sign of its weakness. The quicker and more strongly the heart beats, the more are its nerves paralyzed.

Having thus given a brief outline of the part played by the vaso-motor system in the incipient stages of inflammation, I have now to show that the action of medicines on the living body is precisely analogous. Whatever may be the ultimate effect of a substance, whether it be known as a purgative, a tonic, or a narcotic, its primary action is on the vaso-motor system. No doubt, in many cases there is a variety of secondary phenomena, as might be expected from the complexity of the organism, and it may sometimes be difficult to define the local origin of the symptoms; yet, when these symptoms are carefully analyzed, they will be found to possess a local origin at first. Dr Christison, in his work on poisons, says,—"Poisons have been often, but erroneously, said to affect remotely the general system. A few of them, such as arsenic and mercury, do indeed appear to affect very many organs of the body. But by much the larger proportion seem, on the contrary, to act on one or more organs only, not on the general system." And further on he says,—"Some act chiefly by enfeebling or paralyzing the heart, others principally by obstructing the pulmonary capillaries, others by obstructing the capillaries of the general system, others by stimulating or depressing the functions of the brain or of the spinal cord, others by irritating the alimentary canal, others by stimulating one or another of the glandular organs, such as the salivary glands, the liver, the kidneys, or the lymphatic glands." It is foreign to my purpose to inquire into the secondary or general effects of medicines; these may be accounted for in a variety of ways, and do not influence the matter on hand. But what I wish to point out is, that when the local effects of drugs are minutely examined, they will be found always to involve more or less the vaso-motor system, and to produce in it changes similar to, if not identical with, those which occur in inflammation. The medicines themselves may be arranged under two heads: those which have no reaction after their stimulus on the nerves or spasm of the bloodvessels—the pure astringents, acting chemically on the tissues,—and those which, after their stimulant or spasmodic action has passed off, are followed by more or less reaction to paralysis of the sympathetic or dilatation of bloodvessels. To the latter class belong nearly all the contents of the Materia Medica. They possess the double property of stimulating and paralyzing the sympathetic,—in other words, of contracting and dilating the bloodvessels. Take the case of purgatives. A double action is universally ascribed to them, their purgative effects being usually followed by more or less constipation. But what I wish particularly to call attention to is, that, in accordance with the laws of inflammation, prior to their purgative action, and essential to it,—the cause of it, as it were, —there is a previous spasm of the bloodvessels, or excitement of the sympathetic, and that the subsequent purgation is in proportion to the intensity and duration of that excitement. We know well that laxative medicines act by creating irritation and inflammation of the mucous membrane of the intestinal canal,—an inflammation not to be distinguished from that which is called idiopathic. If, then, this idiopathic inflammation originate in the manner I have already described, as it undoubtedly does, it is but a natural inference to conclude, that purgatives operate in a similar way, and begin their action with spasm of the bloodvessels. Tonics, again, will in large doses irritate the stomach, from which we conclude that, as in the case of purgatives and emetics, their primary action on the bloodvessels is spasmodic. Narcotics also belong to the same category. Indeed, one of them, belladonna, is now recognised as having the double action which I am endeavouring to show belongs to medicines in general. Brown-Séquard, speaking of the remedial effects of belladonna, says,—"Most of these remedial effects are evidently due to a contraction of bloodvessels, as are also other effects, such as the stoppage of hæmorrhages, and of the mammary and salivary secretions produced by belladonna and the ergot of rye." And then he goes on to say,—"A remedy can produce with different doses two opposite effects. Belladonna, for example, by its influence on the bloodvessels of the spinal cord, will diminish sensibility, the reflex faculty, the tendency to convulsions, etc.; but if the dose be toxic, there will be a morbidly increased sensibility, reflex faculty, and convulsions will occur." . . . "A number of remedies may have quite opposite action in remedial and toxic doses."[1] These remarks almost anticipate my paper, but I quote them in confirmation of the views I now inculcate, which I held before they appeared, and which I have worked out independently. It may be difficult at first sight to account for the varied action of narcotics on these principles. But when we consider that the brain is an aggregation into one organ of many separate and so far independent portions, and that each narcotic may have a special preference for one portion more than another, the difficulty vanishes. We know that the base of the brain consists of a number of distinct and separate ganglia, intimately connected, yet functionally independent, and that the spinal cord has its anterior, posterior, and lateral columns, and its central gray matter, each with its own function, though bound together in one organ. There is good reason also for concluding the cerebrum to be a congeries of separate organs. The singular affection called aphasia seems to show that the organ of articulate language is one small convolution on the left side of the cerebrum. Each convolution is probably a separate organ by itself, and the brain an aggregate of these organs. In all likelihood, therefore, each narcotic influences its own particular portion specially, implicating the others only through the intimate connexion which subsists between them. Now, when we examine the action of the best known narcotics, we find that one of the earliest manifestations of their toxic or poisonous properties is a congested state of the parts on which their influence is exerted. The poisonous action of belladonna, for example, is attended by dilatation of the bloodvessels of the face and head, and doubtless also of the brain. Opium also is attended with determination of blood to the base of the brain, and when its symptoms are fully developed, they are not to be distinguished from apoplexy of the pons Varolii. And whatever narcotic we examine, congestion is one of its prominent early characteristics. If, then, a dilated state of the bloodvessels of the brain, or its separate parts, be one of the first symptoms of a narcotic, the inference is plain ; there must have been a previous primary action of spasm or excitement of the vaso-motor nerves ; and narcotics would come under the same category as purgatives, tonics, and other medicines, having, like them, a double action on the vaso-motor system—first excitement, and then paralysis. Thus all the varieties of medicines are identical in their primary action ; the difference in their secondary effects being due, partly at least, to that particular portion of the sympathetic for which they severally have an affinity. Purgatives act on various divisions of the abdominal sympathetic, tonics on the solar plexus and cerebro-ganglionic circle, narcotics on the cervical sympathetic, diuretics on the renal nerves, and so on. Hence, it might be a legitimate question, whether those names which indicate only their secondary action might not with some propriety be abolished, and some such name as vaso-motor stimulant substituted. The term neurotic is rather vague, for, according to these views, all medicines are properly neurotic. A definite term expressing the primary and local effect of a drug would, at all events, convey to the mind a much better idea of its medicinal action than such names as purgative, narcotic, and the like—names which are too often made the groundwork for the purest empiricism, sometimes of erroneous practice.

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[1] *Lancet*, 10th March 1866, p. 248.

It will not be out of place here to notice the singular analogy subsisting between the primary effects of medicines and those of fever poisons. As each medicine acts on its own portion of the sympathetic, so each fever poison implicates a particular part of the same system. The ultimate derangements may be, and are, very various; but the brunt of each fever falls on one division of the vaso-motor nerves. In typhus, for instance, the cervical sympathetic suffers. I have seen both nerves swollen to twice or thrice their usual size in fatal cases; and Dr Beveridge has, I believe, always found the same condition in the post-mortem examinations he has made. Enteric fever involves the abdominal sympathetic, the ganglia of which are found enlarged and softened. The other febrile diseases also have their favourite nidus. Curiously enough, too, they all commence their action by inducing spasm of the bloodvessels (cold stage), and the reaction from that indicates the particular disease present (febrile stage). These facts are most instructive. They show that the vaso-motor system is one, and that whatever agent operates on it, the effects are the same—excitement first, paralysis afterwards.

I come now to the practical application of the doctrines I have been inculcating. This part of the subject is so important, and offers so wide a field for investigation and experiment, that I beg to call special attention to it, the more so, considering the extensive part which inflammation and dilated conditions of the bloodvessels generally play in the production of disease. If, then, medicinal agents stimulate the vaso-motor system primarily (spasm of bloodvessels), and paralyze it secondarily (dilatation of bloodvessels), it may be supposed that inflammation being a paralysis of the same system, it would be aggravated by medicines in their ordinary physiological doses. Experience confirms this supposition. We find in practice that purgatives are injurious in, and aggravate diarrhœa; tonics and emetics increase pre-existing irritation of the stomach; narcotics are hurtful in congested states of the brain; diuretics in inflammation of the kidney. All this is true, supposing the medicines given in the usual prescribed doses of the Pharmacopœia. But if we concede a double action to medicines, if we admit that prior to their recognised effect on the economy, they have a spasmodic action on the bloodvessels, inducing their contraction, we will naturally inquire how this primary action may be turned to practical account. What I maintain is, that medicines given in very small doses when the bloodvessels are dilated (paralysis of sympathetic) will exert their primary action of contracting the bloodvessels (excitement of sympathetic), and no more. Their action will then cease; for whatever benefit is to be expected by administering medicinal agents in doses so small as to go no further than spasm of the bloodvessels, it will then be accomplished. This will be understood by a reference to the diagram, page 685. The point *d* is supposed to represent the state of paralysis to which the sympathetic has fallen from an opposite state of excitement, *c*. It therefore represents the inflammatory state. Now, if we wish to raise the nerve to the level of health, we shall evidently gain our object by applying a stimulus to it. If we administer a medicine in a physiological or toxic dose, the stimulus will be too strong, the nerve will be raised as high or higher than before (*c*), and there would be a corresponding reaction to the old state (*d*), or even lower, representing an aggravation of the symptoms. But if a very small dose be given, so as to afford a comparatively weak stimulus, the nerve will be raised to a point considerably below that of its first excitement, say to *e*, and the reaction from this moderate excitement or vascular spasm would bring the nerve almost or altogether to a state of health, say to *f*, which is much higher than *d*, the inflammatory state. The therapeutical effect of a medicine is thus the reverse of its physiological action, at least in dilated conditions of the bloodvessels. If disease be attended with spasm of the bloodvessels, the usual physiological doses are properly indicated; but if the opposite condition exist, these doses do harm. A much smaller dose is therefore necessary. Now, the question is, How small must the dose be which is required to produce the stimulant effect without causing undue reaction? If one grain of a medicine induce both excessive stimulus and excessive reaction, what fraction of a grain will suffice for a moderate stimulus and reaction? This obviously can only be determined by experiment. On this subject I have been making observations for a considerable time. The limits of this paper forbid me to bring forward the results on the present occasion, but I hope hereafter to submit my experience in an extended form. I may state that my observations fully bear out the views I have been propounding, and that I have arrived at some very interesting and striking results. Care must be taken, however, not to draw inferences, as has hitherto been done, from the effects of medicines on the healthy body. To find out that five grains of aloes will purge, and thence to infer that the proper use of aloes as a medicinal agent is to purge, and that five grains is the dose, is an example of what I mean. This is the only result to be obtained by experimenting on the healthy economy.[1] Unfortunately, however, the whole present system of therapeutics is based on it. It is the secret of the confusion which exists in the profession regarding the medicinal treatment of disease. No idea of medicinal dose can be obtained by such means, although the observations are most valuable on other accounts. The primary stimulating action of drugs is in most cases invisible, when they are administered to a healthy subject. Cold is one exceptional agent. We can see with the naked eye, and appreciate by other means, both its effects. We recognise also the two primary effects of fever poisons. But with most drugs it is different. For the momentary contraction of bloodvessels in separate portions of the body cannot easily be taken cognisance of, either because they are too limited to produce appreciable symptoms, or because the healthy nerves cannot be influenced except by such doses as not only contract the vessels, but induce subsequent reaction. We are forced, therefore, to conclude in this way, that when such reaction exists, it must have been preceded by stimulus of the sympathetic (contracted bloodvessels). The case is otherwise when we come to experiment with diseased nerves, that is to say, with the paralyzed sympathetic (dilated bloodvessels). There the effects of a small dose are at once seen in the contraction of bloodvessels and restoration to the healthy state. How a minute dose should thus affect a paralyzed nerve, while it uninfluences it in its ordinary state, is a phenomenon we cannot explain. But that it does so, there can be no doubt. There seems to be a peculiar susceptibility to medicine manifested by the sympathetic in its paralyzed state. The same thing is witnessed by physiologists in animals. In Brown-Séquard's Physiology and Pathology of the Central Nervous System,[1] we find the following results described, among many others, as due to section of the sympathetic:—

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[1] Lancet, *loc. cit.*

"27. The first convulsions after poisoning by strychnia take place there.

"28. A galvanic current too weak to act on the other side may produce contractions there."

From which experiments we conclude, that a dose of a medicine too weak to exert any action on the sound nerves, will operate on them when paralyzed. If we suppose strychnia to be primarily a stimulant to the vaso-motor system, it would, in the same manner as a purgative, aggravate a pre-existing paralysis of that system, shown by the increased vitality of the parts, and greater readiness to contraction of the muscles. But if a very small dose be administered, it would, by contracting the bloodvessels, remove those symptoms previously existing. All medicines act in a similar way on the nerves for which they severally have an affinity, the therapeutical dose being considerably smaller than the physiological, and only to be determined with accuracy by repeated experiments on those morbid parts for which they are indicated.

A proposal has lately emanated from high quarters, to the effect that a systematic and careful investigation should be made on the physiological action of medicines on the system. The importance of this proposal cannot be over-estimated. We may expect from its adoption the most beneficial results. In particular, we may expect to see existing discrepancies among observers swept away, and more or less uniformity substituted. We may hope that by this means the profession will be brought to a more accurate knowledge of the physiological or toxic effects of drugs on the living body. But I expect much more from the investigation than the proposers probably have dreamt of. When the physiological effects of a medicine are fairly established, they will become a guide to its therapeutical action in those morbid conditions which are attended with dilatation of the bloodvessels (paralysis of sympathetic). We shall know then, that in such conditions its remedial action is the reverse of its toxic; and, moreover, that the dose required for the former action must be much less than for the latter. And then another field of observation has to be entered upon. We shall have to inquire what is the therapeutical dose of each medicine, the physiological dose of which we already know. As already stated, this can only be done by experiments on diseased nerves. Here is a field for experiment, so wide, so interesting, so promising, that when once entered upon, it will be cultivated with an eagerness and assiduity which no other field of science can be, and which will yield such a harvest of practical good as will throw into the shade the results of other branches of medical science. The result to therapeutics will be something like the following: each medicine will be found to irritate or cause determination of blood to that part of the system for which it has a special affinity originally, whatever may be the subsequent effects; and in morbid conditions, where such determination of blood is one of the chief symptoms, the medicine having an affinity for the affected part will prove to be the remedial agent in doses much smaller than are required for its toxic effects.

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[1] Philadelphia, p. 141.

I have thus endeavoured to show that inflammation and medicinal action are identical in their primary origin, and that the administration of drugs must be regulated accordingly. What bearing these views have on other morbid conditions, I must not inquire into at present, but leave for future consideration.[1]

The question will naturally arise, Do these therapeutical views not favour Homœopathy? If by that term be understood the principles and practice of the followers of Hahnemann, so far from these receiving encouragement from what I have stated, I am persuaded that when the double action of medicines is universally recognised and acted upon, the props and stay of Homœopathy will be cut away. But if the term be taken in its literal signification, the principles laid down most certainly give countenance to it. There can be no doubt that, with all its absurdities, Homœopathy contains a germ of truth. That germ is its original dictum of *similia similibus curantur.*[1] Hahnemann's double error, in which he has been exceeded even to extravagance by his disciples, consisted in making that dictum universally applicable to all diseases, and in pushing the small dose to infinitesimals. Had he stopped where he began, there would have been an end of all controversy. But when he developed his extraordinary opinions, and intelligent minds had naturally revolted from a system so absurd, they recoiled also from the truth out of which it sprang. Hence the doctrine of similars came to be, as it is now almost always, associated and confounded with infinitesimalism. The two, however, are essentially distinct. On the other hand, our error has been to push the principle of *contraria contrariis* to a corresponding extreme. The truth, I apprehend, lies midway between these two extremes. It is this middle position that I am endeavouring to establish. Let the homœopaths abandon, as many of them are doing, their wild theories of potentization and infinitesimalism, agreeing to receive the principle of *contraria contrariis* as having a certain range of application, and let us abandon our corresponding exclusiveness, adopting the principle of *similia similibus* where it is indicated by the pathology: let this take place, and we should have agreement upon a rational system of therapeutics. It must come to this sooner or later; for, after all, the doctrines of Hippocrates are still as true as they were on the day of their first declaration—some diseases are cured by medicines which produce the opposite effects, and some diseases are cured by medicines which produce the same effect.

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[1] Since these views were matured, I have come upon the doctrines of Broussais, to which they have some resemblance. Although Broussais' opinions are now very properly abandoned, there is, nevertheless, a germ of truth in them. His idea that irritation was an essential feature of all diseases, is not so far wrong, if we limit it to those accompanied by determination of blood to the affected part. That dilatation of bloodvessels or paralysis of the sympathetic does exist primarily in many morbid states, there can be no doubt; that medicines act primarily in a similar way is also certain. Whatever remedial influence, then, is to be expected from medicines, it will be exercised by contracting the bloodvessels. This is true, notwithstanding the variety of morbid causes. In the case of simple inflammation, the medicines will be directly curative; but in specific diseases, the contraction of bloodvessels either will not take place, or if it does, no effect will be produced on the disease, or its course will be merely modified. Broussais does not appear to have had any proper idea of the application of his views to practice.

In looking over the medical literature of the past few years, it is curious to notice how unconsciously homœopathic is the practice of the profession in the literal sense of *similia similibus curantur.* Let us take the theory of substitution advocated by Trousseau, and named Homœopathy by him. On this theory he proposes to cure existing inflammations by substituting, through the agency of irritants, a milder and more tractable inflammation, *e.g.*, nitrate of silver for ophthalmia, saline purgatives for diarrhœa, etc. The principle, he says, is that the irritant drug creates an inflammation which is milder and more curable than the original. It does not appear from Trousseau's writings that he had any idea of the pathological reasons for his theory. He seems to have acted on it empirically. The explanation, however, is easy. [See diagram.] By such means, an irritant medicine acting on the nerves in the condition represented by *d* (paralysis of sympathetic and dilatation of bloodvessels) would bring them to the opposite state, and being administered in toxic or physiological doses, would elevate the nerves as far as *c* (excitement of sympathetic and contraction of bloodvessels). Reaction would take place to *d* again, which would now represent the new and milder artificial inflammation, from which recovery more readily takes place. Now, experience proves, as in the case of silver in ophthalmia and purgatives in diarrhœa, that by such treatment the symptoms are in most cases aggravated, as might be expected. Some are no doubt benefited, the operation of the medicine probably breaking up the spell under which the nerves are bound, and thus clearing the way for their natural elasticity to recover itself, but the tendency of such practice is the reverse. It comes near the truth; and had the doses recommended been stimulating or contracting doses, the substitution theory would have been correct. It is impossible to read Trousseau's works without observing how homœopathic his practice was, in the sense of *similia similibus*, when he had to do with inflammatory disorders, thus empirically and unconsciously confirming the views I have been expounding on pathological laws. And when we examine the action of the few specifics we possess, it will be found that they exercise their remedial influence in accordance with the same principle. Mercury, the specific for syphilis, will, in large doses, produce symtoms resembling that disease. Quinine, as Cullen remarks, causes intermittent fever and cures it. Chlorate of potash, *the* remedy *par excellence* for ulcerative stomatitis, will, in large quantity, cause soreness of the gums and inflammation of the mouth. Bromide of potassium, so beneficial in epilepsy, will, when very freely administered, induce symptoms resembling the effects of that disease. Antimony, so valuable in pneumonia, will induce congestion of the lungs.[1] I might multiply instances, but these will, I think, suffice to show that, where dilatation of bloodvessels existed, the specific remedy has acted on the literal principle of *similia similibus curantur*, the dose empirically found to be remedial being much smaller than the physiological. We have merely to extend the principle to the more energetic medicines, whose toxic effects are manifested by comparatively minute doses, such as strychnia, arsenic, aconite, etc. The question for us is, how small a dose of these will contract the bloodvessels, *and no more*, when so small a quantity, comparatively, will induce both contraction and dilatation.

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[1] I use the term here for convenience, and not as recognising it to be a scientific law. The medicines given as I recommend really act on the principle of *contraria contrariis*, and can only be regarded as acting homœopathically in respect of their physiological action.

If, then, the foregoing views, be correct, as daily experience convinces me they are, they will tend to reconcile many conflicting therapeutical opinions, and simplify the treatment of disease in so far as medicinal agency is concerned. I have no wish to push their application beyond their proper limits, or to claim for them more success in practice than they deserve; but I do say, that whatever benefit contraction of bloodvessels may be expected to produce in the treatment of disease, the administration of medicines on these principles will certainly effect it. The conclusions to which I have arrived are these :—

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[1] A singular confirmation of these opinions has been lately promulgated. Dr Gairdner declares that the beneficial effect of antimony in pneumonia ceases on the development of its physiological symptoms, and that the remedial doses must therefore be much less. We find also, in Grave's Clinical Medicine, a similar statement regarding antimony as a tonic in dyspepsia.

1. Whatever may be the ultimate and varied effects of medicines on the system, their primary action is analogous to the primary steps of the inflammatory process, namely, excitement of the sympathetic (contraction of bloodvessels), followed by paralysis (dilatation of bloodvessels).[1]

2. The variety in the symptoms produced by the different medicines are due, partly at least, to the local tissues or nerves for which they severally have an affinity.

3. In diseases attended with a dilated condition of the bloodvessels, the proper medicines to administer are those which act physiologically on the affected part, and the dose must be such as will do no more than excite contraction of the dilated vessels (stimulus of sympathetic).

4. In so far as the dose proper to such conditions is unknown, it is to be determined by experience, and by observation on the paralyzed vaso-motor system.

In my next communication, I shall bring forward the results of my experience.

ARTICLE II.—*On a New Method of Extracting in Cases of Cataract, with Reports of fifty-two Cases operated upon in succession, from November* 1865 *to May* 1867, *in fifty-one of which the Lens was extracted, and a successful result obtained.* By CHARLES TAYLOR, M.D. Edin., F.R.C.S.E., Honorary Surgeon to the Nottingham and Midland Eye Infirmary.

EARLY in the year 1865, dissatisfied with the results of some cases of cataract in which the lens had been extracted by Schufte's method, that I had an opportunity of witnessing, and anxious to avoid that feeling of utter powerlessness which most operators must have felt in the event of any accident occurring during the performance of the ordinary flap operation, I endeavoured to ascertain whether extraction might not be accomplished through a wound so small that there would be no risk in such manipulation of the eyeball as would be necessary to facilitate the various steps of the operation, and yet large enough to permit the exit of the lens without the introduction of any instrument into the eye, or consequent risk of those disastrous results which I had seen to follow the lifting out of the lens with Schufte's spoons. With this object I made various experiments upon animals' eyes, and performed some operations on the dead subject; thus arriving at a satisfactory solution of the question in the operation, which I have since exclusively adopted in all cases of senile cataract coming under my care, and which I now recommend with much confidence to all who would adopt a method of extracting which is as nearly as may be "danger free."

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[1] It is of no consequence what view we take of the very incipient act of inflammation, whether the nerves or the molecules, as Lister has it, are first affected, the result is the same—the blood is driven back, and the vessels contracted.

I forwarded a short notice of this method to Dr Richardson for publication in the Medical History of Nottingham, in 1865, and also mentioned it in a paper published in the *Ophthalmic Review* upwards of a year ago, in the following terms: "Premising an iridectomy, and making the incision with a small ground-down cataract knife, entered in the corneo-sclerotic junction, and emerging at a counter puncture similarly placed, so as to incise a little more than the upper third of the cornea, the opening being well back, and larger than the large one sometimes required in Schufte's operation." —"The eye may be safely fixed throughout the operation. The flap cannot be turned down, and yet if the posterior lip of the wound be gently pressed back, the lens may be coaxed out without passing any instrument into the eye." My observations had then demonstrated, to my entire satisfaction, that, with the advantages of the enlarged pupil and extended surface for operation, obtainable either by a combined or preliminary iridectomy, the flaccid receding flap, composed of one half of the true cornea, becomes absolutely unnecessary; and that an extended flap-like incision, comprising one-third only of the corneal surface, if made well back in the corneo-sclerotic junction, will suffice for the ready exit of the lens, without the aid of any traction instrument whatever. I found, also, that such an incision yielded more readily to the advancing lens than one more strictly linear in its character, while it was sufficiently slit-like and elastic to close without any artificial apposition or external support, strong enough to resist protrusion by advancing vitreus, and too resilient to permit of accidental reflection. I propose to call this mode of operating "Extraction by Linear Flap," as I think the term expresses its peculiarities better than any other, and will serve to distinguish it from Von Gräfe's strictly linear method, more especially in those points in which it essentially differs, and, in my opinion, possesses some advantages. I must mention that I had fully worked out this method, adopted it in my practice, and demonstrated its advantages to my medical friends, before I had heard of Professor Von Gräfe's modification of linear extraction, and before any account of his experiments had been published. On first hearing of his modified linear extraction, I was under the impression that the two operations were almost identical; but having seen Von Gräfe's operation, I find material differences, although I have no doubt, from what I have since learned, that we arrived at the same conclusions as to the necessity for a modification of Schufte's and the flap operation by a similar process of reasoning, and apparently at about the same time. In my first operations, as the most convenient instrument accessible in a provincial town, I made the incision with a ground-down Zehender's cataract knife, reduced to about the size of the ordinary secondary cataract knives sold in most cases of ophthalmic instruments. I have since used Von Gräfe's knife; but it is quite possible to make the incision with Bader's smallest sized iridectomy knife, cutting gently round from left to right within the prescribed limits. As the risk, even if vomiting occurs, is very slight with this form of incision, I usually prefer to have the patient narcotized. Keeping the lids apart with the stop-speculum, I then fix the eye by seizing the upper border of the external rectus tendon with a pair of forceps, and enter the long narrow-bladed knife in the corneo-sclerotic junction, about the commencement of the upper third, or junction of the upper with the middle third of the cornea, say $2\frac{1}{2}$ or 3 lines lower down than in Von Gräfe's method, and rather more forward in the corneo-sclerotic junction. The site of the entrance puncture must vary somewhat, according to the size of the globe, as a very shallow flap will suffice if the cornea is of average or more than average dimensions, and a deeper one will be required if it is below the standard size. Care must be taken, in entering the knife, that it is made to penetrate the chamber at once. Any gliding between the layers of the cornea is much to be deprecated, as the consequent want of correspondence between the internal and external wound will be sure to cause disappointment in the subsequent steps of the operation, and the lens will be impeded in its exit by little bands of partially incised corneal tissue occupying the angles of the wound. Having entered the chamber, the knife must be pushed gently across, and made to emerge at a counter puncture similarly placed. The necessity for using a knife very small in bulk, narrow, and sharp-pointed, will here be appreciated, as the resistance offered by the dense tissue to the passage of the knife in this situation is often sufficient to cause slight bending of the blade. It is important that the counter puncture should not be made too far back—an accident very apt to occur, and by which one is betrayed into making an unduly large wound, a considerable portion of which will be situated in the sclerotic, thus giving rise to risk from escape of vitreous humour.[1] A little practice, the avoidance of haste, and occasional apposition of the operator's forefinger, will usually suffice to prevent any trouble on this score. Having completed the counter puncture, I push the knife well through, and, holding it parallel to the iris, cut directly upwards about two lines and a half, or until the edge of the blade is on a level with the central summit of the cornea. It is then turned directly forwards, and the section completed by a gentle sawing movement, thus forming a small, shallow, elongated flap, situated well back in dense tissue, with sloping sides and transverse centre, which last occupies the summit of the true cornea. The aqueous escapes when the counter puncture is made, and unless a preliminary iridectomy has been performed, the iris frequently folds over the edge of the knife. Gentle pressure on the cornea, and careful management of the blade, I have always found sufficient to free it, and enable the operator to complete the section without wounding the iris. The accompanying diagrams show the site of the incision and shape of the flap, in my operation, as contrasted with Von Gräfe's.

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[1] I must not forget to allude to the diminished risks from loss of vitreus obtainable by the form of incision I have recommended, in preference to one placed further back. I have always regarded the loss of vitreus myself as a serious misadventure, and have so often seen vomiting, serious constitutional disturbance, and consequent increased injury to the eye, caused by its evacuation, that I cannot understand the placidity with which I am aware some operators regard this accident.

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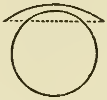

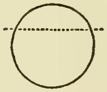

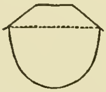

1. Von Gräfe's Incision. 2. Author's Incision. 3. Shape of Flap in Author's Method.[1]

Having completed the incision, I usually remove the speculum so as to allow the eye to recover somewhat, and a little aqueous humour to collect, and then proceed to the second step of the operation. It is safer now and subsequently to insert only one blade of the speculum under the upper lid, the other blade being supported on the patient's cheek by the finger of an assistant; scarcely any pressure is thus made on the eyeball, and risk of rupture of the hyaloid membrane is considerably diminished.

As the conjunctiva is only incised, and to a very slight extent at the extremities of the wound, there is usually little trouble from hæmorrhage up to this stage; should the incision, however, by accident be placed too far back, and bleeding occur, it is well to apply slight pressure to the eyeball with a pad of cotton-wool, before proceeding to the second step of the operation, which, if an iridectomy has not been premised, will be the excision of a small piece of iris. This is readily accomplished by inserting the forceps into the central portion of the incision, which occupies the apex of the cornea, while an assistant, if necessary, gently depresses the globe. This situation of the central portion of the incision enables the operator to enter the chamber at once, and the difficulties sometimes met with when the forceps and pricker have to be thrust under loose conjunctiva, with its sub-mucous tissue infiltrated with blood and aqueous humour (a source of embarrassment in Von Gräfe's method) are thus avoided. Having introduced the forceps, the blades must be opened and the iris seized near to the pupillary border, gently withdrawn, and divided close to the edge of the section. For this purpose I use very fine-pointed bent scissors, and insert the points just within the angle of the incision; if this is not attended to, the iris is very apt to prolapse in this situa-

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[1] The artist has represented the flap rather larger than is necessary.

tion, and to some extent delay the progress of the case as well as occasion a little deformity, which will be noticed afterwards. Having divided the iris at the right angle of the incision, it must be separated gently, so far as may be, from its ciliary border to the opposite edge of the wound, and cut off with the same precautions to secure the small piece so apt to prolapse as before. Should the iris bleed, so as to threaten to fill the anterior chamber, and so obscure the future steps of the operation, it is well to repeat slight pressure with a cotton-wool pad for a minute or two; this will arrest the hæmorrhage, and any blood may then be removed by suction, or coaxed out by manipulation of the eyeball, while the posterior lip of the incision is gently pressed back with the curette. In one case (Mrs N., Case 44), where it was very desirable to enlarge the pupil, I excised a portion of the lower central segment of the iris through the same wound, and at the same time as the upper, with a very good result. The iridectomy is more readily performed, and the excision of a smaller portion of iris will suffice, if the pupil is well dilated prior to the operation. This is best effected by a strong neutral solution of atropine, applied the night before, and repeated in the morning. The pupil contracts on the escape of the aqueous humour, but not so readily as it would, if not previously dilated; and the subsequent dilatation on which the success of the operation may in some degree depend, is afterwards much more easily maintained. I have, in several cases, where great age, marasmus, arterial degeneration, or the existence of other causes was likely to impede the progress of the case, or otherwise vitiate the result, adopted Mooren's precaution, and performed the iridectomy six weeks before extracting, and in very unfavourable cases, as in the eye remaining to be operated on in Smith's case, where suppuration threatened for twenty-four hours after the extraction. I have excised a large piece of iris both from the upper and lower segment of the pupil as a preliminary precaution. This division of the operation of extraction for cataract into two portions, with a prolonged interval, is, however, manifestly inconvenient both for the poor, who can ill spare the time required for two operations, and also for the wealthy, who often have a considerable distance to travel. Sometimes, also, from a want of correspondence between the wound made in extraction and the smaller one used in the iridectomy, I have found the iris more apt to prolapse at the corners than if excised in proportion to the length of the incision at the time of extraction. In some cases, however, a preliminary iridectomy has many advantages; by it the operator becomes thoroughly acquainted with his patient, and he is enabled to form a correct opinion as to his behaviour under extraction—a matter of no slight importance, as all accustomed to deal with such cases must allow. The important question as to the use or omission of chloroform may also then be decided; and, if administered, its effect upon the patient at the time, and subsequently, so far as vomiting is concerned, may be noted and utilized to the evident advantage of both patient and operator. In many cases also, where cataract is complicated with other manifest or incipient diseases of the eyeball, tension is reduced, and the tendency to cyclitis diminished by a preliminary iridectomy; subsequent extraction is easier to perform, and risk from bruising the iris abolished. The operation is thus reduced to its simplest elements, and chances of failure materially lessened by eliminating causes of accident. Having completed the iridectomy, the operator should seize the globe with forceps close to the cornea, above the insertion of the inferior rectus tendon, depress the globe with very slight pressure, and incise the capsule freely, taking care to pass the pricker beneath the lower segment of iris, and open the capsule from bottom to top, in two or three places. In withdrawing the pricker, it should be gently inserted under one of the angles of iris left by the excision, and drawn to the extremity of the other, so as to lay the capsule freely open, on a level with the equator of the lens. It is important to open the capsule in this situation, as I have more than once seen the exit of the lens retarded by little undivided shreds of membrane occupying the angles of the wound. Not unfrequently the lens comes forward, and is extruded immediately the capsule is incised; but, if from any cause it should be retarded, it will be necessary to make pressure on the globe, while the posterior lip of the wound is slightly pressed back with a small spoon or curette. Pressure is best made with the ordinary strabismus hook, placed in contact with the globe, below the insertion of the inferior rectus tendon, about four or five lines from the margin of the cornea. It must be moderated according to circumstances, should cease when the lens is partially extruded, and evacuation completed either by the insertion of a sharp hook into the lens, or by gentle pressure on the corneal surface, so as to lift forward its lower border as it is in the act of escaping. Cortical fragments, if any remain, may usually be extruded by slight pressure; but if this does not suffice, I usually place a temporary pad of cotton-wool on the eye, and allow the patient to recover from the chloroform narcosis, when the aqueous will be re-secreted, and any fragments concealed by the iris may be washed into the pupillary area by slight pressure on the globe external to the lids, and evacuated by gently opening the wound with the curette. If any remain, I remove them with Mr Teale's suction curette (the sucker must always be used with great caution, otherwise hyperæmia ex vacuo will be induced), and then tear across or withdraw any little fragments of capsule with iris or canula forceps. By thus carefully removing the capsule subsequent to the extraction of the lens, I believe all the advantages obtainable by the evacuation of the lens, with its capsule entire, as in Pagenlecher's operation, are obtained; but if the operator should desire to remove the lens and capsule in its entirety, as in that operation, I believe the form of incision which I have recommended is the one best adapted to secure a safe and perfect result. Should the lens, from adhesions or other causes, resist such pressure as may be safely employed without the risk of rupturing the hyaloid membrane, as has happened in some of my cases, I extract it with a small skeleton spoon.[1] From the large size of the wound this becomes very easy, and as the flap yields readily, the lens is easily tilted forwards, and there is no necessity to incur the risks inseparable from the insertion of the spoon too far back into the vitreous cavity.

If it is desirable to extract by the lower section, the same form of incision may be employed. The operator seizes the conjunctiva above the cornea in its vertical meridian with toothed forceps, while an assistant draws down the lower lid, and assists in fixing the globe. The lower section is preferable in cases where the lens is superficially softened, as any soft cortical matter left behind in the eye will gravitate to the bottom of the anterior chamber, pass harmlessly through the coloboma iridis, and either undergo absorption, or be washed out in the gushes of aqueous humour. The risk of its lodging behind the lower segment of iris and setting up subsequent irritation is thus avoided, but the section is less sheltered, and the coloboma uglier and more disturbing to vision.

The operation completed, I apply a drop of solution of atropine in the corner of the eye, and cover the lids with a pad of fine cotton-wool dipped in, but not saturated with, cold water. If sodden, the pad is heavy, irregular, and unmanageable; but just moistened on the surface, it fits accurately into the orbital cavity, and speedily dries into a thin smooth film closely bedded to the surface of the lids, absorbing moisture, insuring equable support, a regular temperature, and the exclusion of light. It must be secured *in situ*, with gentle pressure, by thin strips of soap plaster, spread on unglazed calico (which adheres without warming). I find this by far the best dressing. Pressure can be accurately applied by strips fixed to the forehead and cheek, and, if necessary, by an additional broad strap passed from temple to temple I apply atropine and compress to both eyes, with a view of preventing the rolling of the eyeball and sympathetic action of the pupil, which takes place in concert with its fellow if one eye is uncovered. Over the compress I usually fit a strong pasteboard shade on to the forehead, with a little wool intervening, and extending to the tip of the nose. This will protect the eye from any accidental blow or involuntary motion occurring during the sleep of the patient. Any kind of compress bandage, if required, may be readily applied over the pads and plaster. If there should be much pain complained of after the operation, and it should not cease in a few hours, I give a small dose of morphia, and repeat it if necessary. Should this not suffice to relieve the patient, I apply four leeches to the temple, and as fast as one drops off replace it with another, until either the symptoms are relieved, or it is evident that no benefit may be expected from further depletion. Occasionally, the injection under the skin of the lid of $\frac{1}{6}$ to $\frac{1}{4}$ of a grain of morphia will afford immediate relief; and if there is much heat complained of, or perceptible increase of temperature, a linen compress must be applied, wet with iced water, and kept cold by a small piece of ice placed in the corner of the orbit, and re-applied as fast as it dissolves. A brisk purge is sometimes of service, and blood, in cases favourable for depletion, may be taken from the arm. Other symptoms must be treated as they arise on general principles. If swelling of the lids comes on, with profuse semi-purulent secretion, the compress bandage, as advised by Von Gräfe, must be applied, and changed every four hours, a hot aromatic poultice being applied for twenty minutes at each change.

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[1] This is a mere rim of flattened steel wire reduced to extreme tenuity and shaped like the rectus spoon made for me by Messrs Wood of Manchester.

The swelling of the lids may also be treated with a solution of nitrate of silver, in equal parts of spirit of nitric ether and distilled water (10gr. to ℥j.), or by the application of tincture of iodine. Chronic irritation may be relieved by similar means—mercurial inunction with belladonna over the brow, and the evacuation of the aqueous humour, prolonged rest in the recumbent posture, or a second iridectomy downwards. If any capsule is left, it is better not to attempt its removal for two or three months. As to results, since devising the above described method, I have operated on 52 cases of cataract in succession, with only one complete failure.

The ages of the patients range from 5 months to 86 years. The majority, however, were persons of great age, several verging on 80, six being upwards of that age, and four upwards of 85 years. Some were suffering from marasmus and constitutional cachexiæ, others from various diseases of the eyeball, tending to vitiate the results of an operation. These persons were operated upon in succession as they applied, no selection was made, and I have never refused a case. Indeed, two or three, including the one complete failure (occurring in a feeble woman, æt. 86, blind for twelve years with degenerated cataract and firm synechiæ), had been declined by other surgeons. In two cases (æt. 65, No. 23, and æt. 68, No 17) healing was protracted, and the patients remained a month under treatment. One (æt. 83, Case 18) died from apoplexy, the result of epilepsy, thirty hours after the operation, when the wound was found healed, media clear, and result of operation excellent. This proves that the wound, even in persons of great age, will heal in twenty-four hours. In one (æt. 65, Case 25) suppuration threatened for twenty-four hours, the patient ultimately recovering with a closed pupil, and fair chance of restored vision by a second iridectomy downwards. The remainder recovered without a bad symptom, and were discharged on the seventh day; such as were free from remnants of capsule or amaurotic complications being restored to excellent vision. Some have been too recently operated upon to permit a fair record of the optical results at present. In two cases the lens was removed by the scoop, and in seven extracted by Mr Teale's suction method. Brief reports of the whole of those cases operated upon by myself since November 1865 will be given in the concluding part of this paper. I have preferred to give *all* the cases, including the suction and scoop extractions; and, where there can be no objection to this being done, have followed Dr Mooren's example, and appended the names and addresses of the patients, so that there may be no room for doubt or cavil as to the results.

(*To be continued.*)

ARTICLE III.—*Remarks on Retained Placenta.* By CHARLES BELL, M.D., F.R.C.P.E.

(*Read before the Obstetrical Society, 13th November* 1867.)

HAVING experienced the difficulty and anxiety, as well as the deep responsibility, incurred in the management of retained placenta, I have been induced to bring the following cases before the Society, not only from their interesting character, but in order to draw forth the opinion of the members who have had more extensive opportunities than I have had of judging of the importance of the subject, and with the view of benefiting by their observations.

There are four principal causes assigned as producing retention of the placenta, each of which demands our special care and attention. The most common is inertia, consequent on tedious labour, or natural feebleness of constitution. This may affect the uterus generally, or it may be partial, in which case it is liable to be complicated with the second cause of retention, which has been called spasmodic or irregular contraction of the os uteri—a condition resulting from a too hurried termination of the second stage of labour, when the previous one has been slow, or from the injudicious delivery of the child when the uterus is not acting. It is very questionable, however, if this apparently premature contraction of the os uteri can properly be considered as a spasm or an irregular contraction, as there are strong reasons for believing that it is a natural process; and on careful examination, the parts feel in a normal state, although it has been assumed without the consent or sympathy of the fundus. This will appear the less remarkable when we recollect that in the cervix and os, although "composed in part of tissues similar to those of the body" of the uterus, the arrangement is completely different; and that during the second stage of labour the os is in a comparatively passive state, and when no longer distended by the presence of the child, it is liable to contract irrespectively of the state of the fundus. This independent action of the cervix and os is obviously a wise provision for the purpose of preventing a more serious occurrence, namely the inversion of the uterus. When we pass one hand into the uterus in such circumstances, therefore, we do not overcome a morbid spasm, as is generally supposed, but simply re-dilate the naturally contracted os and cervix.

This opinion is justified by the fact that in removing the placenta when in the vagina, the membranes often seem to be grasped, and in some instances so firmly, that if they are not sufficiently twisted and gently extracted they are liable to be left in the uterus. Also, in passing the hand into the uterus in the case of hemorrhage, we often meet with a difficulty from the diminished size of the os, while the vagina is in a relaxed and fully developed state, showing that it is longer in being contracted after labour; hence, it is reasonable to suppose that it forms the lower chamber in the following cause of retention.

The third cause of retention of the placenta is what is called in books "The hour-glass Contraction," which is erroneously supposed to be situated in the body of the uterus, and to divide it into two equal "chambers or cavities." It is said to be of very rare occurrence.[1] This I readily believe, as it is very questionable if any such contraction ever exists, it is so contrary to the anatomy and physiological action of the part. I am therefore disposed to entertain the views on this subject which have been so admirably expressed by Sir John Chapman of Windsor.[2] Yet, since his time, this form of irregular contraction has been frequently illustrated in books, along with many others equally fanciful. Denman even goes so far as to say that in "those cases in which the placenta is retained, irregular action of the uterus is in some cases evident for several weeks before the time of labour;" and the "forms which the uterus may assume in consequence of this irregular action are innumerable." [3]

The fourth cause of retention of the placenta is its adhesion to the walls of the uterus, which is in some instances so intimate that it is impossible to separate them without "direct injury to the uterus,"[4] or to determine the boundary line between them on dissection.[5] This alarming state may be combined with contraction of the os, which renders the case more complicated and dangerous.[6] Ramsbotham asserts that this adhesion is more common in the lower classes, as they are exposed to those accidents during pregnancy which terminate in inflammation and agglutination of the placenta and uterus; and this morbid adhesion may be suspected, if "the uterus continues to act moderately, and if, on putting the funis rather on the stretch, and then letting it suddenly go, it springs up with a sort of jerk;" but the only certain proof of the existence of this accident is to be obtained by introducing the hand into the uterus. Some women seem to have a constitutional tendency to such morbid adhesions, without any apparent cause or symptom during pregnancy, of which the subject of the first of the following cases is a clear example.

From these facts, the importance of the judicious management of the third stage of labour must be obvious to every one who turns his attention to the subject; yet it has given rise to great diversity of treatment, and in regard to it the most dangerous precepts have been laid down by those of high authority. In former times there were two distinct and opposite rules inculcated: the one existed from the time of Hippocrates, and recommended that the placenta should be left until expelled by the natural efforts of the womb; the other, for which we are indebted to Celsus, suggested that the hand should be introduced into the uterus immediately after the birth of the child, for the purpose of removing the placenta. Each of these practices had their advocates, until the time of Smellie, who states that he "swam with the stream of general practice," which was in favour of the suggestion of Celsus, "until, finding by repeated observation that violence ought not to be done to nature" by hastily extracting the placenta, he resolved to adopt a new method, which is very much what is pursued in the present day in ordinary cases, with the exception that in some cases he was in the habit of inducing vomiting. His contemporary Dr Hunter, however, advocated the practice of Hippocrates, until some fatal cases induced him to change his treatment. Denman was rather inconsistent in his practice, for while he was in the habit of keeping his patients "in the erect position till the waters broke spontaneously, and the child was on the point of being born," with the view of hastening and facilitating the expulsion of the placenta, he informs us that it may be retained for many hours, or even days, without doing any mischief, "unless there be an existing hæmorrhage;" and in conformity with this opinion, he laid down the rule which has been adopted by many others, "that if the placenta was not expelled at the end of four hours from the birth of the child, it is generally wise to determine upon extracting it; and the accoucheur should not leave the room until it is expelled.[1] He had a singular dread of attempting to introduce the hand into the uterus until the os was amply dilated, lest it might "clip round the wrist." After the hand was once introduced, he advised that it should not be withdrawn until the placenta was separated, when it was to be grasped, and then slowly extracted until it reached the vagina, where it was to be "suffered to remain for several hours, to avoid the hazard of subsequent hæmorrhage.[2] This is a most objectionable practice, and could only tend to give uneasiness and alarm to the patient; for, after putting her to the pain of introducing the hand into the uterus and partly withdrawing the placenta, she would not readily be persuaded that everything was right until the afterbirth was delivered; while its being left in the vagina could be of no possible benefit.

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[1] Ramsbotham, Obstetric Medicine, p. 524.

[2] The London Medical Gazette, 12th June 1830. [3] *Op. cit.*, vol. 2, p. 328.

[4] Denman, *op. cit.*, p. 342. [5] Ramsbotham, *op. cit.*, p. 529.

[6] Burns' Midwifery, p. 523.

Burns laid down the rule that the accoucheur should "never leave the room until the placenta is expelled; and if it is not expelled within an hour, it ought to be extracted."[3]

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[1] *Op. cit.*, p. 334.

[2] *Op. cit.*, 341.

[3] A Practical Treatise on Midwifery, p. 182.

Collins, although he does not go so far as Denman, considers that "having waited for about two hours, and employed all the gentle means ordinarily recommended for promoting the expulsion of the placenta, without effect, we ought to give assistance."[1]

Notwithstanding the regulations laid down by those eminent men, as well as by many others who might have been referred to, it is a much better and safer practice to relieve the patient from all anxiety, and make her comfortable with as little delay as possible. This cannot be accomplished so long as the placenta is undelivered; therefore, after allowing from a quarter of an hour to twenty minutes for the uterus to recruit its energies, after the birth of the child, if it then shows no indication of acting, means ought to be adopted for removing the placenta. Slight tension should be made on the cord, and if the placenta does not advance, then the finger of the other hand should be passed into the vagina, along the cord, and if it can reach its insertion, the placenta should be removed immediately; but if it is still within the uterus, we must ascertain the cause of its detention, and, if there is merely inertia of the uterus, endeavour to overcome it by means of stimulants, the ergot, and by gentle friction and pressure on the abdomen. Should these means not induce expulsion in a short time, then the hand should be introduced into the uterus, and the placenta extracted at once. If the os is strongly contracted, the fingers are gently to be insinuated into it, and its contraction overcome, which is easily done if there has been much hæmorrhage; but if this has not been the case, and the os resists the introduction of the fingers, recourse must be had to opium. Bedford, however, strongly recommends the employment of belladonna in such circumstances, which, he says, has a very remarkable effect in dilating the os uteri. But it is very questionable if this hazardous application should be preferred to the employment of opium or chloroform.

When the placenta is adherent, the practice of grasping it and endeavouring to separate it from the uterus, by drawing the fingers towards the palm of the hand, is preferable to inserting them between the two organs, and forcing them separate as by a wedge, as has been recommended by some accoucheurs.

Cases of Retained Placenta.

Mrs C., a large woman, being 13 stone weight, was delivered on the 30th October 1852, of twins, being her sixteenth and seventeenth child, although she had been married only sixteen years. Her labour came on yesterday morning, and when I arrived this forenoon at 11 A.M., the first stage of labour was nearly completed; but the pains were lingering and inefficient until 4 P.M., when she had an enema, without any good effect, and I left her; but no sooner had I done so, than smart pains came on, and she was delivered of a daughter, apparently in the eighth month. I was unable to make out any presentation when I left her, but I suspected that either the back or the breast was the presenting part, and all the information I could get from the nurse was that the head came last when the child was born. Finding the abdomen still large on my return, and that the placenta was retained, I passed my hand into the vagina, and found that there was another child; but in consequence of its being thrown forward by the pendulous position of the mother's abdomen, I could not ascertain the presentation. The pains having entirely ceased, I left, in order to see another patient. On my return, I found matters just as I left them. I therefore suggested a consultation with Dr Graham Weir, who agreed with me in the propriety of delivering the child artificially. On introducing my hand into the uterus, I found the back was the presenting part. I brought down both feet, and delivered a small male child. The placenta adhered, which had been the case in all her former confinements. Although the womb contracted well after the removal of the placenta, considerable flooding came on about three-quarters of an hour after its delivery; but it soon ceased, and the syncope was overcome on the womb contracting. It is not uninteresting to mention that this patient dreamed, the night previous to her confinement, that she was delivered of twins—a boy and a girl.

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[1] A Practical Treatise on Midwifery, p. 178.

In delivering the boy, the feet slipped several times from the hand, in consequence of their unctuous state from the quantity of sebaceous matter. This led to their being a good deal bruised, and on examining him some hours after birth, I found him covered with erysipelatous eruption, especially on his hands and feet, and there were blobs of serum suspended from each toe of the left foot. He had murmured incessantly since his birth. I ordered him to have two drops of the tincture of the muriate of iron every two hours, in a little sugar and water, which had a remarkable effect in soothing him, and he lay quiet. In the evening there was less swelling and redness of his hands.

31*st October*.—The boy's hands and feet more swollen and red, in consequence, apparently, of the nurse having neglected to give the medicine, which was ordered to be commenced again, but with little hope of doing good, as the child had some difficulty in swallowing; and its face was black and swollen, and its eyes closed.

1*st November*.—The boy still alive, but very weak. The swelling and redness of the feet diminished, and the serous blobs at the points of the toes, which are of a dark blue colour, have begun to shrink. The only thing he has been able to swallow has been his medicine. He has made water repeatedly, but his bowels are constipated. He soon died.

22*d November* 1854.—Mrs C. having had repeated threatenings of miscarriage during the last month, which have been apparently checked by her being kept in the recumbent posture, with restricted diet, and her bowels regulated by mild medicines, was again delivered of twins this evening. The circumstances which led to the suspicion that premature labour was impending, was sudden discharges of water, which, on one occasion, was tinged with blood; but these discharges were not accompanied with labour pains, and they took place when she was asleep, or lying on the sofa. This induced me to believe that they were simply discharges of urine occasioned by the movements of the fœtus. This opinion was considerably strengthened on examination per vaginam, by there being no development of the os uteri, although the vagina was much relaxed. The abdomen was very pendulous, and the womb anteverted.

When I first saw her this morning, she had just had an attack similar to what is described above, accompanied with much flatulency. She had no regular labour pains, but she was very nervous. She was ordered some assafœtida pills, and a mixture containing camphor and aqua ammoniæ acetatis, in the hopes of relieving the flatulency and nervous agitation. But, in the course of four hours, I was again hurriedly sent for, and I found on my arrival that one child was born. It was very weak, and apparently not beyond the sixth month. The eyes were closed. The placenta was in the vagina; but I found there was another child. After waiting for sometime in hopes that labour pains would return, I introduced my hand, ruptured the membranes, and delivered the child, which seemed more advanced than the other, the left eye being open. Neither of the children lived above two hours and a half. The uterus showing no signs of contracting, I introduced my hand, and found the placenta firmly adhering, and with great difficulty was enabled to get the greater part of it away; but it was quite impossible to remove the whole, which caused me much anxiety, and I requested to have the opinion of Dr Graham Weir, who, after endeavouring to remove the remaining portion, agreed with me, that there was nothing else to be done,—finding that neither repeated doses of the ergot nor the stimulus of the hand in the uterus had any effect,—than to leave the case to nature, and carefully to watch the progress of the symptoms.

27*th November.*—No bad symptoms until this morning, when excessive flooding came on, with great faintness. Several doses of the ergot were given, then three-grain pills of gallic acid were given every two hours, along with the application of cold to the vulva, and half a glass of port-wine every half hour so long as the faintness continued. This treatment had the desired effect, as the discharge ceased, and the womb became hard and contracted.

In the course of a few hours her strength was so much recruited that she could turn in bed; her face was less cadaverous, and her pulse was 96, of moderate fulness. She stated that she had had a miscarriage in the commencement of the year, and that she had last menstruated in the end of May. Including her miscarriages, she had twenty-one children.

28*th November.*—She felt better, and had had no return of flooding, at the morning visit. To have thirty drops of the tincture of the muriate of iron in water every three hours.

In the early part of the evening she was seized with great faintness, and although she was unconscious of having any after-pains, a large clot was expelled. She had some more of the gallic acid pills, and as the discharge seemed to be entirely stopped, I was in hopes all would go on well; but when I called at 11 P.M., I found her lying in a state of great exhaustion, her skin cold, and covered with a clammy perspiration, with full pulse. I feared that hæmorrhage had again taken place. I was informed, however, that this state had been produced by a fright occasioned by the outrageous conduct of one of her servants. I gave her a glass of port-wine, which produced dark bilious vomiting; but this soon ceased.

29*th November.*—No return of vomiting nor flooding. To have fifteen drops of the muriated tincture of iron twice a day, and to take claret in place of port-wine, and a piece of partridge for dinner.

30*th November.*—Doing well.

3*d December.*—Pulse 84, full and soft. Some small clots have come away; but she has no pain and no discharge. She soon recovered her usual health.

14*th December* 1856.—I was sent for at 10 A.M. to see Mrs C., who was in labour of her twenty-second child. On examination I found that the waters had escaped, and that the foot was presenting. The child was soon delivered, and it seemed to be about the seventh month, and very small and feeble. The placenta adhered, as had been the case in all her confinements except the first. It was readily separated, however, and there was no hæmorrhage. I remained with her an hour, and as everything seemed to be going on satisfactorily, with the exception that her pulse was 120, I left her; but soon after bilious vomiting came on, and she had a severe rigor, for which she took some brandy. On my return, about three quarters of an hour afterwards, she again vomited some bilious matter. On examining there was a large clot occupying the os uteri. She made a good recovery. I saw her a short time ago, when she was in tolerable health, and very juvenile in appearance for a person who had had so many severe confinements.

Mary Fairly, aged 25, a primipara, was admitted to the Maternity on 13th September. She was in good health, of a quiet and taciturn disposition, and rather depressed in spirits on account of her condition.

5*th October*, 9·30 P.M.—Was found sitting in bed suffering from slight labour pains, which she supposed arose from flatulency, or from having taken a dose of castor-oil. At 11 P.M. she was examined, and the os uteri was fully dilated, and the child presenting in the first position. The waters had escaped without her being aware. Forcing pains commenced soon after, and labour progressed favourably, although slowly, until 4 P.M., when the caput succedanum was almost beyond the vulva at each pain. At 8·30 A.M. the head was much in the same position, being retained apparently by the rigidity of the perinæum and a loaded state of the rectum. Chloroform was administered, but it was found to stop the uterine contractions. Antimony was then given with apparent effect; the head began to advance more rapidly, and the child was born at 9.45 A.M. in a state of asphyxia, but was soon resuscitated by means of hot and cold baths, friction, etc.

Dr Bell, who had been previously sent for, arrived twenty minutes after the child was born. He found the abdomen much distended, and the uterus high up quite relaxed, and the placenta retained. On making pressure, with the view of inducing the uterus to contract, a large quantity of clots and liquid blood were expelled. The flooding continuing, and the patient becoming faint, a dose of the powdered ergot was given without effect. Dr Bell, therefore, introduced his hand, and found the placenta retained by the contraction of the os and cervix uteri, which was the more easily overcome in consequence of the exhausted state of the patient, and the placenta extracted. Soon after which the uterus contracted, and the flooding ceased.

From the 6th to the 10th, the pulse ranged from 120 to 130, and she complained of giddiness and weakness, want of appetite, and there was no secretion of milk. During this time she had soothing draughts of camphor and the solution of the acetate of ammonia, and mild aperients.

10*th.*—After taking some broth she complained of headache and nausea. The respiration was oppressed, the pulse accelerated, abdomen tympanitic, lochia normal in quantity.

11*th.*—Felt as if racked on an engine all night. Had a slight rigor, and was seized with pain in left leg. Spirits much depressed. Had her leg rubbed with camphorated oil.

13*th.*—Pulse 130. Mind wandering, and she was very suspicious of all around her. No secretion of milk. Lochia of normal quantity, but resembled pus, and was very fœtid. Retention of urine. Skin moist and clammy. Was unable to attend to her child.

To have the vagina and womb syringed twice a day. The urine to be drawn off. It was dark-coloured, with considerable sediment, and contained sugar.

To have twenty drops of the tincture of the muriate of iron three times a day.

This had the effect of promoting the passage of the urine, and the catheter did not require to be used again.

14*th.*—Is quieter and more coherent, although very much exhausted. Has no appetite, and refuses food. To have wine, and to continue the tincture.

15*th.*—Appears to be sinking. Great prostration; lies on her back as if sunk in the bedclothes. Face pale and shrunk. Tongue dry and covered with brown fur in the centre. Pulse rapid, weak, and fluttering. Twitchings of the fingers and hand. Lochia nearly ceased. Unable to move, and passes urine involuntarily. Bowels open.

To have thirty drops of the tinct. of the muriate of iron every three hours, and nourishing diet and port-wine.

16*th*.—Passed a quiet night. Pulse improved. Feels better. Lochia more profuse. The vagina to be syringed with a solution of Condy's fluid three times a day. To have wine ℥iv. in the course of the day, and the tincture of iron to be reduced to thirty drops three times a day.

17*th*.—Still improving. Pulse 110. The urine passed involuntarily, and produces a scorching sensation. Lochia have become like pus, but have no smell. The mental faculties quite restored. To have castor-oil, and to continue the medicine and wine.

25*th*.—Has gradually improved since last report, and was dismissed to-day at her own request.

ARTICLE IV.—*Sequel to the Case of Aphasia after Typhus Fever, published by the late Dr Scoresby-Jackson in this Journal for January* 1867.

[The further progress of cases of this kind is, unfortunately, very seldom recorded, so that experience is wanting to furnish a basis for prognosis in similar instances. On this account, besides the intrinsic value of the case itself, the following communication in regard to the present state of Mr A. B. X., which has been sent by his physician, will be perused with interest.]

"I have been putting our young friend through a sort of examination, with the view of giving you as correct an account as possible of his present state.

"His general health is pretty good; but he is somewhat liable to headache, especially when his bowels are constipated, which they are disposed to be. He can walk a good many miles, and can grasp as firmly with the right as with the left hand; but he says he still feels the right side, or, at least, the right leg, weaker than the other one,—not, he explains, that it is sooner tired, but that, if he were to fall, it would be toward that side. I do not see that he halts or leans over perceptibly in walking.

"His complexion is clear and rather ruddy, and his expression intelligent and amiable,—perhaps, however, there is still a little appearance of relaxation about the mouth. He can move the tongue to right or left as desired; but, when told to put it straight out, he gives it a *very* slight inclination to the right; moreover, there is an appearance as of a want of correlation in the movements of the tongue, and he says himself that he has difficulty in making it do exactly as he wishes.

"When I put him through the alphabet he pronounced the name of every letter distinctly enough; but he has a good deal of difficulty about the hard G, and his D is apt to become a T. Thus, he pronounces *gad* nearly as *cat*, *gag* as *cak*, *gage* as *case*. *Jog* seems impossible to him; he pronounced it first *chok*, then *tok; King George*, *Kink Chorj*. The guttural sound in *Loch*, as Loch Fyne, seems pretty easy to him. L is rather difficult: I tried him with *Lollards*, the results were *Lorars* and *Lorrads;* but he pointed out that he could say *Bleadon*, the name of a place which he visits occasionally, and which, a few months ago, he called *Bereadon*. *Truth* was pronounced *trūst*, *truse*, *truth*—correctly the third time; *thing* was called *think*.

"These difficulties, and a great part of his difficulty in making himself understood, seem to depend on want of control over the tongue, etc. The other kind of imperfection is illustrated in the paper marked A, which contains a specimen of his handwriting. Paragraph 1 was written at my dictation slowly, but correctly, except that he omitted one *s*,—the punctuation was his own. Paragraph 2 was of his own composition; but I had suggested to him that he might say something about his visit to the Infirmary, and about his drawing lessons. A new ward is being built at the Infirmary just now. He forgot the name of the School of Art, and asked me 'the name of the building opposite' so and so. He pointed to the word *myself* after writing it, and indicated that that was not exactly right."

A.

1. I wish Dr S —— to see the extent of my improvement, in writing. I am now writing at Dr B ——'s dictation, I can write prescription quite well, and without any mistake, but I am still slower about it, than I was before my illness.

2. Yesterday I went down at the Infirmary, Dr B —— was round the wards. They built another ward, but did not get on so fast as I should imagine. The School of Art have taught myself drawing, t'was very slow, since.

ARTICLE V.—*Case of Poisoning with the Rhus Toxicodendron.* By WILLIAM R. SANDERS, M.D., F.R.C.P.E., Physician to the Royal Infirmary of Edinburgh.

THE following case is interesting solely or chiefly from its rarity. The symptoms produced by the juice of the Rhus toxicodendron have been described in works on Toxicology or Materia Medica, but they have not, so far as I know, been made the subject of observation in medical practice in this country. In the instance to be related, the symptoms observed, although they appeared alarming to the patient, presented really no gravity.

Peter Doig, aged 26, a gardener, was admitted into the Royal Infirmary, under my care, on 25th July 1867. He was a strongly built man, of middle stature, and had evidently been in the enjoyment of robust health. He stated that, after the diseases of childhood, he had never suffered from any illness, with the exception of an attack of gastric fever, at the age of 20, from which he perfectly recovered. The symptoms, on account of which he now sought medical treatment, consisted chiefly of an erysipelatous eruption on certain parts of the skin, of which he gave the following history:—About three weeks before admission—viz., on July 6th,—he had, in company with other gardeners, in Messrs Lawson's nursery, been employed in gathering the shoots of the Rhus toxicodendron for the purpose of supplying a homœopathic druggist in town. Doig continued at this occupation for about two and a half hours on that afternoon. Instead of pulling up the whole plant, as the other workmen did, Doig plucked off the young shoots, and, in so doing, his hand came into frequent contact with the juice of the plant. This juice, which was white and milky when fresh, became dark on exposure, and concreted on the palms and wrists, forming dark scales, which adhered so closely as to be with difficulty removed by rubbing off the superficial layer of cuticle. At the time, no inconvenience whatever was felt; but, four days afterwards, Doig noticed two blisters, each about the size of a threepenny piece, on the flexor surface of the right wrist. On the top of each blister there remained adherent a portion of the black concreted juice, and there was some redness around the vesications, but no pain. Four or five days subsequently the other wrist became similarly affected, and about the same time the redness began to spread slowly up both forearms. Nothing further was noticed till the 23d, when he was obliged to give up work in consequence of the swelling and stiffness of the forearms, accompanied by severe numbing and stinging pain, to relieve which he had rolled his arms in cold moist cloths. This was seventeen days after exposure to the poisonous juice. On the 24th, being now greatly alarmed at the progress of the eruption, he applied to the Infirmary, where he was first admitted into the surgical wards, under Dr Gillespie, who next day transferred him under my charge.

On examining the patient, I found the skin on the flexor surface of both forearms swollen, and of a florid red colour, like that of erysipelas; and the red surface was covered with small transparent vesicles, each about the size of a pin's head, closely set together. The vesicles resembled those of eczema, or the minute inflammatory vesications produced by the application of turpentine. Both forearms were considerably swollen, and felt stiff to the patient. Some of the black spots, formed by the dried acrid juice, were still seen on the palms of the hands and on the adjoining parts of the wrists. The skin of the (upper) arms was natural. The face, though less affected than the forearms, and not vesicated, was swollen and erythematous, the eyelids being puffy and partially closed. The trunk of the body was untouched, but the skin of the penis and scrotum was red, œdematous, and painful, and there were scattered spots of inflamed and slightly elevated skin on the inner surface of both thighs. The inflamed portions of skin were the seat of pain, sometimes of a numbing character, sometimes stinging like the irritation of nettles. The pain was worse at night, owing to the heat, but it was not severe anywhere, except in the forearms.

It was remarkable that these symptoms were unaccompanied by constitutional disturbance; the pulse was quiet and the appetite good. The tongue was somewhat furred and dry, but this was owing to constipation, which was easily relieved. During the course of the following day (26th), the red patches extended up the arms, and also down the thighs as far as the knees, while some scattered spots appeared over the pubes. Next day (27th), the swelling and redness of both the face and arms were diminishing, and the vesicles on the forearms were drying up into scabs, but the erythematous eruption on the thighs continued to spread downwards to the legs, and upwards on the trunk of the body. On the 29th, at noon, the abdomen was found covered with irregularly-shaped patches of inflamed skin, which had extended from the pubes upwards, as far as the hypochondria. At the margins of the large patches there were numerous detached small reddish spots, like the eruption at the outset of measles, the larger patches resembling the continuous rash of scarlet fever. On the 31st, the redness had extended to the back, while anteriorly the skin, from the pubes to the clavicles, was marked with inflamed patches and spots, the region of the sternum alone being unaffected. The legs were almost entirely covered with eruption. There was no vesication on any of these parts. But while the eruption was thus spreading on the lower half of the body, the upper half was recovering. Thus, the forearms were now nearly well; the redness and swelling were gone, only a few small vesicles remaining on the backs of the hands and between the fingers. The face had nearly the natural appearance, very slight desquammation being observable.

At evening visit, Doig complained of stiffness and rheumatic pains in the knees and elbows, but no swelling of the joints was present. These pains were ascribed by the patient to sitting up in a cold sideroom, and they were quite gone next day. Doig still presented no constitutional disturbance; with the exception of a tendency to constipation, all the functions were normal. On the 1st August, the eruption ceased to spread on the trunk, and soon began to fade. It had now completely disappeared on the face, arms, scrotum, and other parts first affected. There was very slight desquammation, the skin returning nearly to its natural state. On 2d August, Doig left to go to the country, having been eight days in the Infirmary.[1]

I saw him again about three months afterwards, when he told me, that after going to the country from the Infirmary, the eruption, which had almost entirely subsided, re-appeared on the trunk of the body; but it lasted only for a few days, and then completely and finally disappeared.

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[1] The case is abridged from the report by Dr Ramsay, then acting as House Physician in my wards.

It is unnecessary to detail specially the treatment, which consisted of moderate purgatives and cooling diaphoretics internally, and of anodyne and evaporating lotions externally. The case required only ordinary care, never presenting at any time an appearance of danger. The patient, indeed, was at first in a state of extreme alarm; the fact of being poisoned, and the persistent progress of the symptoms filling his mind with undefined terror. But on being confidently assured that his symptoms were free from danger, he had the good sense to believe the statement, and the relief of his fears was perhaps the most important part of the treatment.

My purpose being merely to record the symptoms of a kind of case which, though rare at present, might become common if the plant were more cultivated, I shall add only a few remarks by way of commentary.

The symptoms just recorded correspond with those described by authors on Toxicology.[1] The Rhus toxicodendron, poison ivy, poisonous sumach, or trailing poison oak, belongs to the Terebinthaceæ, or turpentine family of plants. The white juice of the Rhus contains a volatile, acrid, and narcotic principle, to which its active properties are due. This acrid principle has not been much nor very successfully investigated. But from older observations it appears that when the juice is applied externally, or when the body is exposed to the emanations of the plant during night or in the shade, itching, redness, and a kind of erysipelatous inflammation of the face, hands, or other exposed parts is produced. These effects are followed by vesication and desquammation of the cuticle. It is remarked that all persons are not subject to its influence, some delicacy of skin or other susceptibility being required to produce the effect. When taken internally, the leaves of the plant are said to cause diaphoresis, diuresis, and relaxation of the bowels; they produce also a stimulant effect upon the nerves. On account of the last-named property, the plant has been administered in cases of paralysis, and is alleged to have produced a return of the sensibility and motion in the paralyzed parts, as evidenced by a feeling of burning and pricking, accompanied by muscular twitchings. "Large doses occasion pain in the stomach, nausea, vomiting, giddiness, stupefaction, and an inflammatory swelling of the paralyzed parts" (Pereira). In animals, according to Orfila, the watery extract "when internally administered or applied to the cellular texture, produces a local irritation, followed by an inflammation more or less intense, and it exerts a stupefying action on the nervous system after being absorbed." Death has been caused by the poison in experiments on animals, but I am not aware of any fatal cases in man. One circumstance deserving of notice in Doig's case is, that he alone was affected out of a number of persons employed in gathering the poison ivy. This might be ascribed to a special susceptibility of skin above referred to, but his own account suggested a more obvious cause in the fact, that while others gathered the entire plant, he alone plucked the young shoots, and in breaking off the twigs set free the juice, which came in abundant contact with his hands and forearms.

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[1] Professor Christison, who saw the case, kindly furnished me with references to various authorities. The results obtained by Kalm, Fontana, Dufresnoy, Krüger, Alderson, who introduced the plant into British medical practice in 1793, Van Mons, Lavini, and others, I have found in "Wibmer's Wirkung der Arzneirmittel u. gifte," bd. iii., 403, *et seq.*, München, 1837, lent me by Dr Christison. Some of these researches refer to the R. radicans, but this appears to be identical in properties with the R. toxicodendron. The principal results are given in the works on Materia Medica of Christison and Pareira. Dr Fraser has called my attention to recent researches by Dr Clarus of Vienna, who found no effects on man or rabbits from the leaves or juice, and ascribes the symptoms said to occur in gardeners, if not exaggerated, to gaseous exhalation. Winter relates, among others, a case of vesication of the whole arm, in consequence of contact with a section of root, which began after two days, was intolerably painful, and lasted four weeks. Maisch refers the acrid principle to a volatile acid, Acidum toxicodendricum. Formic acid is also present in the juice.—*Supplement to Husemann's Toxicologie, Berlin*, 1867, p. 94.

It is further to be observed, that the symptoms in Doig were entirely confined to the integument: no constitutional reaction occurred,—not even so much as the amount of inflamed skin might have rendered probable. And, in particular, there was no affection of the nervous system, such as giddiness, stupor, or nervous twitchings. The burning and stinging sensations were confined to the affected portions of the skin, and were evidently part of the local affection. The question nevertheless arises, Were the symptoms observed due to the local contact of the poison, or were they caused by the absorption of an irritant principle into the blood? The different parts of the skin affected, and the slow and somewhat irregular course of the eruption, together with the persistence of the symptoms, seemed to indicate that the poison had been absorbed. Nevertheless, I think the chief part of the symptoms can be readily accounted for by the mere local irritant application; for the hands and forearms, which were first and most affected, were the situations chiefly exposed to contact with the juice. The points next attacked—the face, scrotum, and thighs,—are plainly the parts of the skin most liable to be touched by the hands while still retaining some of the irritant juice upon them. And although the erythematous redness spread gradually over the body, it was always observed to be propagated by continuity from parts already inflamed. In Doig the irritant was long of taking effect, for, on the wrists, which were most acted upon by the juice, the inflammatory reaction did not occur till after a considerable interval; and the face, etc., which had received a small dose of the irritant principle, were only subsequently and less severely affected. The only circumstance requiring special explanation is the spreading of the eruption over the trunk of the body at a later period of the case, after all sources of irritation were removed. This, it appears to me, can only be explained on the supposition that some of the poison had at length been absorbed into the blood, or at least into the tissues of the skin,—and this agrees with the observations of toxicologists, which have shown that the cutaneous symptoms may arise from the internal administration of the poison, or, what is the same, from its absorption in the blood, as well as from its external local application.

ARTICLE VI.—*The Care and Treatment of the Insane Poor, with special reference to the Insane in Private Dwellings.*[1] By ARTHUR MITCHELL, M.A. and M.D., F.R.S.E, etc., Deputy Commissioner in Lunacy for Scotland.

IN his address as President of the Medico-Psychological Association, Dr Robertson discusses the various modes of making public provision for the insane poor; and one of the three modes which he recommends is that of disposing of a certain number of them in private dwellings.

The address recognises the principle that the insane poor are not to be provided for in one inflexible way. Provision is to be made for them according to their requirements, and it is admitted that these vary. The management of insanity is not to depend on its name, but is to be determined by the varying needs of those labouring under it. *Asylums*, *poorhouses*, and *private dwellings* are accordingly sanctioned and recommended; and among them Dr Robertson says that the whole of the insane poor may be distributed "with due consideration of all their claims and requirements."

Without formal acknowledgment, perhaps, but still in fact, the principle is a guide of action with all physicians engaged in the treatment of insanity. One patient, regarding whom advice has been asked, is removed at once to an asylum, another is sent to travel, another is taken from home and boarded with strangers, another is left among his friends, and so on—the counsel given to different patients being regulated by differences in their condition and circumstances. The very statutory certificate on which a lunatic is placed in an asylum recognises the same principle, since it is necessary that it shall not only certify the person to be of unsound mind, but also that he or she is a fit and proper object for care and treatment in an asylum. Our whole laws, indeed (for England and Scotland both, though not equally), rest on the idea that some of the insane may properly be left out of asylums, and they accordingly make provision, more or less effective, for the protection of such patients.

In his address, Dr Robertson makes frequent allusion to "harmless and incurable lunatics" and to the "chronic and harmless stages" of mental disease, and points out clearly that asylums are only needed for the "majority" of the insane. One-fourth of their whole number, he thinks, may be provided for in poorhouses, "with due consideration of all their claims and requirements," and 15 per cent. more in private dwellings. "I am very far from asserting the opinion," he says, "that all the insane poor without exception ought to be treated in the county asylum or in the workhouse. A certain proportion might, with increased enjoyment of life, be restored to their own families;" and he elsewhere tells us that there are patients now in asylums who "might certainly, under proper restrictions, be restored" to their homes.

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[1] This abridgment and *recast*, by the author, of an article which appeared in the January number of the *Journal of Mental Science* is given here, because it is thought that the views discussed in it should be made known, and will prove interesting to the profession generally; and also because the subject is one to which great and increasing attention has of late been directed.—ED.

Something of the nature of the "*increased enjoyment of life*" which is here referred to, we learn from a statement he makes to the effect that, with a certain class of patients, "mixing with persons of sound mind is a comfort much appreciated, as also the greater freedom, the facility of visiting old friends and associations, and such like."

But it would not simply be a certain number of the insane who would derive happiness from the operation of these views, for he tells us that "great comfort would result to many families in having their afflicted loved ones again with them." Both the patients and the friends of the patients would thus be benefited; and this is not yet all, for the advantage would extend to the asylums themselves. By adopting this home treatment, Dr Robertson says that "*the confidence of the poor in the authorities of the asylum would be greatly strengthened.*" This important statement may be appropriately linked to another from the same pen, which tells us that asylum populations "include a large proportion of incurable lunatics, *whose treatment, speaking generally, is a matter of organized system rather than of individual observation.*"

At various times and from various quarters opinions of a character not unlike the foregoing have been expressed in Scotland. That a certain number of the insane poor, carefully selected and under proper restrictions, may be satisfactorily provided for in private dwellings, is the opinion held and acted on by the Scotch Board, and it will be found to rest on considerations very much like those which lead Dr Robertson to the same conclusion. Yet, with much inconsistency, everything that has been done and said on this matter by the Scotch Board receives from him a wholesale condemnation; and as the question is an important one, and is daily becoming more so, it seems to me that it will prove of practical utility to show the real scope and nature of what has been called the Scotch system.

In my last general report to the Board of Lunacy, I stated, as the result of experience, that the *majority* of single patients should consist of "the fatuous and idiotic." Dr Robertson says that the very "existence of the system is condemned by this official admission." It certainly is a fact that a considerable majority of the insane in private dwellings in Scotland may be tabulated under the headings of the demented and idiotic; but is not the same thing true of the 6638 single patients in England, and is it not true, also, of the inmates of poorhouses? With reference to the 6638 single patients in England, we are plainly told that "they are chiefly cases of idiocy and dementia," and with reference to the lunatic inmates of the ordinary wards of poorhouses, that they are "idiotic and demented." Nevertheless, when Dr Robertson speaks about Scotland, he writes as follows:—"I would just ask you to recall the demented and fatuous inmates of one of our county asylums, with their depraved habits and many wants, and to remember the daily, hourly care required to keep them decently clean, and to retain some faint image of humanity and civilisation about them, in order to realize what their condition must be when all the costly remedial agents of the asylum are once withdrawn." This, of course, is true of a certain number of the idiotic and demented; but of some of them, is it not a fact that little, or next to nothing, of it is true—there being many degrees and many forms of dementia and idiocy, some giving great, and others but little difficulty in their management?

It is not proposed to interfere with the present 6638 single patients in England, unless by sending some patients out of asylums, and so adding to their number. These single patients, therefore, will still consist "chiefly of cases of idiocy and dementia."

Objection is taken to the fact that we have 28·5 per cent. of our pauper lunatics in Scotland in private dwellings, instead of 15 per cent., which is the proportion in England.

The history of the Scotch number is briefly this:—In 1855 there were 1363 single pauper patients, or 32 per cent. of all pauper lunatics; in 1859, under the operation of the Lunacy Law, the number had risen to 1877, being an increase of more than 500; in 1866 the number had fallen to 1568, being still 200 above the original number, and constituting 28·5 per cent. of all pauper lunatics. Of the original 1363, and the additional 514 whom the inquiries of the Board brought to light, a considerable number were improperly kept in private dwellings, and were removed to asylums at the instance of the Board.

Dr Robertson gives the proportion for England at 15 per cent. He gives no reason whatever for adopting this proportion except that it is the one which presently exists.

But the operation of such a system as that under discussion is not to be regulated by a pre-arranged percentage. On the contrary, the percentage must be determined by the number of suitable cases, and the power of providing for them satisfactorily. All those patients whose well-being and happiness will be increased by being at home, for whom a comfortable and safe provision can be found there, and whom asylums cannot benefit, should be left at home or should be sent there, whether their proportion to the whole number of the pauper insane be 10, or 15, or 20 per cent. And in practice it will always be found that the percentage is one thing in one country and another in another; one thing even in one parish and another in another; and such differences will be proper ones, arising sometimes from accidental causes, and sometimes from causes of more fixity or permanence, such, for instance, as may be involved in the habits or in the circumstances of the people.

I have before me the published report of a country parish, liable for the maintenance of 18 lunatics, who are as 1 to 232 of the population. Of these patients 9 are in asylums and 9 in private dwellings, that is, 50 instead of 15 per cent. A variety of circumstances combine to bring this about and to make things possible in that parish which are impossible in others almost adjoining.[1] For example, 9 of the 18 patients happen to be suitable; relatives and friends of the patients, with comfortable homes, are found willing to be guardians; the Parochial Board takes a liberal view of its obligations in these cases; the medical officer and inspector of the poor are interested in making arrangements to satisfy the Board of Lunacy; and the circumstances and occupations of the people of the parish are favourable. All these and other things combine to give the result I have stated, but if any one or two of them were changed, we should immediately have a different result, reducing the proportion, perhaps, even below 15 per cent.

This same parish furnishes in another way an illustration of how widely the operation of the system may be influenced by local and other causes. One of the nine patients left at home was at first regarded as of doutful suitability, but a well-considered and liberal arrangement for her comfort and safety was made by the parish, and for many years she has done well. In other words, the very range of suitability seems capable of being widened by good management.

As yet, indeed, we know little of the extent to which the system may and should be worked; 15 per cent. may be found generally too high, while, on the other hand, it may be found safe to go as far as 30 per cent., or beyond it. Very much will depend on the spirit and way in which the trial is made. Where failure is desired there will not be much chance of success. It will be very easy, indeed, to secure failure, if that is wanted, for even the most earnest and honest desire after good results will assuredly encounter at the outset a multitude of difficulties and discouragements, which will neither soon nor easily be removed. Hitherto in no country has a full and fair trial been made. More, however, has been done in Scotland than anywhere else, though the difficulties and hindrances there have not been few. Still more, I trust, will yet be done, since already the general result leaves no doubt as to the propriety of allowing a certain number of the insane poor to remain at home, and as to the possibility of increasing their number. It will be difficult, indeed, to say that we have reached the limit so long as there is one "harmless and incurable" patient in an asylum, whose removal to his home would give him "increased enjoyment of life."

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[1] In the very next parish, out of five patients in private dwellings, four were removed by the Board to an asylum.

Dr Robertson is of opinion that, in the superintendence of patients in private dwellings in Scotland, "the amount of the official inspection they receive cannot be worth much." In the Ninth Report of the Scotch Board it is thus described:—

"Patients in private dwellings are visited by a medical man at such intervals as the Board shall determine; and it is directed that at each visit an entry shall be made in a book, kept in the house for the purpose, of the date of each visit, and of the mental and bodily condition in which the lunatic was found. As a rule, these visits are required by the Board once a quarter. By the Poor Law Act it is further provided that every pauper shall, unless under certain exceptional circumstances, be visited at least twice a year by the inspector of poor or his substitute. By the authority of the Board, every patient in a private dwelling is directed to be visited by one of the Commissioners or Deputy-Commissioners once in every year, unless such dwelling shall be situated in Orkney or Shetland, or the Western Islands, when, owing to the difficulty of communication, a biennial visit only is required."

In exceptional cases, the visit of the parochial medical officer may be ordered to be made monthly or fortnightly. In like cases the visits of the Commissioners or Deputy-Commissioners may be two, three, four, or five in a year. In addition to these official visits, there is also that daily inspection which arises from the fact that persons going into or passing the cottage in which the patient resides, see him, and care is generally taken that he is such a one as this may be true of. The selection is made with the knowledge, in the first place, that the Board may order removal to an asylum if the guarantees for proper treatment be not deemed satisfactory, or if it be thought that asylum treatment will promote recovery or improvement; and, in the second place, that after such removal has been ordered, the patient cannot be taken off the poor roll without the Board's consent. This knowledge tends to prevent, on the part of the local authorities, such a selection of patients, or such arrangements for their keeping, as they expect to find condemned, since this would involve them in a twofold trouble. Considerable importance is always attached to the fact that a patient is in such a state as will permit of his going in and out of the cottage in which he resides, at his own pleasure and like any of its other occupants,—so that the inspection of neighbours may thus be secured as frequently as possible. As regards the visits of the Commissioners or Deputy-Commissioners, there are patients for whose safety and comfort provision has been made of so satisfactory and sure a character that the annual visit may be, and occasionally is, omitted, without fear of consequent injury to the patient. There are other patients, again, whom it would be desirable to visit, and who are visited, oftener than once a year.

I think this correctly describes the general nature of the inspection and of the guarantees for the proper care and treatment of the single patients in Scotland. If these be of little, they are still surely of more value, than the inspection which is recommended for the 15 per cent. in England, and which Dr Robertson thus describes:—"The medical practitioners in the district should be employed to make a quarterly medical report to the visitors, and, in exceptional cases, further visitation could be made by the medical officers of the county asylum," and there is also "a periodical visit" by *a relieving officer*, who is to be added to the staff of the asylum.

This relieving officer, or the medical superintendent of the county asylum, is to fix the allowance given to the guardians of the patient for his maintenance, and this allowance is not to exceed the asylum rate. This refers only to those patients who have been in asylums; and his "machinery" *does not in any way reach the great majority of single patients, who have never been there.* I do not know how he would deal with them, or with the cases of those patients who only require aid from the public while in asylums, and whose friends are willing and quite able to support them at home. Between such cases and those in which the whole maintenance of the patient must be provided by the public, wherever he is, there occur all gradations. The dealing with questions of this kind, and the dealing with them arbitrarily, would be a peculiar and difficult part of the duties of an asylum physician. He would have to sift the claims of applicants for parochial aid, and inquire into their circumstances; and he would have to reject these claims, or fix their extent if he thought them established. This would be somewhat difficult, even in the cases of patients who have been in asylums, and who are allowed to return from them to their homes unrecovered, for it is admitted that some patients obtain admission into these asylums by evading the restrictions of the Poor Law. But if difficult with this class of patients, what would it not be with *those who have never been in asylums, and who are always the great majority, and many of whom reside with their friends in districts remote from those to which they are chargeable?*

In Scotland, 70 to 80 per cent. of the pauper patients in private dwellings have not been in any asylum, being in such a state at the time of becoming chargeable as to make that step unnecessary. It is the object of the Board to secure that the single patients consist of a properly-selected class, and that their safety and comfort are reasonably provided for; and that this double object is possible, and has been practically attained, there is evidence in these two facts —1st, that with an average number of between 1600 and 1700 there has been no suicide or dangerous assault during ten years; and, 2d, that the yearly mortality has maintained throughout a remarkably low figure, being at its highest 6·4 per cent., at its lowest 4·5 per cent., and on an average about 5·2 per cent. These two facts cannot fail to carry weight, and they go far of themselves to justify the recommendation of the Board as to the propriety of providing for a certain class of the insane poor in private dwellings.

This recommendation is further strengthened by the consideration, that this disposal of the patients being more economical, and not injurious to them, it is but fair and right to the ratepayers, since the support of the insane poor, while a duty, is also a charity, and is only one of many like duties which we are bound to discharge.

If it were generally known, as well as it is to me from the nature of my duties, how many insane persons there are on the confines of pauperism, whose claim for public aid is rejected, chiefly for the reason that lunacy is already felt to be an oppressive burden, the increase of which is studiously avoided, this consideration would not be lightly passed over, by those at least *whose desire is the greatest good of the greatest number.* After a time such applicants for relief cross the Rubicon, and come unmistakably within the region of pauperism; but relief is then given when it is comparatively useless, and when the disease is fairly confirmed.

It has further to be considered that there are many cases in which the friends of patients refuse relief when offered, because the accepting it would involve removal to an asylum, which they regard as a separation unto death. I know many instances in which extraordinary struggles have been made, painful privations endured, and cruel restraints imposed on the patient, in order to prevent removal to an asylum from which they expect no return.

We know the number of pauper patients who leave our asylums cured. We are constantly regretting its smallness, and properly complaining that so many of those who enter the asylums are already in a hopeless state of disease. If, then, in addition to those who leave them cured, but few others leave them unless on their way to the grave,[1] we cannot marvel much at these mistaken views on the part of the people. And if these views can be corrected without injury to any of the insane, and if the confidence of the poor in the authorities of the asylum can be strengthened by returning to their homes a certain number of unrecovered patients,—that should certainly be done, if possible.

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[1] The difference between the discharges of *unrecovered private patients* and *unrecovered pauper patients* is always exceedingly great, so great as to force us to the conclusion either that many private patients are improperly discharged, or many pauper patients unnecessarily detained. This point is one of much practical importance, and deserves careful investigation.

Finally, it has to be borne in mind, that the first and highest aim of an asylum is the cure of those labouring under mental disease—a disease so frequently requiring in its treatment those special appliances which the homes of the rich cannot furnish, and still less the homes of the poor. It is the second aim of an asylum to provide for the safe keeping of those lunatics who are dangerous to themselves or others, irrespective of curability, and to provide also for the comfortable keeping of those who, though not dangerous and not curable, are in such a state from their disease as to make it difficult, if not impossible, to provide properly for their peculiar needs anywhere but in a special institution. When asylums pass these two aims they exceed their proper functions, and they do this to the injury of the whole body of the insane poor. That there is a feeling that these aims are being passed, and that an injury to the deepest interests of the insane poor is being thus done, late writings on lunacy supply good evidence. In his recent and very able work on the "Physiology and Pathology of the Mind," Dr Maudsley speaks strongly and clearly of the desirability of lessening the sequestration of the insane, and of allowing many of the harmless and incurable to spend their days in private families, with the comforts of family life and the blessings of the utmost freedom that is compatible with their proper care. He tells us that he thinks the future progress in the improvement of the treatment of the insane lies in this direction, and he goes on to say that when it has been found possible to act on such views, "then will asylums, instead of being vast receptacles for the concealment and safe keeping of lunacy, acquire more and more the character of hospitals for the insane; while those who superintend them, being able to give more time and attention to the scientific study of insanity and to the means of its treatment, will no longer be open to the reproach of forgetting their character as physicians, and degenerating into mere house stewards, farmers, or secretaries."

In their last report, the English Commissioners, when speaking of chronic patients in asylums, say,—"A patient in this state requires a place of refuge; but his diseases being beyond the reach of medical skill, it is quite evident that he should be removed from asylums instituted for the cure of insanity in order to make room for others whose cases have not yet become hopeless;" and they say further, that the removal of such patients will render "the present asylums effective for the reception of curable cases, and such as require special care."

The effects of such a withdrawal of chronic cases on the functions of asylums will be the same, whether the patients are sent to poorhouses or to private dwellings; it will enable the asylums to receive the two classes—*the curable, and such as require special care*—and will tend to establish their medical character.

The general recommendations of the Scotch Board seem to me to spring from reasons which are sound and unassailable. With this object in view—the greatest good of the greatest number—they appear to be offered; and what are they, after all, but an extension of that *non-restraint* which is the boast of this land and the glory of Conolly? It is a necessary effect of what he introduced, that the character of asylum populations should somewhat change, and also that the very number of those who are classed as the insane should be somewhat widened. Hence comes a *new state of things*, of the growing existence of which I believe all are conscious, though some may be unwilling to acknowledge it, and though there may be differences as to those *other new things* in which the redress is to be found. The recommendation to provide for a certain number of the insane in private dwellings may be regarded as an extension and a product of *non-restraint*, and it is so in the sense just indicated, but it is so in a still more literal sense; for if there be in an asylum an "incurable and harmless lunatic," whose "enjoyment of life" would be increased by a return to his friends, is not his detention in the asylum a *restraint*, and should not efforts be made to bring it to an end, and to place him in those circumstances which will best promote his happiness?

The efforts which have been made, and the discussion which has taken place as to the management of a certain number of the insane poor in private dwellings, and as to the condition and treatment of the insane at Gheel, have already borne fruit. There are few men dealing with insanity whose opinions have not through this source undergone some modification, and the fruit is further to be detected in the management and construction of many an asylum, and in the strength and width which have been given to the great principle of non-restraint.

The different ways in which pauper patients in Scotland can be legally provided for in private dwellings are as follow:—

	Percentage of patients in private dwellings (Scotland) disposed of in the different ways.
1. With their relatives as guardians........................	75·5
2. With persons as guardians who are not relatives—there being only one patient in the house...............	21·1
3. With persons as guardians who (as in No. 2) are not relatives, but who have obtained from the Board a special licence, and who may, according to the Board's approval, receive either one, two, three, or four patients....................	3·4

The first and second methods are in operation in England as well as in Scotland; the last is in operation only in Scotland. But there, as in England, the great majority of persons in private dwellings are under the care of relatives. Wherever suitable guardians can be found in relatives, these are chosen in preference to strangers, and accordingly we have 75·5 per cent. of the single patients boarded with their friends. The remaining 24·5 per cent. embraces those who live singly with persons not related to them, and those also who are in houses with special licences for two, three, or four. These last are but a small number, being in all 53 patients, and forming only 3·4 per cent. of the whole.

It naturally occurs here to inquire in what respect the position of the single patients in England differs from that of the single patients in Scotland.

There are in England 6638 pauper lunatics in private dwellings, and it appears that 81·6 per cent. of them live with relatives, and 18·4 per cent. of them with strangers.[1] These last all live singly, *so far as we know.* In Scotland, a small number (53 patients) do not live singly, but in twos or threes; that is,—in a few instances, instead of intrusting only one patient to a guardian, two or three are intrusted to him. It is only, therefore, in reference to these fifty-three patients that Scotland differs from England. And even this difference, which, after all, is a matter of degree rather than of kind, might disappear if we knew as much about the single patients in England as we do about those in Scotland.

It is true that the proportions of the patients under the care of relatives and under the care of strangers differ in the two countries; but this does not affect the principle of "farming out," which, if it exist in Scotland, exists equally in England. Indeed, if you take absolute numbers, there are in England 1221 patients so farmed out, and in Scotland only 384.

The 6638 single patients in England are under the care of boards of guardians and their officers, while in Scotland the 1568 single patients are under the direct and immediate control of the Board of Lunacy, who have considerable powers in respect to them. In Scotland their condition has been carefully inquired into, and is well known. In England little is known of their condition, and that little, we are told, is not much to its credit.[2]

In Scotland, the law places the whole body of the insane poor under the care of the State, whether they be in establishments or in private dwellings. It fully and clearly recognises the latter class, and assigns duties and gives powers to the Scotch Board regarding them. Under these powers their condition has been carefully looked into, and efforts have been made to render it as satisfactory as possible, and to see that none requiring the appliances of an asylum for treatment or care are denied that advantage.

In no other country is the law so comprehensive. Nowhere else in Europe is the saying of John Stuart Mill made so fully a matter of fact. "Insane persons," says this author, "are everywhere regarded as proper objects of the care of the State," and this is acted on in Scotland to a larger extent than anywhere else. The Scotch law may have defects; but, taken as a whole, it is not only behind none, but is in advance of all, and its promoters may well find pleasure in the work they accomplished. When it has been twenty years in operation I hope it will be able to point to achievements equalling those already performed in England, which are regarded with as much pride by the Scotch as they are by the English, and which are less the triumph of a nation than the triumph of enlightenment and humanity.

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[1] The proportions existing on 1st January 1864, are here taken, as no other figures are accessible. Since no influence has been at work to change the proportions, they may be assumed to be substantially correct.

[2] Journal of Mental Science, No. lii. p. 479 and p. 482.

Dr Robertson's remarks would lead his readers to suppose that a very large number of pauper patients in private dwellings in Scotland are under the care of those who have a special licence from the Lunacy Board to receive two, three, or four patients. The fact is, as I have stated, that only 3·4 per cent. of the whole single patients are thus disposed of. What their number will eventually be it is neither possible nor proper to predict. When suitable guardians can be found in relatives these will generally be chosen, and there is good reason for believing that the majority of single patients will always, as now, be found under the care of friends; but there are certain patients who have no friends at all, and yet who are harmless and incurable, and belong to the class whose enjoyment of life is increased by being out of the asylum; there are others, in the same condition, who have friends, but whose friends are not trustworthy, or are otherwise not suitable as guardians; there are others, again, also in the same condition, whose mental state has such peculiarities as to make absence from home and friends, though not detention in an asylum, desirable as a means of promoting their happiness and wellbeing. For these, and for other patients in like circumstances, it appears to me a very proper thing that the Board of Lunacy should have the power of sanctioning whatever arrangements inquiry shows to be satisfactory.

Various epithets are bestowed on the persons who are thus approved of by the Board as guardians. Dr Robertson, for instance, calls them *ignorant* and *needy*. I cannot call them *learned* and *affluent*, but I am able to state that they belong to the respectable working class, and this I regard as sufficient. I am able also to state that they are less ignorant and less needy than many or most of those relatives who are the approved guardians of single patients, and that they are certainly not more ignorant and needy than the class which yields the male and female attendants in asylums. Omitting the *ignorance*, and remarking only on the *neediness*, I have further to point out that, even in the case in which an approved-of guardian takes charge of two pauper patients solely and entirely for the reason that it will be of advantage to him, it is not necessary that he shall be needy in any other sense than would be applicable to a carpenter who undertakes to make a table, or to a surgeon who undertakes to reduce a dislocation. It should also be borne in mind, that many guardians who are classed as strangers because they are not relatives, are, in reality, connected to the patients by old acquaintance and friendship, for the sake of which they agree to receive them into their families, and undertake the care of them, though unable to do this without remuneration.

The average allowance to patients in private dwellings in Scotland is sixpence per day, and Dr Robertson says:—"There is little but the sixpence a day between them and neglect and want." But if the sixpence a day does secure the patients against neglect and want, why make it a shilling, or why make it even sevenpence, or why, indeed, give anything beyond what is found sufficient?

Practically, the matter stands thus:—the friends or guardians of some patients ask and require but little aid from the public, perhaps only what will provide clothing; the guardians of other patients need more, and the allowance must be such as will cover food and clothing; in other cases, again, it must be larger still, and the whole maintenance of the patients must be provided, and some remuneration given to their nurses or guardians. There is, and there ought to be, a considerable range in the amount of the parochial allowances. Each case should get what each case requires. Between nothing and a large allowance, it would be an absurdity to have no stage. From the person who is beyond the need of public aid, we go by a long succession of steps down to the person who depends on it entirely. It matters nothing how cheaply a patient is kept, if he be well kept—the cheaper, indeed, the better. It is the result which concerns us, and if that be good and satisfactory, it is no fault that the price is not a great one.

It so happens, and I speak from observation, that the condition of a patient has no necessary relation to the amount of the allowance in his case. In other words, he is not the better kept the more he costs, any more than those are the best asylums whose rates are the highest.

Sixpence a day is the *average* allowance for single patients in Scotland, and this *average* results from allowances considerably below and considerably above sixpence. Sixpence a day is also the *average* allowance for single patients in England—for both countries the average being thus the same. It does not follow, however, that sixpence in the two countries has the same value to the working class, and there are some reasons for thinking that a difference exists[1] which would be in favour of the North.

It is, of course, a proper thing to endeavour to lead the parochial authorities to take a liberal view of the peculiar wants of the insane, and in that direction much has been done in Scotland, where the average yearly allowance to single patients has risen, since 1858, from £7, 11s. 7d. to £9, 10s. 1d., or 25 per cent. But the requirements of each case have always been separately considered, and the recommendations have never been made on any such assumption as that doubling the allowance *necessarily* involved the doubling, or even the increasing, of the comfort and wellbeing of the patient.

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[1] Sixth Report of the Medical Officer of the Privy Council.

In providing for the insane poor, asylums take the place of first importance, and they do this *in a very emphatic sense.* Poorhouses, or something analagous to poorhouses, and private dwellings are merely supplementary. They complete the scheme, and become necessary as part of a whole. For obvious reasons, the need of these supplementary forms of providing for the insane poor is increasingly felt, and they are consequently receiving more attention than formerly. In Scotland this greater attention has for a considerable time been given to them, and with good results, both as regards poorhouses and private dwellings. It is to the last, however, that this communication almost exclusively refers, and I have endeavoured to make it convey a correct view of what is thought and done in Scotland in regard to the insane poor in private dwellings.

I think all will agree that it is desirable to ascertain the condition of lunatics in private dwellings, to see that none are there who require such care and treatment as an asylum only can furnish, and to see, also, that a proper provision is made for the safety and comfort of those whom residence in an asylum will not benefit.

This is the idea which underlies the so-called Scotch system. Its soundness no one can question, since every one admits the propriety of extending the care of the State to the whole number of the insane poor.

The system in no way or sense takes the place of asylums, being merely one of the *various* ways in which provision may be made for the insane poor.

That its operation may afford some relief to the accumulation of chronic cases in asylums is certain. Of the extent to which it may do this no one can yet speak with precision, but the experience of Scotland shows that it may be one which is quite appreciable.

There can be no doubt that where a system like that existing in Scotland is in full and active operation, many things are possible which are scarcely so in its absence. Asylum physicians, for instance, might have less hesitation in discharging unrecovered patients if they knew that the interests of such patients continued to be looked after, though they ceased to be under asylum care.

The discharge of such unrecovered patients increases the happiness and wellbeing of the patients themselves, gives pleasure to their friends, confers a benefit on the country, and is an advantage to the rest of the insane poor. This last is true even in a fuller sense than has yet been stated. "The rapid way in which county asylums are increasing in size, and the ever-recurring necessity of building new ones,"[1] seriously interfere with the accomplishment of those other schemes for the benefit of the great body of the insane, of which we may dream, but which, under existing circumstances, we need scarcely propose. Is there anything, for instance, more needed than public asylums for the middle and lower middle classes? Could the country fulfil a clearer duty or do a greater act of charity than in providing them? Do we not require places where the brothers, sisters, sons, and daughters of doctors, and clergymen, and lawyers, and schoolmasters, and people of such classes, may find care and treatment, apart from ordinary pauper lunatics, but at moderate rates? Do we not even feel the need of some gratuitous asylum provision for such persons? And do we not know how much mischief and misery occur in the efforts to prevent the sinking into pauperism of a member of a family which is quite above the ordinary pauper class in its feelings, in its history, in its social position, and in every sense, but which cannot meet a continued yearly deduction of even £40 or £50 from its income?

Do we not also need training institutions for young imbeciles, and asylums for the care of the young who are degradedly idiotic? And should not these look for their origin and support to some surer source than the voluntary contributions of the charitable?

To approve of the disposal of a certain number of the insane poor in private dwellings implies no narrow view of the claims of the insane. On the contrary, I think, it involves a comprehensive benevolence in their regard, and the promotion of their best interests. Such an opinion I believe to be held by a yearly increasing number of men; and the more the subject is investigated the more do I think it will be acknowledged that a certain number of the insane may properly be provided for in private dwellings, and that such a procedure will, both immediately and remotely, be a benefit to the insane.

ARTICLE VII.—*A Case of Gastritis Phlegmonosa, with Inflammation and Gangrene of the Gall-Bladder.* By T. GRAINGER STEWART, M.D., F.R.C.P.

(*Read before the Medico-Chirurgical Society, 4th December* 1867.)

As a good clinical and pathological example of a very rare disease, the following case seems to me to be worthy of being submitted to the Society. I shall mainly dwell upon the gastric affection, although some might consider the lesion of the gall-bladder the most interesting point.

Ellen W., a servant, æt. 28, had been in general healthy, but twice suffered from jaundice, connected with gall-stones. The latter of these attacks occurred in January 1867. She was treated in the

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[1] Journal of Mental Science, No. xlvii., p. 362.

Royal Infirmary, and made a good recovery. She complained that she was insufficiently supplied with food in the family to which she went on leaving the hospital. In August she entered the service of the family in whose house I saw her. From the time of her coming to them she complained of pain in the left side, and used to sleep with a pillow placed under it. She had a constant craving for food, particularly butcher-meat, and commenced her meals with considerable avidity, but was soon satisfied. She never vomited excepting once up to the period of the commencement of her illness. On Monday, October 14th, after seeing another patient in the house, I was asked to prescribe for her. She was feeling ill, and vomiting green and yellow bile. She could take no food, complained of flatulent distention, and a pain in the upper part of the abdomen, neither acute nor aggravated on pressure. Her bowels were open, but not loose. I prescribed some simple sedatives with effervescing draughts, and afterwards ice, and directed that the diet should be of the simplest description. By the end of the week she was again in her usual, and on Sunday, the 19th, she was out all day and took tea with a friend. She appeared quite well and ate heartily; but on returning home in the evening complained of flatulent distention, and soon began again to vomit. The following day her symptoms became more marked, and on the Tuesday I was again asked to see her. I found her restless, and complaining of much pain, vomiting frequently, the abdomen distended with wind, the pulse rapid, the countenance denoted serious disease, but the pain was not increased on pressure, nor was it of the sharp twisting character so common in cases of gall-stone. She frequently rose from bed, often changed her position, vainly seeking for an easy attitude. There was no jaundice, the liver was of natural size, the urine appeared natural. I ordered an aperient of calomel, and afterwards gave her repeated fœtid enemata with a view to relieve the meteorism. As the vomiting of bile and mucus still increased, she had various sedatives, including bismuth, hydrocyanic acid, opium, and ice, but without benefit; and on Friday, the 25th, as it was impossible to treat her satisfactorily in the house, I sent her to the Infirmary, where she was admitted to the servants' ward under the care of Dr Balfour. In the hospital she continued to exhibit symptoms like those above mentioned, but even more severely. Her vomiting was almost constant; she had no purging, but the bowels were open. Her urine was natural, her pulse rapid. She never rested, tossed about, often got out of bed, complained of great pain in the abdomen, particularly the upper part, and said also that she had pain extending up to the throat. The flatulent distention was extremely severe, and, notwithstanding the employment of suitable remedies, she rapidly became feebler, and died simply from exhaustion, without any head-symptom, on the 29th, about a week after her second attack had commenced,—a fortnight after the first.

The autopsy was made on the day after her death. The body was well nourished; there was no icterus nor œdema. The heart was natural, the lungs were congested and slightly œdematous. On opening the abdomen, the peritoneal cavity was found to contain a considerable quantity of serum and pus; coils of intestines were connected with one another by a recent lymph; while the liver was by similar material connected with the diaphragm and with the stomach. The liver itself was enlarged and congested. The gall-bladder was thickened and indurated; there was a gangrenous slough about the middle of its anterior wall, causing a communication between the interior of the gall-bladder and the peritoneal cavity. The gall-bladder was filled with biliary calculi, and a few similar concretions were found in the ramifications of the hepatic duct, causing local biliary congestion. The spleen was normal; the œsophagus was normal; the stomach was of fully the natural size, distended with air, and containing a small quantity of a fluid resembling beef-tea. The walls were throughout greatly thickened—the thickening tolerably uniform. On section, pus escaped from the cut surface, mostly from the sub-mucous cellular tissue. The mucous membrane itself was thickened, dense, and almost coriaceous, of a reddish gray colour. Its tubules and stroma contained pigment, but did not appear otherwise altered. The muscular coat was in some parts firm and continuous, in others partially disorganized. The peritoneal coat was inflamed and thickened. There was no pus in the sub-peritoneal cellular tissue; in the sub-mucous it was in parts infiltrated, in parts collected in little sacs of various sizes. The intestine was natural, excepting on its peritoneal surface. The kidneys and pelvic viscera was natural.

This case puzzled me extremely, and neither Dr Moir nor Dr Balfour, who saw it in its advanced stage, could satisfy themselves as to its nature; and even after post-mortem examination, it was difficult to understand it fully. The peritonitis had probably supervened only in the last days of life, and was apparently induced by the disease of the gall-bladder. It may be that that structure was the original seat of the disease, and that the inflammation of the stomach was secondary to it; but considering the seat, and the universality of the gastric inflammation, it seems to me more probable that that affection occurred independently of the other. The girl had evidently long suffered from gastric disease, and it is possible, that after the privations of her previous place, she had too freely indulged her appetite on her arrival in more liberal quarters. It may be that from this the disease took origin. Among its causes Cullen mentions external contusions, the swallowing of acrids, of indigestible food, or of very cold drinks while the body is warm, extension of inflammation from neighbouring parts, and the presence of various acrimonies generated within the body, either in the stomach itself, or in other parts, and poured into its cavity. More recent writers have not added materially to this list of causes, and so far as I know, none of them was in operation in my patient excepting inflammation of a neighbouring part, viz., the gall-bladder, which, however, could scarcely account for so diffused and general a gastritis, and could hardly correspond with the history of the case. Bamberger mentions that it occurs sometimes as a secondary process in pyæmia, and that Oppolzer has twice seen it in cases of puerperal fever, that it is very rare as an idiopathic disease, but that he has seen it once occur as such in the case of a strong young soldier, who speedily died of it. With regard to the symptoms, Bamberger justly remarks, that from want of observations it is scarcely possible at present to give satisfactory grounds for a diagnosis, and this fact has weighed with me in inducing me to put the case on record. Its general features well correspond with what Cullen long ago described, and with the case of the young soldier recorded by Bamberger, and differs but little from those given by Brinton in his work on the Diseases of the Stomach. As to the termination of such cases, I suspect they must be always, or nearly always, fatal. Dittrich has indeed described the cicatrices formed after the discharge of submucous gastric abscesses as leading to stenosis of the pylorus. Rokitansky and others describe the discharge of pus into the stomach from destruction of the mucous membrane. Of this, however, there was no appearance in our case. Lastly, as to treatment, probably no measures would have materially altered the course of the malady; but, had I known what was taking place in the stomach, I would have been more assiduous in the administration of opiates and of nutritive enemata.

ARTICLE VIII.—*A Case of Acute Yellow Atrophy of the Liver.*

By A. CHRISTY WILSON, M.B., L.R.C.S., Resident Physician, late Resident Surgeon in the Clinical Wards, Royal Infirmary, Edinburgh.

ACUTE yellow atrophy of the liver is one of those diseases the true nature of which is not yet definitely determined, and as it is only by a consideration of a number of cases that we can hope to arrive at certainty regarding it, it is incumbent on every one, who has the opportunity, to record cases of this rare disease. For permission to publish this case, I have to thank Dr Haldane.

Eliz. R., aged 25, an unmarried servant, was admitted into Ward XIII. of the Royal Infirmary on the 6th of December, at a late hour of the night. She stated that she had always enjoyed good health until last spring, when she began to complain of a feeling of weakness at her stomach, occasional vomiting, and a bad taste in her mouth. Three months ago the vomiting became more frequent, occurring after taking food. About two months ago the urine became high coloured, she vomited oftener, and she became weaker. Her appetite was good until three weeks ago, when it began to fall off. At the same time she noticed that her skin was getting yellow, that her stools were light coloured, and that the vomited matters, which had previously been whitish, became brown. Her urine was dark coloured, and constantly deposited a reddish sediment. A week ago she had pain in the region of her bladder, and difficulty in making water; but these symptoms soon disappeared. These facts were elicited from the patient with very considerable difficulty, as she constantly contradicted herself. From her sister it was learned that she had a child three years ago, and had been in the habit of frequently taking spirits. She also stated that the patient went about her work yesterday, but felt somewhat out of sorts. This morning she slept until late, but on being roused went to her work, but was dull and stupid. When seen by her sister in the evening, the patient was moping over the fire, and when spoken to, she answered incoherently.

Dec. 7. To-day the patient is drowsy, and can only with difficulty be made to answer questions. She is not at all emaciated, and complains of no pain, only a feeling of heaviness in the head. The skin of her whole body, and her conjunctivæ are of an exceedingly bright yellow colour. Skin dry. Her abdomen is flat and firm. Pressure on no part of it causes any pain. On percussion, the hepatic dulness can only be appreciated in comparison with the perfect resonance of the lungs, and measures a little more than an inch vertically in the mammary line. The left lobe cannot be discovered by percussion. Her bowels are constipated, but were moved yesterday. Tongue somewhat red and tremulous. Has no appetite. Lungs and heart normal. Pulse soft and variable, rising within a few minutes from 86 to 116 beats. The urine has a density of 1020, is of a dark greenish amber colour, and has a slight deposit of urates.

Vespere.—The patient is almost insensible, and can only be roused by severe pinching, or tapping with the finger-nail on the forehead. Arm when raised falls flaccid. Pulse 100. Is unable to swallow. Ordered an injection containing two minims of croton-oil.

Dec. 8. During last night the patient vomited a good deal, and was so insensible as to be unable to eject the vomited matters from her mouth. To-day she is less drowsy. Considerable hæmorrhage per vaginam. Pupils dilated.

Dec. 9. Patient had her bowels freely moved this morning after more croton-oil. Her stools were light coloured, resembling porridge, and were passed in bed. To-day her pulse weak, and 116. There are sordes on her teeth. She lies on her back, moaning occasionally, and insensible. She jerks about considerably, and sometimes cries out very loudly.

At 4 P.M. she was in much the same state, but there was muscular rigidity and slight opisthotonos.

At 7 P.M. she was rigid. Pulse 150. Temperature 104. She is now passing a large amount of blood by the vagina and rectum. The moaning is now more frequent.

At 10 P.M. her pulse was 100, and her temperature 105°. Muscular rigidity gone. Every now and then she makes ineffectual attempts to vomit. Skin dry. Catheter passed.

At midnight the pulse was 144 and very weak. Temperature 104·5°. Respirations 35 per minute. Patient is bathed in perspiration and is becoming livid. Hæmorrhages continue.

Dec. 10. At 2.15 A.M. pulse 144—at times quite imperceptible at the wrist, and can only be counted satisfactorily at the femoral. Temperature 104·5°. Respirations 52. Frequent hiccup. Loud rattle in throat. Face quite livid.

From this time the patient continued gradually to sink, and died shortly before 3 A.M.

The urine which was drawn off by the catheter, a few hours before death, was found after evaporation to contain numerous balls of lucine and acicular crystals of tyrosine. The latter were arranged either in a stellate manner, or like a bundle of arrows tied at their middle. There were also fusiform crystals of creatinine, and a great number of fatty casts.

The following is Dr Grainger Stewart's report of the post-mortem examination:—

"The body was very icteric, and there was considerable hypostatic congestion, and a few purpuric extravasations beneath the skin. The heart was flabby and stained with bile. There were numerous purpuric spots beneath the pericardium. The lungs were somewhat congested. The liver was diminished in size, and weighed a little less than two pounds. Much of its substance was of a bright ochre-yellow colour. There were also numerous extravasations in the substance of the organ. Many of its cells were in a state of fatty degeneration, and many were completely broken up. Some were swollen and opaque. The destruction was most advanced at the periphery of the lobules. The gall-bladder contained a little reddish gray mucus. The spleen was somewhat puffy. The kidneys weighed together 11½ oz. The capsules were readily separable. The cortical substance was dense and opaque, not congested. The tubules were full of dense opaque matter. The cells in some were swollen and opaque, in others fatty, and in yet others had completely broken down. The bladder was natural. The uterus was slightly larger than natural, not pregnant. Its mucous membrane was coated with a layer of bloody mucus. The follicles of the stomach were opaque. In many of them the cells were found in a state corresponding to that of the cells of the kidneys and liver. The intestines were normal."

The temperature, according to Frerichs, is only raised during the premonitory febrile stage, and when the delirium and convulsions are excessive; but in this case it was very high until shortly before the patient's decease, and probably continued so until death.

So far as I am aware, this elevation of temperature in acute atrophy of the liver has never before been observed.

Part Second.

REVIEWS.

Lehrbuch der Laryngoscopie und des Local-Therapeutischen Verfahrens bei Kehlkopfs-Krankheiten. Von Dr ADELBERT TOBOLD. Berlin: 1863.—[Text-book of Laryngoscopy and of the Local Treatment of Diseases of the Larynx. By Dr TOBOLD.]

Die Chronischen Kehlkopfs-Krankheiten, mit specieller Rücksicht auf laryngoscopische Diagnostik und locale Therapie. Von Dr ADELBERT TOBOLD. Berlin: 1866.—[Chronic Diseases of the Larynx. By Dr TOBOLD.]

Die Anwendung der Galvano Kaustik im Innern des Kehlkopfes und Schlundkopfes, etc. Von Dr RUDOLPH VOLTOLINI. Breslau: 1867.—[Application of the Galvanic Cautery to the Interior of Larynx and Pharynx. By Dr R. VOLTOLINI.]

Klinik der Krankheiten des Kehlkopfes und der Luftröhre, etc. Von Dr LUDWIG TÜRCK. Vienna: 1866.—[Clinic of Diseases of the Larynx and Trachea. By Dr TÜRCK.]

IT is gratifying to observe the large share of attention that is now paid to diseases of the larynx. Certainly no diseases more urgently required it. Their diagnosis was obscure, and the treatment unsatisfactory. All this has changed, or is rapidly changing. It is but a few years since Czermack introduced and pressed upon the notice of the profession the instrumental methods of exploration which he had invented or perfected; and already a number of competent observers, in the different countries of Europe, have devoted themselves to this department of investigation and practice. At present the laryngoscope is no longer a doubtful experiment: it has fairly and indisputably established its position as an indispensable means both for the investigation and for the treatment of diseases of the larnyx and the parts adjoining. Necessarily, the use of the instrument is still to a great extent restricted to the hands of specialists. But that is not for the present a disadvantage. The field is new, and the materials so abundant, that experts or specialists alone could collect and arrange them with sufficient rapidity to satisfy the present age. But while a minority of specialists may always remain, we have no doubt that before long the instrument will pass into general use. Already we find laryngoscopy employed in our public hospitals and in clinical classes, as well as in private practice; and, meanwhile, the contributions in regard to this instrument, and the diseases which it is used to explore, daily accumulate, proving that with the laryngoscope a new and fruitful era has begun for the pathology and therapeutics of laryngeal disease. We have previously noticed with approbation our English treatises on these subjects. We now introduce to our readers some of the newer Continental treatises, chiefly from the Prussian and Austrian capitals, where laryngoscopy is cultivated with much ardour and notable success.

The text-book of Dr Tobold, although not quite new, having been published in 1863, deserves notice, as it forms an introduction to his later work on the Chronic Diseases of the Larynx. It is divided into four parts. The first part is chiefly historical, and contains a sketch of Laryngoscopy up to the date of publication; but it gives also a complete account of the elementary details of the subject—such as the adjustment necessary in using the laryngoscope on a patient, or on one's-self. The construction of the instrument is fully explained; and the one which the author himself uses is described particularly with reference to the strong illumination which he obtains, by transmitting the rays of light from the lamp through a series of lenses, before reflecting them into the larynx which is to be examined. As a specimen of Dr Tobold's careful manner of description, we subjoin his directions for making a laryngoscopic examination:—

"The patient should sit in an upright posture, with the head slightly bent backwards, in front of and near the physician who is making the examination, whilst the illuminating apparatus is placed on a firm table, a little in advance of the patient's right side. The examiner should be seated at such an elevation that his eyes are nearly on a level with the patient's mouth. The latter is then directed to breathe naturally, to open his mouth widely, to protrude his tongue and hold it firmly so by means of a handkerchief in his right hand, and finally, to sound the letter Æ. The reflecting mirror, which receives the rays from the illuminating apparatus, is then adjusted opposite the patient's mouth in such a manner as to throw a bright cone of light into the fauces. The operator with his left hand supports the patient's under jaw, and then quickly, but gently, with his right hand introduces the laryngeal mirror, previously warmed over the lamp, and well rubbed with a piece of chamois-leather, holding it like a pen, with that part where the handle is inserted kept upwards. By the phonation of Æ, the patient's uvula and soft palate are raised, and the space of the fauces considerably enlarged. At the same moment the operator advances the mirror farther backwards and pushes it under the uvula, which is made to rest on the back of the mirror. The observer now shutting his left eye, looks with his right through the central opening in the reflector, or past the right edge of it, and sees the brilliantly illuminated mirror in the patient's pharynx, with a true picture of the larynx reflected upon it."

The second part relates to local treatment by means of the sponge, brush, syringe, and cauterization; and describes the process for the removal of tumours, and the application of electricity to the larynx.

Dr Tobold advocates the use of the sponge in preference to the camel-hair brush, and introduces either, without the aid of the laryngeal mirror, to see whether he goes into the larynx or not; both of which appear to us to be practices of doubtful expediency, especially the latter, as without using the mirror one is very apt to get into one or other of the hyoid fossæ, situated between the hyoid bone and the ary-epiglottidean fold, and so miss the cavity of the larynx altogether.

Part third is taken up with the subject of diagnosis by means of the laryngoscope, and a brief but fair description is given of the various appearances presented by the different affections to which the larynx is subject.

The fourth and last part is anatomical and physiological. Under the former are described the soft palate, fauces, tongue, and larynx; and under the latter the interesting subject of voice is discussed.

The second work by the same author—"On the Chronic Diseases of the Larynx," as already said, may be almost called a continuation of the former, and treats of the diseases of the larynx and their special treatment. It is divided into eight sections, as follows:—

1. A short chapter on the histology and physiology of the parts which form the larynx.

2. Diseases of the mucous membrane and of the superficial sub-mucous tissues,—as simple chronic laryngitis with and without ulceration, inflammation of various parts of the larynx, of the epiglottis, true and false vocal chords, and anæmia of the laryngeal mucous membrane.

3. Secondary inflammatory and ulcerative processes, and the affections of the deeper sub-mucous tissues,—as tubercular and syphilitic disease, œdema, and abscess in and about the larynx.

Each of these affections is fully described, as the following heads will show:—first, the anatomical and pathological conditions are given; then follow the symptomatology, course of the affection, and the etiology; next the diagnosis, as made by means of the laryngoscope; after this the prognosis; and, lastly, the treatment. The whole is succinctly detailed, and evidently by one who is perfectly conversant with the subject upon which he writes.

4. Treats of disease of the perichondrium and cartilages which form the larynx.

5. Nervous affections of the larynx divided into two sets,—in one of which the sensibility, and in the other the mobility is affected.

In the treatment of paralysis of the vocal chords, when electricity is used, Dr Tobold prefers to apply both poles of the battery externally, and not one inside the larynx and the other on the neck. We think he is too decided in his preference on this point, as undoubtedly one pole applied to the vocal chords directly—as with Dr

Morell Mackenzie's "galvanizer"—is in many cases most advantageous.

6. Is a short chapter on the anomalous forms the larynx may present.

7. Relates to the entrance of foreign bodies introduced by the mouth, through perforation of the œsophagus, or passing into the bronchi from wounds of the breast or neck. While

8. Treats of growths in the larynx of various kinds, as fibrous and mucous polypi, condylomata, cancerous formations, etc.

Finally, descriptions are given of the operations upon the larynx, and of the instruments employed in performing them. A few remarks are added upon the subject of producing local anæsthesia of the larynx, and parts adjoining. Dr Tobold doubts its utility, but on this point we must differ, as we have found it undoubtedly useful under certain circumstances.

On the whole, these two works deserve the attention of all laryngoscopists, as forming a very careful and complete manual of the subject.

The treatise of Dr Voltolini, of Breslau, is a small work of 73 pages, chiefly treating of the use of the galvano-caustic in diseases of the larynx and pharynx.

Dr Voltolini commences by describing the carbon and zinc battery of Middeldorpf, which is the one he prefers for this purpose; and he mentions it as being a *sine qua non* with this method of operating to have the galvanic current of such a strength that the platinum wires are made white-hot in a moment, on completing the circuit, for, if not, the patient naturally becomes frightened, and moves if he feels them gradually becoming hot. He points out how every instrument for operating in the larynx should be as fine as possible, so as not to obstruct the light, and, at the same time, simple to work.

The cauteries he describes are the "knife-like, the pointed, the nippers, and the noose or écraseur," the last of which, he states, heats soonest, and cools again rapidly on breaking the current.

The subject of Rhinoscopy occupies several pages, in which directions are given how to proceed in observing the nasal cavities from behind; and the treatise ends by the relation of 15 cases in which the galvano-caustic was used for touching and destroying ulcers, removing polypi, etc.

We now come to Dr Türck's work, which is much larger than those we have already mentioned, consisting of an 8vo volume of 582 pages of letterpress, with 250 woodcuts interspersed, and accompanied by an atlas of 24 plates, containing 114 most beautifully coloured chromo-lithographs, copies of drawings taken from nature, for this publication, by Dr Heitzmann and the late Dr Elfinger, of Vienna.

The whole work is well done, and reflects great credit upon its author, Professor Türck, who, with Professor Czermack, may be fairly said to divide the honour of having advanced laryngoscopy to its present important position.

The volume commences by giving general directions as to the use of the instrument, and the different methods for getting a good strong illumination. Then follows a description of the various diseases of the larynx, with the symptoms and treatment recommended, much in the same way as Dr Tobold has done, with the addition here of cases and of examples of the several diseases, illustrated by woodcuts—the more important ones being, besides, depicted in the atlas. The work concludes by a description of the instruments used by Dr Türck in his various operative procedures in and about the larynx, several of them being, if nothing else, extremely ingenious in their construction.

The author also enters at some length into the subject of producing local anæsthesia of the fauces and larynx, by means of various substances, morphia and chloroform being those he seems to prefer.

While we commend Dr Türck's elaborate work, we regret to have to notice two considerable drawbacks on its excellence. One is, that the particular kind of treatment employed is not stated in many of his recorded cases, which greatly impairs their value to the student or practitioner. The other blemish is the egotistical tone which pervades the whole treatise, and mars not a little the pleasure of its perusal.

Clinical Histories, with Comments. By HENRY DAY, M.D., Member of the Royal College of Physicians; Physician to the Stafford County Hospital. London: Churchill & Sons: 1866. 8vo., pp. xx and 254.

FROM unforeseen circumstances this book has remained longer without being noticed by us than, by virtue of its own merits, it should have done. We therefore now hasten to repair an apparent negligence, by at once assuring our readers that the work is, in every way, well worthy of their perusal.

Dr Day has here strung together, without any attempt at arrangement, the histories of thirteen different cases of a highly interesting character. Each case possesses some point or points of more than ordinary practical value; and as every "history" is prefaced by very shrewd and pertinent introductory remarks upon the particular type of disorder or disease to which they separately belong, and as, after a detailed account of each case, there are "comments" of a thoroughly instructive clinical nature, the book cannot fail to carry out the author's hope of proving both "interesting and instructive."

The histories, or essays, as we think they might with equal propriety be called, are as follow:—Cerebro-spinal Meningitis; a rare Case of Ovarian Disease; on Secondary Cancer in the Lung; the Treatment of Acute Rheumatism by Blisters; Rheumatic Fever without Pain; Chorea from Spinal Irritation; Pelvic Hæmatocele; Epilepsy from Peripheral Irritation; Epilepsy from Hepatic Congestion; Alternative Leucocythemia; Leucocythemia; Hysterical Facial Paralysis; (?) Cardiac Apnœa, formerly called Angina Pectoris.

It would exceed the limits of an ordinary review to attempt anything like an analysis of each "history," but we cannot help directing attention to certain parts of one or two of them, as appearing to us to be of peculiar interest.

The case of cerebro-spinal meningitis (sporadic) afforded an example of this disorder, in which the rheumatic condition of the articulation of an extremity was an early and well-marked symptom, and (as bearing upon the symptom) the history of rheumatism treated by blisters affords Dr Day the opportunity of directing attention to the opinion of the late Professor J. K. Mitchell as to the spinal origin of rheumatism. We observe, by an original communication of Dr Day's to the *Medical Times and Gazette*, of 31st August 1867, on this subject, that he is disposed to consider the almost universal occurrence of an arthritic-rheumatic condition in epidemic cerebro-spinal meningitis as proof, almost conclusive, of the correctness of Professor Mitchell's views; and we are certainly not indisposed to admit that the connexion between the two, *i.e.*, between spinal meningitis and acute rheumatism of the joints, is well worthy of future and further investigation.

The termination of the case of ovarian disease by rupture of the cyst, and the discharge of its contents *through the stomach*, is, as far as we are aware, the only case on record of such a termination eventuating in recovery.

Rheumatism of an acute character—that is to say, presenting the usual symptoms of fever, swelling of the articulations, acid sweats, etc., in fact displaying all the peculiarities of the disorder, *minus the pain*—is by no means a common form of the complaint, and is a type of this ailment well worthy of being studied in the history of the case here given.

The remaining "histories" are equally deserving of attentive perusal; whilst the last one in the book, "Cardiac Apnœa," affords our author the opportunity of relating a case of this terrible disease, which he had the opportunity of observing from its very commencement, till its sudden and frightful termination. The directions given by Dr Day for the general treatment and management of a case of this kind, seem to us to be in every way judicious, and the very best that can be made use of to combat this formidable affection; but, alas! who can hope for a successful result?

We must now conclude our notice of Dr Day's Clinical Histories; and, in according our strong commendation, have pleasure in recording our opinion of them as being a series of sound practical essays, written in an easy and pleasant style. We shall heartily welcome a second instalment of similar practical essays from the pen of the same distinguished physician.

Suggestions for the Systematic Study of the History and Relations of Cholera. By Dr BRYDEN.

Letter from JAMES L. BRYDEN, M.D., Assistant Surgeon, in Charge of the Statistical Branch Office, Medical Department, to N. CHEVERS, Esq., M.D., Officiating Inspector-General of Jails, Lower Provinces. Calcutta: October 27, 1865.

First Annual Report of the Sanitary Commission for Bengal, 1864–65. Calcutta: 1865.

Second Annual Sanitary Report for Bengal, 1865.

Annual Returns of the European and Native Armies, and of the Jail Population of Bengal Presidency for the Year 1866. Compiled and Systematically Arranged from the Original Documents. By JAMES L. BRYDEN, M.D.

IN India we have under our rule an extensive continent, covered by a great population, living under every possible variety of climate and condition; we have people inhabiting the low and marshy deltas of great rivers, as well as the lofty ridges and elevated valleys of the highest mountains in the world; we have different races living in the same localities, as well as people feeding on different dietaries, and observing varying habits, according to the arbitrary rules of caste and custom. There are few questions debated by anthropologists upon which some light may not be thrown, by a proper method of statistical research carried on in some part of the great continent between the Himalayas and the Indian Ocean.

Pathology has a right to expect much from the careful collation of the mass of facts which have long lain in the possession of the Indian Medical Department, but which have until lately been of comparatively little value, from the want of an officer set apart to examine and classify them, and to direct and superintend further inquiries. Dr Bryden has already given good proof that he is well fitted for the difficult task with which he has been intrusted; and the Sanitary Commission for Bengal has notified to the Indian Government its sense of the value of his labours.

Dr Bryden's remarks on cholera, made in a province where it is always present, and based on the statistics of above 150,000 individuals daily under European medical treatment and control, who are spread over the whole area of the Bengal Presidency, claims our attention at once. Regarding cholera as a specific disease propagated from a miasma existing without the human body, and whose action on the human body is merely a test of its presence, he treats it in two relations, as endemic cholera, "the cholera of a distinctly defined geographical area, within which it is perennially existent," and epidemic cholera, "the cholera of all regions beyond the limits of endemic area." Since the last appearance of cholera in Europe there has been a strong tendency, in our own country at least, to suppose that its progress is in a great measure due to contagion, though few physicians of any credit for ability are likely to support Dr B. W. Richardson in his thesis, that cholera spreads entirely by contagion. It would be difficult, indeed, on such a hypothesis, to find a resemblance between cholera and any really contagious disease, either in India or in Europe; and cholera would, at one and the same time, be the most contagious and the least contagious of all maladies. We have seen cholera patients treated in the same hospital tents in India, just as we saw them in the wards of the hospital of St Louis at Paris, and yet the disease did not succeed in propagating itself to patients in beds a few feet off, while it was at that very time traversing whole continents, and crossing deserts and seas with a rapidity which the most virulent contagious malady never attained. It is possible that cholera may in some instances be communicated from one individual to another; but its contagious powers are, as a general rule, very feeble. It is true that instances of contagion could be more readily observed in Europe than in India. It would be difficult to trace them in India, owing to the habitual want of social intercourse between different castes, the well-known untruthfulness of the natives, their neglect of certain sanitary precautions, and their disposition to take alarm at any interference with their social customs. At the same time, allowing full weight for these considerations, the phenomena presented by cholera on the great scale will show the impossibility of accounting for the diffusion of this mysterious disease by any coherent theory of contagion.

"Epidemic cholera," observes Dr Bryden, "is never in any case spread over a definite geographical area by human intercourse alone; nor can human agency cause the boundaries of a natural province, which has been occupied by cholera, to be transgressed, so that a cholera epidemic from this source shall appear in the province immediately adjoining, and become generally diffused among its inhabitants."

"Cholera has no locomotive power in itself, and the geographical distribution of an advancing epidemic is essentially dependent upon the existence of a vehicle." What idea does this word vehicle symbolize? To us it is like the unknown character in algebra, assumed to form the equation, though, unhappily, it does not solve the pathological problem. What is certain, cholera, from its endemic area of Lower Bengal, begins to invade Central India about the 20th of February. A second invasion in July and August reaches the upper provinces, but often stops on the confines of the Punjaub "for want of a vehicle." Cholera is at this season not much felt in Lower Bengal, "probably because the great breeding grounds are for the time entirely submerged." Towards the end of October there is an increased attack in the valley of the Ganges, which also visits Assam. Lastly, cholera, sometimes overstepping its usual limits, carries its ravages into different parts of Asia and Europe, and even across the Atlantic to America. High mountains, such as the Himalayas and the Alleghanies, seem to possess the power of stopping the epidemic, and, as Dr Chevers has pointed out, there are places in different parts of India which cholera has never visited.

"It has been stated above," observes Dr Bryden, "that the epidemic area of this presidency, or any of the provinces of the area, may become free from a cholera that has made an invasion; reckoning from the date on which an invading cholera leaves its endemic province, the life period of an epidemic cholera is probably three years, consequently, in the fourth year, an epidemic province should be free from cholera, provided no new epidemic has made its way into it, and introduced within it a new focus of reproduction."

Dr Bryden admits that cholera finds localities in the invaded territory where it can reproduce itself, and whence it spreads all around. At the same time this reproductive force becomes exhausted in a certain time.

The observation so frequently made that certain houses or barracks are liable to be again and again visited by different epidemics of cholera is confirmed by statistics.

"It may appear," says Dr Bryden, "on the banks of rivers; it may attack certain houses or barracks, which have, on this account, a deservedly bad repute; and it is very apt to reappear in military hospitals in which cholera patients have been treated during the first invasion, in the year before. It may be accepted as a general truth, that *where cholera has once appeared* on the occasion of an invading epidemic, *there it is apt to reappear;* and putting aside the question whether a cholera patient multiplies the specific thing cholera or not, we cannot doubt that he may become the vehicle for its conveyance, and that hence our hospitals are infected." . . "In regard to such cholera," says Dr Bryden, in his letter to the Inspector of Jails, "Dr M'Clelland has shown how the cholera of an European hospital at Meerut, of 1861, was not a cholera of the hospital, but of one ward of the hospital. It is from such cholera we can march away, *leaving the very thing cholera behind,*—persistent for the duration of its life-period,—still present if we return before the time of its existence has expired, but extinct as far as regards its power of producing cholera if we wait for the period of its natural death; a cholera, however, which there is every reason to dread in the same spot months afterwards, when the natural period of reproduction shall have come round, which may be fixed almost to a week for the various provinces and stations of the presidency, as we know when a flower will bloom, or the bud appear upon a tree."

The contagionists will probably insist in treating this localization of latent infection in a particular building or apartment as something analogous to the persistence of the germs of variola in the cabins of the "Wellington;" and, strictly speaking, these singular observations do not seem to hang very well together with Dr Bryden's theory on the duration of the cholera epidemic and the necessity of a renewal of the miasma from without. Unless we are misinformed, cholera has returned to these infected localities after being apparently absent for periods much beyond the life duration of the epidemics.

From such important researches, carried out on so wide a scale, the Indian Government can learn when cholera may be expected to visit a certain district, when or in what manner to march troops so as to avoid an invading epidemic, and where to march them should it threaten to visit one of our great military cantonments. Practically it is found, that it is only necessary to march troops a short distance from the infected locality to bring about the total cessation of the disease.

In the letter to Dr Norman Chevers, Dr Bryden shows, by the application of the views derived from the study of the large mass of statistical information at his disposal, the folly of erecting large central jails in unhealthy localities situated on the route usually taken by invading epidemics. The large jail is peculiarly liable to typhus, diarrhœa, erysipelas, and other diseases which assume a contagious form. The chance of survivorship is diminished by one-half if the prisoners be confined in a central, as contrasted with a district jail. Dr Bryden, therefore, objects to the proposal of collecting the prisoners in a few central jails, which would greatly increase the amount both of zymotic disease and air-borne miasmata.

Dr Bryden adds some curious remarks upon typhus fever as observed in the jails of the north-west provinces. Relapsing typhus he regards as the probable type of a disease which has for its varieties pleuro-pneumonic typhus, dysenteric typhus or epidemic typhoid dysentery, enteric typhus the fièvre typhoïde of the French. Yellow fever and the Bubo plague are also treated as generically descended from typhus, though now capable of reproducing themselves as separate varieties. Dr Bryden estimates "the value of typhus in a natural-history system as that of a *species*, a thing existing, and which we cannot call into existence." This definition is exposed to the retort of Socrates:—"I ask you what one thing is, and you tell me what many things are;" for there are plenty of things which exist, and which we cannot call into existence, that are not species. Moreover, a species is a thing which has no real existence. It is a mere generalization of the human mind. Though there is reason to doubt whether our generalizations of diseases are grounded upon certain unvarying characteristics, we should regret if Dr Bryden's pathological views were to lead him into any unrecognised manner of grouping or conceiving of diseases, or that he should allow his zoological illustrations to lead him so far away as not, in every instance, to observe the exact relation between the language of his assent and that of his evidence.

It is but fair to note that the classifications of diseases in the reports are not open to any greater objection than could be urged against any other possible classification, and that Dr Bryden's style successfully conveys the thoughts of a mind which takes a close and powerful grasp of the difficult and complex phenomena which it examines.

There is little space now left for the interesting statistics which fill the reports. While in Europe the mortality from cholera has diminished at every visit,[1] in India the epidemic of 1861 was more fatal than any of its predecessors. There were 1663 admissions, and 1065 deaths in the Bengal Presidency. At the single station of Mean Meer, 725 admissions and 450 deaths among 2452 individuals, in the space of little more than a month.

Though there are proofs from these reports that the amount of disease and mortality in India has been considerably lessened by increased care and sanitary precautions, it would be incautious to join in the over-sanguine hopes of those who believe that the mortality among our soldiers in India will fall to the rate of healthy districts in Europe. The rate (about 2 per cent.) and the number of hospital admissions are at present double what they are at "home stations" since the introduction of sanitary improvements; and let it be noted that this rate by no means indicates the real unhealthiness of an Indian climate. The death-rate is diminished by sending all those who are evidently sinking under the diseases of the country into a temperate climate, either to the hill sanitaria or to Europe. In the latter case, their deaths do not go to the average of Indian mortality.[2] The parallel could only be made a fair one by sending our soldiers suffering from phthisis, chronic bronchitis, or Bright's disease to some warmer country than Great Britain. The hot months are found the most unhealthy. Four-fifths of the deaths in the Bengal Presidency are occasioned by diseases incidental to an Indian climate—fevers, dysentery, diarrhœa, heat apoplexy, hepatitis, and cholera: next comes phthisis. The proportion of deaths in the Bengal European army during 1864–5–6 were: for phthisis 1·7–1·38, and 1·57 respectively; for hepatitis 2·8–3·49, and 2·71 per 1000 of strength. With the Native Bengal army the deaths were for the same years: phthisis ·5–·31–·51; hepatitis ·2–·28 and ·16. In our army at home, the deaths from phthisis are about ·37 per cent. In the jails the deaths were, for phthisis, 1·71–1·32, and 1·55; for hepatitis ·19–·29, and ·30. Most of the phthisical cases, both in the jails and native army, occurred in Bengal Proper and Assam, the climate of which is more constantly hot and moist, and more exposed to malaria than any other province of the presidency, and predisposes people of all races, climates, and dietaries, to asthenic diseases.

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[1] In the Annuaire de l'Économie Politique for 1867, the diminution of mortality in the department of the Seine is stated as follows:—During the epidemic of 1831–32, the mortality from cholera was 2·28 per cent.; in 1849, 1·79; in 1853, 0·76; in 1865, 0·31; in 1866, 0·26.—See pp. 232–233.

[2] In 1866, for example, there died of our European army 20·11 per 1000, but 49·04 were invalided, of which 608 were discharged, and 1109 sent for change of air.

The native troops are more liable to fevers, and as much liable to bowel complaints as the Europeans; but it must be kept in mind that their huts are much inferior to the European barracks, and that they are fond of sleeping out of doors.

The Sanitary Commission[1] is decidedly of opinion that trees planted in military cantonments will assist in purifying the atmosphere and in checking malaria if the proper varieties be selected, and they be not allowed to exclude air and light.

The mortality of Europeans is found to increase steadily with age. In India there is no such a thing as acclimatization. The death-rate of European regiments in the hills was, for 1866, 5·62 per 1000; the deaths of European women in the Bengal Presidency 25·46; of children 75·11.

Why is liver disease so common among Europeans in India, while amongst natives it is rarer than phthisis? Some attribute this simply to the change from a cold to a hot climate; others add to this cause the great use of alcohol and flesh meats among Europeans. Mr Strachey, the president of the Cholera Commission, observes: "Although it may be true in theory that we all eat too much animal food, we feel in practice that we shall eat it or nothing." This, in our opinion, is hardly correct; nor do we at all wonder that, with such a gross submission to inveterate custom, Mr Strachey should go on to say, "European appetites in India, during extreme heat, are constantly in an abnormal state, and more or less capricious."

On the other hand, Dr Macnamara has found that fatty and otherwise diseased liver existed to a great extent among men using a large proportion of animal food. Hoping for more statistics on the question, we insist, in the meantime, that alcohol directly stimulates the liver to inflammation; that flesh meats most probably do the same; and that, at any rate, they increase the quickness of the pulse during digestion, and render the whole constitution more liable to inflammatory disorders, which the climate directs towards the liver.

The statistics of the jail population give us some idea of the mortality of natives within their own climate. Political reasons prevent us quartering native troops in the country to which they belong; and we draw no recruits from Bengal Proper. The mortality in jails is above 8 per cent. Much of this is probably due in part to the previously bad tone of health of the prisoners, but much to overcrowding and underfeeding, owing to the corruption and pilfering of native jailers. Bowel complaints, atrophy, anæmia, sloughing of the cornea, and other diseases depending on a low vital energy, are very fatal to prisoners. The Government has ordered that, for the future, every prisoner should be allowed 648 cubic feet of air.

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[1] See Report for 1865, p. 47.

In the jails of Bengal Proper and Assam there were 346 deaths from anæmia and atrophy, on an average yearly strength of 14,634, during seven years (between 1859 and 1865); in those of Behar, Benares, Oude, and Cawnpore, they amounted (we continue speaking of the seven years' period) to 214 on an average yearly strength of 12,847; but in the Punjaub they were only 79 to a strength of 10,213. The deaths from leprosy were 62 all over India. Diseases of the kidney were rarer in the Punjaub than in Bengal Proper, being 3 in the one and 10 in the other. They were most numerous in the Agra, Meerut, and Rohilcund divisions, where they gave a total of 63 deaths to an average strength of 8067. In the rest of India there were only 33 cases, during the seven years, to a yearly strength of 42,819. Hepatitis is commonest in Bengal Proper and Assam, where it caused 52 deaths; in all the other provinces in India, only 47 to an average yearly strength of 36,252. Suicide is not common amongst the prisoners in India; one year's mortality records six cases, and there were five cases amongst the Sepoys, to a strength of 37,000.

Obstetric Clinic: A Practical Contribution to the Study of Obstetrics, and the Diseases of Women and Children. By GEORGE T. ELLIOT, M.D., etc., etc.: New York, 1868.

As the work of Dr Elliot shows, he is well acquainted with the Medical Schools of Great Britain, where, indeed, he studied for a considerable time. Since his return to his native country, he has been still a diligent pursuer of obstetric science, and of this the profession has had welcome evidence in several papers published in American medical journals. The work now under review is his first-born book or volume, and shows how fine opportunities he has had, chiefly at the Bellevue Hospital, for acquiring experience, and how diligently he has availed himself of them. But his book shows much more. It is the work of a physician of high education, a qualification in which obstetric authors are often deficient; it shows qualities of mind and skill of hand rarely attained by so young a man as George T. Elliot. Many ripe elderly practitioners might, but few young could, write a book so distinguished by candour, want of prejudice, kindly feeling, soundness of judgment, extent of erudition. While we do not say that the book is faultless, we say there is no book in American obstetric literature that surpasses this one. One of its faults we may point out, because it strongly "leans to virtue's side." The author says, speaking of cases, that he has published "especially such as terminated unfortunately." If he had said, "especially those in which an autopsy was procured," we should have approved his declaration. But we do not think any one is called upon "especially" to publish unsuccessful or unfortunate cases. He is bound, if an author, to avoid actively concealing them, and he is bound to state them if he pretends to give a record of his experience, or if they are specially instructive. But it sounds Pharisaical,—and we assert that Dr Elliot is not at all so,—to say that cases are published especially because they were unfortunate, assigning no other reason.

Considerably more than 100 pages of the book are devoted to the important topic of puerperal eclampsia; and while the author has little in the way of addition to make to our knowledge of the disease, and no novelty in treatment to propose, yet he shines as a painstaking, intelligent practitioner—an eclectic in therapeutics. The following passage gives our author's views, and will be interesting to many as a statement of the comparative reputation in New York of sulphuric ether and chloroform:—

"If only one method of treatment were given to me for these cases, my choice would unhesitatingly be for chloroform. The chief indications are, to terminate the labour as speedily as may be justifiable, and meanwhile to keep the patient moderately under the influence of chloroform.

"In looking over my cases, while recalling some in which the agent was administered unskilfully, I see every reason for believing that chloroform is the most prompt and certain agent that we possess for moderating the violence and preventing the recurrence of the convulsions.

"In this, as in all other cases where anæsthetics are used, the choice must be made between chloroform and sulphuric ether; and a man's practice is influenced by his familiarity with one agent rather than with the other; by the judgment of the consultation, and by the prevalence of opinion in the locality where he may chance to practise.

"In general surgical practice, and for general use, there is preference in New York for sulphuric ether. Some years ago a patient died under chloroform in the surgical wards of the City Hospital, and that agent was then banished from use in that institution. But since that time another one has died there from ether, and this anæsthetic continues to be used, from the conviction of the staff, that it is safer than chloroform. On the whole, it is generally believed in this city, that sulphuric ether is safer for surgical operations. Hence, a man in this city who should have the misfortune to lose a patient by an anæsthetic, would have more sympathy and approval if he had used sulphuric ether, than if he had selected chloroform.

"These influences do not, however, obtain to the same extent in obstetrics, for therein chloroform has been shown to act reliably, powerfully, and with trifling risk; while in no class of cases are its benefits more apparent than in those we are considering. In

Bellevue I have lately fallen somewhat into the habit of using sulphuric ether in obstetric operations, though in my experience the use of this agent in obstetrics bears no relation whatever in frequency to that of chloroform.

"The Bulletin of the New York Academy of Medicine for December 1861, contains a report of my remarks on the uses of chloroform in obstetrics, in the debate on the valuable paper of my colleague, Prof. B. Fordyce Barker, in which I see very little to change. Before that time, however, I had given sulphuric ether scarcely at all in obstetrics, but since then I have used it in a number of cases. In one I have found it better borne than chloroform, the latter agent depressing the pulse, which intermitted under its use, while ether produced a stimulating effect. The labour was natural. There is no difference in the effect of ether or chloroform upon the labour-pains, so far as my experience goes. The stoppage of the pains, or diminution of their force, which demands that chloroform should be occasionally denied to a patient, is produced as readily by ether. In the same labour I have several times seen these agents equally and successfully produce this effect, and the pains improve after each withdrawal. Hence the patient has been deprived of their benefit, as the alternative was an operation. In but one case have I personally met with alarming symptoms when I have given chloroform to a woman in labour."

To complete our view of the author's eclecticism, we quote another passage in which he gives a good account of his views regarding the value of venesection in eclampsia, the old trusted remedy.

"We are tempted to use venesection in cases of puerperal eclampsia by the recommendations of authors, and of so many practitioners—by clinical traditions in short—as well as by the appearances of great congestion observed in the head and face during the progress of the attacks. The purple, livid face and lips, and tongue; the congestive conjunctivæ; and the duskiness of the skin, which is often observed; suggest abstraction of blood as a measure of relief. If we analyze our apprehensions, however, we find that extravasation of blood upon the brain is what we chiefly dread; and if we examine the records of autopsies we find that such a contingency is extremely infrequent, and probably associated in the majority of cases with fatty degeneration of the blood-vessels of that organ.

"Nor can it be said that cerebral extravasations are infrequent because of venesection, for statistics show that we may assert the fact in cases where this treatment has been discarded.

"Moreover, it may safely be said that a majority of the severe cases of eclampsia occur in patients who are anæmic, and whose subsequent histories display tendencies to hydræmia, and that a roborant treatment with iron is most generally indicated after the immediate dangers of the confinement shall have passed. It is obvious that copious abstraction of blood, during the progress of these attacks, must therefore unfavourably influence the future convalescence of many of these patients.

"Another and natural argument for venesection at the present day, may be found in the likelihood that it may remove some of the poisonous principles which are supposed to affect the nervous centres; but we may eliminate these by other channels, and meanwhile powerfully control their influence by chloroform, until we shall have terminated the labour.

"Still, in patients of a plethoric habit, and more especially in cases where we suspect that atheromatous degeneration may be present, the moderate abstraction of blood may be a judicious practice, and not liable to do harm. But large and repeated bleedings do not seem to me to be indicated. Where there are evidences of anæmia, the abstraction of blood should be resorted to with the greatest hesitation.

"In a great majority of my cases, blood has been taken, though rarely by venesection, and in moderate quantities. In most wet cups have been used, a process applicable in various parts of the body, which adds the advantage of counter-irritation to the treatment, and measures accurately the amount of blood which is withdrawn, without the risk, so common in venesection, of taking more than is desired.

"I find myself resorting less frequently to this practice even, or with less confidence in the abstraction of blood in each succeeding year. In some cases it has seemed to me that the application of dry or wet cups over the kidneys has favourably influenced the action of diuretics."

Dr Elliot, in the midst of his anxieties and cares at the bedside of the suffering, has a moment to spare for pure science, and shows to his pupils the well-known experiment of Dalton demonstrating the Hunterian theory of the circulation in the placenta. Perhaps Dr Elliot is not aware that, in spite of Dalton's proof and all the rest, we have still among us disbelievers in the Hunterian theory to be won over. We hope that Braxton Hicks has tried the experiment, and that he will soon show us, if possible, how to gainsay it. We give Elliot's little notice on this point:—

"I proceeded in the dead-house to display the observations of Dalton by introducing a blow-pipe into one of the divided vessels of the uterine sinuses, and then inflating first the venous sinuses of the uterus, next the deeper portions of the placenta, and then the superficial portions. But although I did my best, the trial failed for the first time in my experience; a fact which was rendered annoying by my previous prophecy of success. In the evening, however, on examining the uterus with Prof. A. Flint, jun., and Prof. White of Buffalo, we found that the air had penetrated by this time immediately underneath the transparent chorion, and we each succeeded in readily inflating the whole placenta by Dalton's method, and proved the fact by cutting the chorion under water and blowing a stream of bubbles through the incision."

Speaking of the important operation of plugging the vagina, an operation, which regarded as mere plugging, is over-estimated, but has other valuable actions to sustain its recognised importance, Elliot gives us a good hint of Prof. Taylor's.

"My friend and colleague, Prof. I. E. Taylor, recommends in his admirable paper on placenta prævia, published by the New York State Medical Society, the use of 'the ordinary rolled surgical bandage, two inches wide, which is easily introduced.' The vagina can be packed with it fully and completely, pressing the soft, long, flabby cervix against the placenta (thus acting as a free compressor, and preventing any hæmorrhage taking place internally or externally, and also as an oxytocic), and permitting the plug to remain till the pains are increasing, or becoming more active, or sometimes even till it may be cast out of the vagina.

"The colpeurynter is preferred by many, and offers great advantages from the fact that it can frequently be refilled with cool or iced water; and Dr Greenhalgh prefers an air ball covered with spongio-piline. Large sponges are often used, and these rendered astringent by vinegar and other agents. It is therefore evident that there is no method which is so pre-eminently good as to rally all practitioners to the support of its claims."

Though Elliot is always solemn, not sombre, *gravis et doctus*, yet he sometimes condescends to mix in "Vanity Fair." Speaking of the unsatisfactory uterine treatment of what he calls menstrual epilepsy, he adds,—

"Thackeray has most delicately satirized the liability of our profession to a mistaken appreciation of the effect of our remedies.

"Major Dobbin was returning home invalided from India, wretched in mind from the conviction that Amelia was married, when he unexpectedly learned from Jos. Sedley that such was not the case, and he instantly began to mend just as the puzzled doctor had changed his prescription.

"The ship-surgeon congratulated himself on the treatment adopted by him toward his new patient, who had been consigned to ship-board by the Madras practitioner with very small hopes indeed, —for from that day, the very day that he changed the draught, Major Dobbin began to mend."

Sometimes he even introduces histrionic action into scenes that are seldom admitted to be grotesque. Here is a fœtus playing guisard, showing its poor visage at the end of a long-face case, and teaching a lesson against meddling:—

"As a rule, under the circumstances we are now considering, the labour passes on quietly and naturally: and if unnecessary manipulation be withheld, the face at last peers out from the vulva as from a frame, recedes in the intervals of the pains, and finally passes naturally into the world with only the likelihood of a disagreeable but temporary ecchymosis."

The same waggishness comes out again at p. 259, where, judiciously omitting the names of the physicians and their state as to bewiggedness, he describes a case partly as follows:—

"Loses self-control. Allows examination with great repugnance. Catches physicians by the hair."

Here again he brings great characters on the stage for an obstetric book, marshalling and settling the affairs of kings in republican simplicity:—

"If it be really true that Henry VIII. and Napoleon I. were asked, by the physicians in attendance on Jane Seymour and the Empress, whether the life of the mother or of the child should be saved; then, indeed, does it seem to me that they asked improper questions of those incompetent to judge. The great Napoleon's reply was the most admirable that could have been made: 'Treat the empress as you would the wife of a grocer.' 'Save the child,' said the horrid king; 'I can get wives enough.'

"The only accoucheur who might have been justified by history for the destruction of the child born in the purple, was Musa, who was sent for to the mother of Nero—*pro partu accelerando.*"

Puerperal eclampsia is evidently a favourite subject with him, for he is ever returning to it, and beginning again to make a grave medical quotation. We give the following very curious family history of an eclamptic patient:—

"The mother of the patient had given birth to four daughters, and had then died from puerperal eclampsia in the delivery of a son, who is now living and well. Of the four daughters, one died of eclampsia at the sixth month of her first pregnancy. Another had two miscarriages at an early period of pregnancy, and then died in eclampsia in the third, leaving a living child which weighed less than four pounds. The third sister had eclampsia at about the sixth month of her first pregnancy, and recovered."

In moderately deformed pelvis, Elliot prefers delivery by the forceps to resorting to version. He has printed a good statement of German experience on this point by Dr William T. Lusk, and concludes, on this old and vexed question, in a general preference for the forceps, an opinion in which he has Hodge and other distinguished American brethren on his side. But, after all, he is not very decided in his preference. Let him speak for himself.

"Every now and then we meet with recorded cases of version in deformed pelves where the results are so satisfactory that one feels as though they should establish the law for our interference. But, alas! few men are willing to print a frank statement of all their failures. Certainly, for my own part, despite my admiration for Sir James Simpson's theory and argument, my own preferences are for forceps as an elective operation, though time and further observation may change my views. Still, my experience thus far, and the opportunities which I have enjoyed for witnessing the operations of others, strengthen my opinion.

"In a conjugate of three inches and upward, with a living child and a head presentation, my first choice would be for forceps. Between two and a half and three, if the child were living, I should perform version."

We had marked out several other subjects in the book for remark, especially the use of pessaries, the shameful frequency of induced abortion in America [a subject more than once referred to in the last number of the "North British Review"], an excellent appreciation of the value or rather the comparative want of value of the postural treatment of prolapsus of the funis, etc.; but we stop with a recommendation of carbolic acid, appropriate to some questions of the day :—

"During the past year, the wood-work of the lying-in wards of Bellevue has been dampened every day with a weak solution of impure carbolic acid. Dr Done informed me that one of the professors of the Smithsonian Institute had expressed his confidence in its ability to destroy the germs of infection, and I introduced it in the hospital. Certainly we have had much less puerperal fever and inflammation since it has been employed."

Part Third.

MEETINGS OF SOCIETIES.

EDINBURGH MEDICO-CHIRURGICAL SOCIETY.

SESSION XLVII.—MEETING I.

4th December 1867.—Dr MOIR, *President*, in the Chair.

I. TREPHINING IN EPILEPSY.

Dr George W. Balfour exhibited (1), a piece of a left parietal bone with a trephine hole in it; (2), the circular disc removed by the trephine, exhibiting irregular thickening of the bone; (3), a portion of the parietal lobe of the brain, from the same patient, exhibiting a cavity the size of a walnut, opening on the surface, through a convolution, by a small circular aperture about the size of a fourpenny-piece, just anterior to the parieto-occipital fissure; it contained a yellowish serous fluid, and was lined with a thickish membrane, from the surface of which numerous crystals of hematoidine could readily be scraped, being evidently a cyst resulting from an apoplectic clot. These preparations were from the head of a young man who had been epileptic for thirteen years, and who had been trephined about two years before his death. The operation was successful in averting the downward progress of the disease, and rousing him out of a state of almost brutal imbecility, in which he then was; it also, for the time, materially relieved the fits. In a diagnostic point of view, it was interesting to find that the piece of cartilage which, with its superior surface, filled the trephine hole, was also found to cover with its inferior the opening of the cyst. The place of operation was selected for reasons given with a concise history of the case; but as this will be published in our next number in a more detailed form, it is unnecessary to anticipate, by giving any further particulars at present.

II. DIPHTHERITIC MEMBRANE.

Dr Alexander Miller showed a specimen of diphtheritic membrane, expectorated on the day of the patient's death, bearing distinct markings of the thyroid cartilage, tracheal rings, and bifurcation of the trachea. It was 5 inches in length, and, throughout its greatest extent, 2 inches broad. It formed an almost perfect cast of the affected portion of the air-tube, as far as the bifurcation.

III. URETHRA AFTER HOLT'S OPERATION.

Dr Alexander Miller also showed the bladder and urethra of a man who had suffered from stricture for many years. The stricture was "ruptured" according to Mr Holt's method. The case proceeded perfectly satisfactorily till about a fortnight after the operation, when the patient took ill and died of obstruction of the bowels, from a double twist on the ileum. The preparation shows the urethra without a trace of stricture or injury, eighteen days after "rupture" of the stricture; a remarkable testimony to the efficacy and safety of Mr Holt's operation.

Dr Grainger Stewart read a paper on

IV. A CASE OF GASTRITIS PHLEGMONOSA.

This will be found at page 732 of this number.

Dr George W. Balfour stated, in reference to Dr Grainger Stewart's case, that it was, without exception, the most latent case of peritonitis that he had ever observed, and even when he last saw her alive, a few hours before fatal syncope occurred, she presented no symptoms which would lead one to suppose that she was dangerously ill, still less that she was death-struck from peritonitis—her movements being free, and made without pain; while her pulse, though rapid, was by no means extremely so, and was to the last tolerably full. In regard to the pathology of the case, Dr Balfour thought that Dr Stewart had omitted referring to the most interesting part of it. It was undoubtedly a well-marked case of gastritis phlegmonosa; but it was still more interesting as being also a case of gangrene of the gall-bladder, the gall-stones lying exposed and bathed in the peritonitic pus, from out of which he, Dr Balfour, had picked two of them. Escape of gall-stones into the peritoneum through a gangrenous gall-bladder, was so rare an affection, that Frerichs only mentioned one case as having been observed by Bretonneau, but to this he gave no reference. Dr Craigie also referred to one case as having been related by Andree: this, however, was an instance of rupture of the gall-bladder, in the case of a London physician's wife; it occurred while she was walking on the street; a gall-stone escaped into the peritoneum, but no further particulars were given. Dr Balfour had been unable to find any other cases of rupture into the peritoneum, though escape by ulceration into the duodenum, or on the surface of the body, was a comparatively common occurrence, and this he considered constituted the most interesting part of the case. In regard to its etiology, Dr Balfour exhibited the biliary calculi which he had removed from the body, and pointed out that the largest of them had a narrow constricted portion at one end, which lay in the cystic termination of the duct, where it acted as a ball-valve, permitting accumulation of bile, and preventing its escape; and to the distention thus produced, was probably due the fatal attack of inflammation.

SESSION XLVII.—MEETING II.

18*th December* 1867.—Dr Moir, *President*, in the Chair.

I. CASE OF CLEFT PALATE.

Mr Annandale introduced and showed a male patient, æt. 18, on whom he had operated, in September, for congenital fissure of the palate. The fissure involved one half of the hard and the entire soft palate. The result was perfect union, and already a considerable improvement in speaking. The operation was performed in the manner already described to the Society. No muscles were divided.

Dr Handyside remarked on the excellence of the results, which were, in his opinion, most creditable and satisfactory.

II. COMPOUND FRACTURES AND DISLOCATIONS OF THE ANKLE-JOINT.

Mr Annandale exhibited three preparations illustrating compound fractures and dislocations of the ankle-joint. One of these was an example of a somewhat rare injury, namely, a fracture of the os calcis, which was taken from a patient æt. 45, who had fallen forty feet from a scaffold.

III. ENUCLEATION OF A FIBROID TUMOUR OF THE UTERUS.

Dr Matthews Duncan exhibited a fibrous tumour, 2½ inches broad, which he had just removed by the operation of enucleation and avulsion, which he had described at a meeting of the Society last year. The operation involved no loss of blood worth mentioning. It was effected by continued traction, and the tumour did not come away till after perseverance for three-quarters of an hour.

IV. DISEASED EYE.

Dr Argyll Robertson exhibited an eye which he had removed from a young man on account of sympathetic disease in the other eye, and in which a piece of iron the size of two barley-corns put end to end was to be seen on the posterior surface of the iris, close to the ciliary processes. The patient, while at work, received a blow from a piece of iron which caused a large wound of the cornea. This after a time healed, but the eye continued irritable and sensitive to the light. As the patient was of opinion that no portion of the iron had remained in the eye, iridectomy was performed with the view of moving the irritability of the injured eye, with, however, only temporary good effects, so that, to save further impairment of sight in the good eye, it became necessary to enucleate the other.

Dr Handyside read a paper on

V. CARTILAGES SEMI-DETACHED AND LOOSE IN JOINTS.

The author referred chiefly to their mode of origin and minute structure.

Mr Annandale read a paper

VI. ON THE TREATMENT OF CLUB-FOOT.

This will appear in an early number.

The *President* then proceeded to deliver the following address:—

VII. VALEDICTORY ADDRESS BY DR MOIR, PRES. R.C.P.E., PRESIDENT OF THE SOCIETY.

Gentlemen,—Though two years have now passed since you did me the honour of electing me your President, I have not forgotten the surprise with which I then learned your intention of bestowing upon me this altogether unexpected distinction, nor my serious misgivings as to whether it was my duty either to you or to myself to accept of it—believing sincerely that there were many other members who had much more time at your disposal than I had, and were much more competent to conduct and regulate your meetings than I was; and if, notwithstanding all my fears and misgivings, I have found my duties much easier and more pleasant than I anticipated, I feel that this is due very much to your kind consideration and indulgence, and chiefly to the kind and valuable assistance of your Secretaries—Drs Watson and Dycer.

With many others here, I remember well that, on various occasions, the floor of this Society has been the arena of serious and well-fought battles—occasions in which the highest talent of our profession has taken part, and waged a valiant combat for the sake, not of victory, but of truth—and I am sure I but express the feelings of all present when I assert that the consequence of these full and free discussions has been the advancement of truth; which, though perhaps for a season overcome, has, in the end, carried off the palm of victory. Need I, as confirming these views, remind you of the intense excitement that accompanied the discussion of such subjects as Homœopathy, Change of Type in Disease, Ovariotomy, Chloroform, Acupressure, and others that will occur to you; and of the complete change of opinion on some of these points which many of their former opponents have openly avowed and acted on.

During the last two years there have been no such exciting scenes, but the Society has held on the quiet and even tenor of its way, promoting, by its meetings, feelings of brotherly goodwill and fellowship among its members, while, at the same time, advancing the knowledge of our profession by well-written papers and discussions.

On taking a rapid and cursory review of our two years' work, the first point that occurs to me as deserving notice is that portion of our time devoted to the exhibition of pathological specimens. These, to the general practitioner—and indeed to all not intimately connected with or attending a large hospital—are both interesting and instructive, refreshing the memory on subjects almost forgotten, and often, to the oldest amongst us, invested with new and real interest from the advances made in pathological knowledge since the time when, in days gone by, we were more in the habit of seeing and examining similar specimens with our own hands; while to all, even the youngest, they are highly profitable by making us more and more conversant with the appearances of pathological structure, both medical and surgical.

It would be vain, on such an occasion as this, to attempt an enumeration of all the interesting specimens that have been exhibited. I shall mention only a very few, which, from their rarity or importance, ought to be brought to your recollection.

Professor Spence, and Drs Watson, Annandale, and Bell, exhibited various calculi, urethral and vesical, obtained after death, or removed from the living body by cutting or crushing.

The same gentlemen also brought under our notice tumours of various character, and from various organs of the body—one of the largest of these being a fibrous tumour of the uterus of about 30lb. weight, removed during life by Dr Gillespie, who gave us a full account of the case, with highly judicious remarks as to the propriety of interfering in similar circumstances, in a paper since published in our monthly Medical Journal.

Mr Annandale exhibited an interesting and instructive specimen of a twisted knuckle of ileum causing death—Mr A. very properly suggesting, I think, that, in similar circumstances, an operation should be performed by cutting through the abdominal parietes, to relieve the obstruction, thus giving the patient a chance for life.

Numerous specimens of disease of the knee-joint, principally obtained after removal by operation, were exhibited by Dr. H. Watson. The same gentleman exhibited about one half of the tongue successfully amputated by linear écrasement without hæmorrhage; as, also, a successfully treated case of cleft palate —both of the latter cases long considered as two of the *opprobria medicinæ* —though now, thanks to the advance in surgical science, no longer so; and, while on this subject, I must not forget to remind you of the very ingenious and beautiful vulcanized india-rubber diaphragms, so successfully employed for the treatment of this distressing malformation, by Mr Ramsay of London, who was so obliging as to exhibit them to the Society.

We had large aneurismal sacs exhibited by Drs Haldane and John Duncan; a live case of Addison's disease by Dr Sanders; and by the same gentleman the pelvic veins in a case of uterine phlebitis and pyæmia, with inflammation in all the three coats of the veins, and small abscesses in the external and middle coats.

To Dr Grainger Stewart we are indebted for a variety of interesting pathological specimens. *From the brain.*—A case of atrophy of the right optic nerve, in front of the commissure, from a patient blind of the right eye from infancy. *From the thorax.*—Specimens of gangrene of the lungs from embolism; of rupture of the heart from aneurism; and of a laceration of the aorta, near its commencement, from which the blood gradually made its way to the pericardium. *From the abdomen.*—Non-malignant ulcers of the stomach, as well as cancer of that organ, and of the small intestines; and an interesting case of stricture of the duodenum from the cicatrization of ulcers. Hydatid cyst of the liver, and a series of specimens illustrating the changes undergone by syphilitic masses in the same organ:—a case of pylephlebitis and abscesses in the liver, the consequences of an abscess behind the cæcum, ultimately giving rise to pulmonary abscesses: and a curious fatty tumour about the size of a goose-egg, discharged per anum, with the full particulars of the case.

The only other specimen exhibited by Dr Stewart that I shall allude to is one highly instructive and illustrative of the evils of long-continued labour, but one fortunately of rare occurrence now-a-days, I hope, in any part of Britain—so rare at any rate in this quarter, that I do not remember of having seen or heard of more than two or three cases in Edinburgh during the last forty years. The great hazard incurred by allowing labour to be protracted for days without interference was long before that time pointed out and strongly inculcated in this University by the late Professor Hamilton, which is, I believe, the reason that such cases have been rarely met with here, though the case at present referred to shows that they do occasionally occur. We saw in it how the long-continued pressure on the linings of the pelvis had caused inflammation and sloughing of the whole of its contents; the surface of the promontory of the sacrum being abraded, with sloughing of the uterus and of the bladder, phlebitis, thrombosis of the iliac veins, and death suddenly from embolism.

Such, Gentlemen, is but a specimen of the intellectual viands provided for you during the first course at our meetings, and I am sure I but echo the sentiments of all of you when I say that this is a part of our ordinary programme, that none of us would like to see missed out or curtailed.

I shall next very briefly refer to some of the Communications which have been read here, and I must candidly confess that I am afraid I can give no very definite reason for the selection, save that they either particularly interested me from their novelty, or were on subjects to which I had, at one time, paid more than ordinary attention.

One of the first of these communications was an interesting paper by Dr Sanders on Aphasia. This subject, illustrating the connexion of the faculty of speech with the anterior lobes of the brain, attracted a good deal of attention some thirty or forty years ago; and I may be excused for stating that, in the 42d volume of the *Edinburgh Medical and Surgical Journal*, for 1834, I reported two cases as bearing on this very subject—one a case of punctured wound and injury to the left lobe of the brain, through the orbital plate of the frontal bone, in which it is stated that, notwithstanding very extensive disease of the anterior lobe of the left side, the patient, for more than a fortnight after the accident, and up to within two days of his death, retained the faculty of language, and repeatedly named individuals who came to see him; while, in the other case, with no diseased structure in the anterior lobes, but, on the contrary, in the superior part of the left hemisphere and posterior lobe of the same side, the patient had lost the faculty of expressing her ideas, though not of speech, and could not mention the names of her friends though she knew them. For a time the subject then seemed to attract little attention, but it has been revived within the last few years, principally in connexion with the dissections of Mons. Broca and some others. Dr Sanders favoured the Society with an able paper summarizing the views at present held on this subject, and also reporting the particulars of a case of aphasia, in which the autopsy confirmed the views of Broca. But Dr Sanders, while allowing its due weight to this coincidence, wisely and philosophically concludes that the subject requires fresh investigation, and that the new opinions cannot be accepted without the most ample and conclusive evidence. I have no doubt that, ere long, we shall have the reports of many cases, either confirming or disproving the correctness of these views. Indeed, already, since the paper was read, we have had some reported by Drs Sanders, Tuke, and others, bearing on the subject.

Dr Warburton Begbie favoured us with an admirable and well-reasoned essay on Paracentesis of the Thorax in the treatment of Pleuritic Effusions. In this paper, involving a question of great practical importance, Dr Begbie clearly demonstrated the value of this operation—not only as a curative remedy, but also as being frequently the only means of saving life. He, at the same time, disarmed our minds of the great dread with which. in the younger days of some of us, we were taught to anticipate the result of such an operation.

On the subject of Bronchiectasis—a pathological condition to which much less attention has been paid in this country than on the Continent—we had a paper by Dr Grainger Stewart, interesting as showing the advance in pathological knowledge during recent years, and the great advantages that may be gained from the opportunities of studying pathology which are afforded in our hospitals. In this paper, Dr Stewart has very clearly indicated the various forms of this affection, as well as other morbid changes of the bronchi, with which the true disease may be confounded; and, after minutely describing the change from healthy structure in the bronchial walls, proceeds to the enumeration of the contents of the sacs, which in some cases, as you are aware, give rise to most offensive fœtid breath, quite distinct, however, from that accompanying gangrene of the lungs, a fact insisted upon, as you will remember, by Dr Laycock, in a communication read before this Society, and published in the *Edinburgh Medical Journal* for 1865. As to the nature of the affection, Dr Stewart's opinion appears quite a legitimate conclusion, from the observations he has adduced, that it is caused by pressure of the air on the bronchial walls, previously weakened by an atrophic process. It is to be regretted that, as yet, little more has been advised, or can be expected to be beneficial in the way of treatment of this affection, save the alleviation of the more urgent symptoms.

I must not forget, in this hurried sketch, to remind you of a short but valuable practical paper by Dr Hjaltelin, Inspecting Medical Officer of Iceland, on the Treatment of Hydatidic Liver, a disease little known in Scotland, but very common in Iceland. Previous to 1863, the general practice there was to open the hydatid cyst by the knife or by caustic; but, in that year, Dr Hjaltelin believing, after careful reasoning, that alcoholic remedies taken into the stomach do reach the portal vein, and would from it be thrown directly on the liver and might reach the parasite, had recourse to the tincture of kamala, and having found it successful, recommended it to his medical brethren, who have given very satisfactory reports of the success of the remedy; though, in cases of long standing, an operation has been still found necessary as the only means of relief.

The members of this Society are at all times delighted to be honoured by the presence of strangers and foreigners at our meetings, and that they should take part in our discussions, or favour us with papers. In such cases it is extremely gratifying to perceive how well informed many of our foreign medical brethren are in British medical literature, and that they are able not only to write, but also to speak remarkably well in our language. All who had the pleasure of hearing Dr Hjaltelin the evening he was with us, must have felt how completely such remarks are applicable to him,—and that he was most justly entitled to our best thanks for favouring us with his company, as well with a most interesting communication.

I cannot notice at length, or as it deserves, Dr Tuke's paper on the Insanity of Pregnancy, Puerperal Mania, and the Insanity of Lactation; but must refer those interested in the subject to the paper itself, which will amply repay a perusal, the result of which will be, I feel sure, the conviction that Dr Tuke has clearly pointed out the various symptoms that characterize these three states of insanity, and the treatment adapted to and required by each.

Dr Frederick Skae also favoured us with a short but interesting and useful paper on Insanity caused by Injuries to the Head and by Sun-stroke,—indicating a great resemblance in the symptoms, period of attack after injury, and pathology of cases produced, either by blows or sun-stroke; and illustrating his views by some very characteristic cases.

There is no more appropriate time, perhaps, than the present to refer to a communication on Choreomania from Dr Davidson, Medical Missionary at Madagascar, a gentleman known and held in the highest esteem by many of us for his many excellent and Christian qualities. It contained a most graphic and interesting account of that curious psycho-physical disease, as it occurred under his own observation at Madagascar;—confirming, as exactly as could well be expected in such a disease, the accuracy of the accounts given of similar epidemics as they occurred at various periods in the middle ages, in France, Germany, Belgium, and Italy. May I be permitted, in passing, to make the remark,—how wise in every respect, and among others, in the interest of our profession, has been the plan of sending out thoroughly equipped physicians and surgeons as missionaries to heathen lands, seeing that not only have they been eminently successful in planting the religion of their Master in quarters otherwise apparently inaccessible, but have also been able to send to their medical brethren at home descriptions of various diseases from places which another medical man, under ordinary circumstances, would scarcely have been allowed to visit.

Having as yet taken no notice of any surgical papers, allow me first to remind you of one, or rather two, ably written articles by Dr John Duncan, on the Employment of Galvanic Puncture in the Cure of Aneurism, with some very interesting illustrative cases. Dr Duncan not only gives us a full account of what had been already accomplished in that field of surgery up to the present time, but also of a course of experiments, very carefully conducted by himself and his friend Dr Fraser, with the view of clearing up many difficulties and of pointing out not only the best mode of operating, but also the results to be expected. The information is most important, and cannot but be useful to all future operators by this method. I need not add, that any new remedy likely to be successful in so serious a disease will be hailed with satisfaction by the profession; and it is to be hoped that future trials will confirm Dr Duncan's views, and tend to save, or at least prolong, life in some cases where all other known means would be inapplicable.

It is scarcely necessary to remind you of the great interest taken in Dr Watson's illustrations of successful Excision of the Knee-Joint, and also the new and improved apparatus employed by him in the after treatment of these cases. These new modifications of mechanical appliance for the support of the limb after the operation, seem almost perfect. At any rate, the contrast between the means used in former days, which I well recollect, and those that I lately had the privilege of observing in some of Dr Watson's cases, is most astonishing; indeed, the wonder is that, under all the annoyances and inconveniences to which patients were subjected under the old system, any one of them escaped without the most painful bedsores and distortion of limb; whereas, under this new plan, the patient is allowed from the first almost as much freedom as if he had undergone no serious operation whatever, while there is scarcely the vestige of a chance of his suffering from the dreadful attendants of the former method. It is needless to speculate as to the extent to which this change must conduce to a favourable result, not only in the cases under review, but in the management of many other accidents and operations affecting the lower extremities, whether at home or, still more, in the field.

I must also remind you of the account of a successful case of Traumatic Tetanus, by Dr Junor of Peebles, the principal remedial agent being infusion of tobacco; also of a case of Ununited Fracture, by Dr John Duncan, in which, after other means had failed, the cure was effected by scraping up the periosteum, close to the fracture, with a blunt tenotomy knife; and lastly, of a highly interesting and characteristic case, by Dr Joseph Bell, of Pulsating Tumour in the Orbit, cured by ligature of the common carotid.

Dr M. Duncan's communication on the Nature, Source, and Surgical Treatment of Hæmorrhage connected with Fibrous Tumour of the Uterus, though the last I shall refer to, is perhaps, in a practical point of view, one of the most important that has been read to the Society last session. The accident is one of frequent occurrence, involving the usefulness at least, if not the life, of many a patient; and Dr Duncan has, in this paper, enunciated what I believe to be the sound rational principles of treatment, and the best modes of carrying them out in practice. For this excellent paper our thanks are due to Dr Duncan.

I trust the members will excuse my making here a suggestion with regard to the meetings of the Society, which may, I think, be worthy of future consideration.

At the meetings of the Obstetrical Society, it has not been the general practice of the members to read long papers or dissertations, but rather to give an epitome of such, containing the views of the author, while reserving the entire paper for publication. A similar practice is, I believe, followed in other Societies. But more frequently—and it is to this part of the business that I here more especially refer—cases are brought forward by members that are either of unusual occurrence, or in some other respect remarkable and deserving of notice, or in which some doubt or difficulty has been experienced; and then a quiet conversational discussion takes place, in which a great many of the members join, and thus an amount of information and instruction is elicited that is most delightful and useful to all—both the older and younger practitioners—the former happy to impart the benefit of their longer experience, the latter as glad to profit by it. Now, would not something of the same kind, at times, introduced in our meetings here, be as much a source of benefit and pleasure as in the other Society? There are many ailments met with in daily practice by the younger practitioner which either do not fall under his observation during his Hospital experience, or which, in private practice, present a type different from that with which the Hospitals or Dispensaries had made him acquainted, and as to which he would be delighted to obtain a clearer knowledge from those who have been longer in general family practice than himself. There are, for example, the great variety of headaches, of constipations, of dyspepsia with its almost innumerable symptoms, of neuralgic, rheumatic, or gouty affections, and a host of other ailments; which, though not in themselves deadly, are yet the source of many of the ills to which flesh is heir, and make up the bulk of daily practice. Might not such practical discussions or conversations be the means of enticing out to the meetings of the Society many of the older members of our profession, when they felt, that instead of making or listening to set speeches, their matured experience could be made conducive to the benefit and comfort of their younger brethren as well as of the general public? And were this really to happen, it is unnecessary to speculate as to the extent to which our meetings would rise, both in estimation and importance.

My last duty, though mournful, and one which will always hold a place in such addresses as the present, is to pay a passing tribute to the memory of such of our members as have been removed from amongst us during the last two years.

There are few here who do not remember the distress—I might say dismay—with which we received the announcement of the death of one so young and esteemed as Dr David J. Simpson, under circumstances that rendered it peculiarly painful. His father, Sir J. Y. Simpson, had been but a week before invested by our Queen with well-merited distinction, and was still receiving the congratulations of the profession and the public. To crown his happiness, his son's start in professional life had been most successful, having shortly before entered on active duty as his assistant, with every prospect of fulfilling the high expectations of which his student life had given promise; for so rapidly was he acquiring, notwithstanding his youth, the confidence of his patients as a skilful and attentive practitioner, that his father had every reason to anticipate for him a brilliant professional career;—when suddenly all these hopes were blasted by his unexpected and early death.

Dr David J. Simpson was born in Edinburgh in 1842—died 14th January 1866.

The next of our members of whom I shall take notice is Dr Craigie, a man of high literary as well as professional acquirements. These he early brought into active exercise, first as a teacher of Anatomy, and then of the Practice of Medicine, and of Clinical Medicine, on which subject he for many years published most interesting and instructive reports in the *Edinburgh Medical and Surgical Journal;* and it is principally by his connexion with that Journal, of which he was for many years sole editor and proprietor, that Dr Craigie's reputation was established. The numerous original papers, reviews, and clinical reports he contributed to it are characterized by a spirit of deep research and intimate knowledge of all branches of his profession; and the same may be said of his two larger works on Pathology and Practice of Medicine, which contain a complete resumé of all the various views and opinions held on these subjects up to the time of their publication.

For many years Dr Craigie was entirely confined to the house from a painful rheumatic affection, which rendered him almost completely lame—an affliction which he bore with great Christian resignation. For some years, however, before his death, his health improved, and he was enabled to resume some professional duties. At this time he was elected President of the Royal College of Physicians, the duties of which he performed in a manner most satisfactory to the College and creditable to himself. Though a man of apparently reserved habits, it was only in appearance; for, possessing a truly genial disposition and genuine kindliness and warmth of heart, he was respected and beloved by all who knew him.

Dr Craigie was born 6th June 1793, and died 17th May 1866.

There is another fellow-member whose cheerful happy smile we miss from our midst, one of my oldest professional friends, Dr James Duncan. Thoroughly educated in every branch of Medicine, he devoted himself during the earlier years of his professional career more especially to Anatomy and Surgery. I may mention, for the benefit of my young surgical friends, a fact perhaps not generally known among even his intimate acquaintances,—namely, that with the view of perfecting himself and excelling in surgery, Dr Duncan, for many years after he had commenced practice, prosecuted practical anatomy most diligently in the dissecting-rooms during the summer months, from six to nine in the morning. The consequence, as might be expected, was that his skill as a surgeon stood extremely high, and had he devoted himself exclusively to that branch, he would undoubtedly have attained the highest position as a consulting and operating surgeon. But he gradually got absorbed into the whirl of a most extensive general practice, which for many years taxed his utmost energies from morning to night. For rest and recreation he, in the autumn of 1866, visited the continent, where he was seized with malignant cholera, under which he sank at Tours, after a few hours' illness. The grief and dismay experienced in Edinburgh at the receipt of the mournful intelligence was widespread and deep, for he was beloved and respected by an immense circle of friends and patients. Dr Duncan filled the office of Secretary to this Society for five years —from 1840 to 1845—and was afterwards member of Council.

Dr Duncan was born at Perth in November 1810, and died 6th August 1866.

It was with deep and genuine sorrow that, early this year, the medical profession in Edinburgh received the intelligence of the death of Dr Scoresby-Jackson from fever, caught in the discharge of his professional duties as Physician to the Royal Infirmary. Though comparatively young, Dr Jackson had already acquired such a reputation as to make it almost certain that, had his life been spared, he would have risen to high eminence in his profession. The work that introduced him to notice as an author was his interesting Memoir of his uncle, the Reverend Dr Scoresby, the well-known author of two first-rate books on the Arctic Seas. He was also author of a work on Medical Climatology; and on the Influence of the Weather upon Disease; on Pharmacology and Therapeutics; and of various papers read before this Society, each characterized by a thorough mastery of his subject. An earnest Christian—in every sense of the word a gentleman—possessed of a fund of literary and scientific knowledge, there is no wonder that he left behind him a large circle of sorrowing friends.

Dr Jackson was born at Whitby 22d October 1833, and died in Edinburgh 1st February 1867.

A bright ornament of our University, Professor Goodsir, one of the greatest anatomists and physiologists of his day, has also passed from among us. I fear we scarcely yet realize the loss that medicine, especially his favourite branch of it, has sustained in his too early death. Energetic, enthusiastic, indefatigable, unremittingly engaged in the earnest investigation of his favourite sciences, he was busy laying up vast stores of information with which the world was one day to be enriched; a day, alas, so far as we are concerned, never to come! Ever engaged in prosecuting some original discovery or adding to the knowledge of what was old, Goodsir, unfortunately for science, postponed publishing till he could lay before the world something perfect—forgetting that perfection is an attribute scarcely attainable in this passing life—and so his stores have perished with him.[1] Let us hope that into the mind of some of his numerous students some germs may have been planted by him which shall yet bring forth much fruit. In 1845 Mr Goodsir was member of Council of this Society; in 1846 he was Vice-President; and was elected President in 1858.

Beloved by a large circle of friends, Prof. Goodsir departed this life after many years of illness, on 6th March 1867, having been born at Anstruther in 1814.

Dr John Black, though he joined this Society when somewhat advanced in life, yet deserves a passing notice from me at least, as a very old friend, for whom I acted as *locum tenens* during a six months' absence from his home so far back as 1833. Dr Black received his medical education in Edinburgh, and joined the Royal Navy in 1809. After serving in various parts of the world, he returned on half-pay, after the war, and settled in Bolton, as physician, in 1820. Active, energetic, and a thoroughly scientific practitioner, he there entered on the duties of his profession with an earnest desire to advance the knowledge of medicine, as well as promote the welfare of his fellow-creatures. As physician to the Infirmary and Dispensary—as president of a local society, of which he was also the originator, consisting of the medical practitioners of Bolton and neighbourhood, where papers were read and cases discussed, in which he took a leading part—Dr Black soon acquired the confidence and respect of his fellow-practitioners, and of the leading families in the neighbourhood. In 1838 he removed to Manchester, when he was appointed Physician to the Manchester Union Hospital, and Lecturer on Forensic Medicine at the Medical School. In 1856, finding that from the effects of advancing age he must retire from active duty, he chose Edinburgh as his final residence, chiefly with the view of enjoying the intercourse of like minded men with himself, and attending the various scientific and literary societies with which our city abounds; and at that time he joined this Society.

Dr Black was a diligent student of the microscope at a time when it was not so much studied as now, and wrote on the Capillary Circulation of the Blood, and the Intimate Nature of Inflammation, so far back as 1825. Besides other professional works, and a host of papers in the various Medical Journals of the day, he was the author of a variety of papers on Geology, Antiquities, and other scientific subjects. He was a member of the Manchester Philosophical Society, Fellow of the Geological Societies of London and Paris, of the Royal Society here, and F.R.C.P., London. So long as health permitted, he thoroughly enjoyed all those literary and scientific pursuits, and friends, for the sake of whom he came to Edinburgh; but latterly, symptoms of softening of the brain supervened, which gradually became aggravated, and under which he sunk.

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[1] Since this address was delivered I have learned with pleasure that some of Prof. Goodsir's Essays are to be published immediately, in 2 vols. 8vo,—information which I doubt not will be received with delight by his numerous pupils.

Dr Black was born at Newton-Stewart in 1788, and died in Edinburgh, 30th April 1867.

For some years we have missed from amongst us a professional brother, much respected by all who had the privilege of his acquaintance—one whose carriage and bearing in every repect well bore out the fact of his connexion with the army, and whose cheerful, happy disposition ever made him a welcome guest and companion. Dr Pagan commenced life as a soldier—was wounded at Waterloo—in 1821 exchanged the service of Mars for that of Esculapius—and, after finishing his studies, practised his profession in Edinburgh with marked success and acceptance. For about four years before his death, Dr Pagan was laid aside from active professional work by creeping paralysis, the result of disease of the upper part of the spinal marrow—his mental faculties remaining entire. He was on two different occasions in the Council of this Society, viz., in 1843 and 1854, and he was Vice-President in 1853.

Dr Pagan was born in this city 22d July 1793, and died 23d Sept. 1867.

I have to record the removal of still another of our members—Dr Young—one of the oldest practitioners in Edinburgh. He for many years enjoyed the respect and affection of a large circle of patients and friends; and his wiry, active appearance, up to a very short period before his death, seemed to give promise of still many years of professional usefulness; but, early in October, he was attacked by peritonitis, under which he gradually sunk.

Dr Young was born at Kinross in 1797, and died 17th November 1867.

During the last two years there has been an accession of 14 new members, so that though our loss has been great, the number of members has been more than kept up; and when I look around me, I feel satisfied that there are many here—some of them still comparatively young men—who from the zeal, ability, and talent they have already displayed, will assuredly add much to our stores of medical knowledge—will not only maintain, but extend the reputation of our medical school, and will render your meetings here as full of interest and instruction as they have ever been in the most prosperous days of this Society.

I have, in closing, to express my regret that I have detained you so long, and have again to return you my hearty thanks for all your kindness, and to introduce to you, as my successor, one under whose presidency I sincerely trust you will have very many pleasant and instructive meetings.

PROCEEDINGS OF THE EDINBURGH OBSTETRICAL SOCIETY.

SESSION XXVII.—MEETING III.

11*th December* 1867.—Dr Burn, *President*, in the Chair.

Dr George Dickson was elected an ordinary Fellow of the Society.

I. *Dr Andrew Inglis* exhibited an inverted uterus, which had produced death by bleeding, eight days after the accident. The patient had been attended in her confinement by a nurse, who had with much difficulty removed the placenta *piecemeal;* vomiting came on soon afterwards, and for a week she had alternate attacks of bleeding and vomiting. At the end of this time, the nurse thought it necessary to send for a medical man, to take away the rest of the placenta, which, on examination, turned out to be the inverted uterus. The case was then hopeless, and the patient died shortly after his visit. A post-mortem examination was obtained, and the uterus was found to have four holes on one side, and one on the other, corresponding, it was supposed, to the fingers and thumb of the nurse. A portion of the fundus had also been picked off. The case was prevented from becoming the subject of a medico-legal examination by the fact that the patient, after delivery, had refused to allow a medical man who was in the house to make any examination.

II. *Dr Inglis* likewise showed a large piece of membrane containing two placentæ—one apparently a six-month one, and the other a four-month one—which had been expelled after the birth of an ordinary full-sized child and placenta. A six-month fœtus, very much decomposed, was attached by a cord to the larger placenta, but no trace of any fœtus could be discovered in connexion with the smaller one. Dr Inglis considered that the patient had become pregnant of triplets; that one had become blighted in the early months, and no trace of it remained save the placenta, which had increased in size to a certain extent; the second fœtus had died about the sixth month, but was not expelled; while the third had gone on to the full time, and was born alive and healthy.

Dr Burn said that he had, on one occasion, exhibited to the Society a preparation consisting of two distinct placentæ, being the first he had ever seen.

III. *Dr David Gordon* exhibited a placenta from a twin case, in which the vessels of the two placentæ anastomosed, showing the propriety of tying both ends of the cord.

IV. CASE OF CEPHALOTRIPSY. BY DR ANGUS MACDONALD.

About ten o'clock P.M. on the 14th of January last (1867), I was called by Mr Maclean, a pupil of the New Town Dispensary, to see Catherine Macdonald, residing in Morrison's Close, High Street. Dr Inglis, at the time, was suffering from an abscess of the palm, and was scarcely able for duty. The patient was 34 years of age, unmarried, a primipara, and presented in her own person almost an encyclopædia of pathology. Having sustained an injury behind the left ear about fourteen years previously, she had, as results of it, a marked cicatrix, paralysis of the left facial nerve, and complete loss of hearing on both sides. The loss of hearing, however, did not supervene immediately on the occurrence of the injury, but followed after the lapse of a year, and it remained persistent. Besides this, she suffered from double femoral hernia, from bronchitis, and had albuminuria. Previously to the time I saw her, she had been in labour for upwards of twenty hours. There had been a somewhat profuse discharge of a red colour during the whole of that time, but it was not fetid. Mr Maclean had not been able to hear the fœtal heart for two days, though he had previously heard it distinctly; and on examining for it, I also failed to detect it. The uterine tumour felt particularly hard, round, and nodulated anteriorly. On examining per vaginam, the external parts were ascertained to be well dilated, cool, and moist; and the pelvis well formed and roomy. But the whole of the cervix uteri was replaced by a mass of cartilaginous, or rather of almost horny hardness. The os uteri was dilated from the size of half-a-crown to that of a crown-piece. Its outline was irregular and uneven. The head was suspected to present in the right occipito-posterior position, which suspicion was proved correct by what followed. The patient was put under the influence of chloroform, and I tried to dilate the os with the finger; but this proving impossible, I notched the cervix slightly in two or three places, with the aid of a probe-pointed bistoury, to try whether, after this, the uterine action, which was powerful, might not be able to complete the dilatation, and before doing anything else, went and communicated the facts of the case to Dr Inglis, who, after providing forceps and also instruments for cephalotripsy, accompanied me to the patient's house. Finding, on my return, matters in almost the same condition as when I left the patient, and Dr Inglis, on examination, fully corroborating the previous statement as to the condition of the parts, we thought it best to enlarge the incisions, and endeavour to deliver by turning. Chloroform was accordingly administered, the bladder emptied, and the incisions enlarged, more especially laterally, so as to avoid wounding the bladder. After some time I was able to introduce my hand through the os uteri; but, partly owing to the obstruction caused by the cancerous mass which filled the pelvis in front, and which corresponded with the anterior half of the cervix uteri, and partly in consequence of the spasmodic condition of the uterus, I was unable to get hold of a foot, the only part of the uterine cavity my hand could reach being a portion of the posterior wall. Failing to turn, after attempting it for some time, forceps were next applied, both by Dr Inglis and myself; but, owing to the same obstructing mass in the front part of the pelvic cavity, it was impossible to apply the second blade so as to get the forceps to lock, and they consequently slipped backwards every time. We tried them three or four times.

Having been equally unsuccessful in our attempts to deliver both by turning and forceps, we resolved to have recourse to cephalotripsy. It was now two o'clock A.M., and we were beginning to fear that, if delivery were not effected soon, our patient's strength would fail her. Besides, since the fœtal heart had not been heard for some days previously, we had reason to believe the child was dead, and, consequently, had no reason to delay the extreme operation on the ground of preserving the life of the child.

I had no difficulty in perforating the head, but experienced the same difficulty in applying the cephalotribe as had been previously felt in introducing the forceps. The masses of cancerous matter, both before and behind, inside the pelvic cavity, rendered it very difficult indeed to get the blades of the cephalotribe so opposed as to lock, and at the same time to grasp the head. After repeated trials and failures, both on my own part and on that of Dr Inglis, who, I ought to state, laboured under serious disadvantage, from the abscess in his right palm, we succeeded in getting a firm hold with the cephalotribe of the occipital portion of the child's head, along with a portion of the cervical region of its spine. Then, by the exercise of a considerable degree of force on my part, and for a considerable length of time, Dr Inglis and Mr Maclean maintaining counter-extension of the pelvis, the head and body of the child were pulled through the passages.

The difficulty experienced in extraction was clearly not due to any deformity of the pelvis, which was roomy and well formed, but to the obstructing mass at the os uteri, and partly to the oblique hold which the instrument took of the child's head. The placenta came away about five minutes after the child, and there was no hæmorrhage whatever post partum, nor was there the slightest injury to the passages discoverable, though these were sought for carefully. The patient was able to retain her water, as well as pass it voluntarily, though, to save her trouble, Mr Maclean passed a catheter occasionally for two days after delivery. It was accurately determined that none of the incisions made in the cervix had passed into the body of the organ.

On the morning after delivery, the pulse was 100, small, but of pretty fair strength.

On the 16th, the patient's cough began to be more troublesome, and pain to be felt in the right iliac fossa and in the hypogastric region, aggravated by pressure. The discharge was profuse, with an intensely disagreeable odour; pulse still 100 per minute, but respirations rather hurried.

The patient continued pretty much in this condition, but getting gradually worse, more especially suffering from aggravation of her bronchitis, till half-past ten o'clock on the evening of the 19th, when she expired.

Her friends allowed the abdomen to be opened, which was done by Mr Maclean and myself.

On opening the abdomen, a quantity of gas escaped. There was seen a slight inflammatory reddening of the peritoneal surface of the uterus, on its right lateral aspect, and this redness extended slightly outwards towards the right iliac region. Otherwise, nothing was to be seen, either on the uterus or its appendages. I had the uterus taken, untouched, to Dr Inglis, who expressed a wish to examine it; but the further examination of it was prevented by the accidental circumstance of its being put into a vessel containing too little spirit and a considerable portion of it was thus allowed to decay. It, however appeared, externally, of normal size, and healthy, and was certainly without material injury, either internally or externally. The kidneys were enlarged and looked fatty.

As the cause of failure, in so far as regards the particular form of cephalotribe employed, is to be commented upon by Dr Inglis, it is unnecessary for me to say anything on that subject. But I would invite the special attention of the Society to the very great difficulty experienced both in the introduction of the hand to effect turning, and in the introduction, or rather application, of instruments, when the pelvis is filled both anteriorly and posteriorly, as happened in this case, with a hard unyielding cancerous mass. I am firmly convinced that the proper treatment to adopt in a case of the kind, is a method suggested, if I mistake not, by Sir James Y. Simpson, namely, to remove the whole of the cervix, or, more properly, the whole of the cancerous mass which has replaced it. In that case, I feel assured that the labour would have been completed much more rapidly; and the mother's life would certainly not have been a whit more endangered than it was by simple incision. It is likewise worthy of particular notice, that incision of the cervix led in this case to little or no bleeding, nor does it do so, I believe, as a general rule, in such cases. It is also noteworthy, that the incisions made into the cervix presented no tendency to pass from it into the body of the organ, during the distention of the parts consequent on the passage of the child through the os uteri. It would almost appear that the danger of this accident, though real, is very much exaggerated in books. In this opinion we are borne out by the almost constant rupture of the cervix in primiparæ, which ruptures rarely, if ever, do any harm, or pass farther than the cervix. There seems to have been no uterine cause for this woman's death. The peritonitis, which was most probably a consequence of the labour, was trifling, and seemed to act chiefly by diminishing still farther her already too limited breathing power.

But when we remember that this poor woman laboured under severe bronchitis, that she had persistent albuminuria, that her external circumstances and surroundings were of the worst possible character, we need not be astonished though she succumbed five days after such a serious operation.

Dr Pattison remarked that there was a great difference between ruptures and incisions, the one being a tear, the other a clean cut.

Dr Inglis stated, on the authority of Professor Martin, that incision was the regular practice in Vienna, in first cases.

Dr Burn had met with several cases in which the os was cancerous, and in one patient the cancerous mass came away. He had, however, never met with cancer in a first labour.

V. ON CEPHALOTRIPSY. BY DR ANDREW INGLIS.

On the 11th January last I was sent for by a pupil of the New Town Dispensary to see a patient who had been long ill, and had not made satisfactory progress. She had been about thirty hours in labour. I found her delirious, the pulse about 120, and the tongue dry and furred. The abdomen was very hard, tense, and irregular in form. The uterine tumour was double, the main bulk of it ascending to the right side; on the left there was an oval mass, extending from the pubes to a level nearly two inches above the umbilicus. Between this smaller mass and the main uterine tumour was a well-defined constriction like a band of contraction running obliquely from near the short ribs on the left to the middle of Poupart's ligament on the right. The lower tumour was nearly in the position, and of the size and shape of the bladder when over-distended during labour, except that its lower part was too much to the left; moreover, it seemed solid. The limbs of the fœtus could not be felt through the abdominal walls, but its heart-sounds were good. On passing the catheter, the bladder and urine were found to be all right, but as she had made some water not long before, there was not much drawn off, nor did its withdrawal seem to alter the position of the parts. The vagina was tolerably cool and moist, the os all but open, and the presentation seemed good, the occiput being at the right groin, but very high up. The pains were excessively severe, and came at long intervals, making no impression on the head, but the lower uterine tumour came down a little, and ascended again during each contraction. I had brought a pair of my very small forceps with me, which I applied at once. The left blade was rather difficult to introduce, for after the point had nearly reached the ear, it struck against a rounded projection, something like a shoulder of the fœtus. This difficulty was noticed the first time the blade was put in, and did not give trouble at subsequent introductions. The right blade, however, went in as easily as possible, and the only difficulty experienced in locking was from the shortness of the instruments, the soft parts being apt to get nipped in the joint. A gentle pull caused the os to be obliterated, but no force that I could use during the height of the pains could bring the head so far into the pelvis as to make it remain there, and the moment traction ceased, it resumed its original position. Manipulation of the smaller abdominal tumour had no effect. Pulling was then tried in the intervals between the pains, hoping that then the constriction might relax. A new hold of the head required to be taken when it was down as far as it could be pulled, for the blade nearest the face had a tendency to come off when the handles were directed forwards, and many attempts were made to obtain this second hold, but, as the head always retracted to the same place, this was quite impossible. Traction was next tried, until the forceps came off, in expectation that perhaps the head might get fixed before they slipped, but this also failed. After having persevered for two hours with my own short forceps, I next tried Sir J. Simpson's, for fear that I might have trusted too much to my own pattern (which had at that time only been tried about a dozen times), but the result was equally disappointing. In fact, my own held more firmly, and a quarter of an hour or twenty minutes of the second pair was enough to convince me that some other plan must be adopted. Accordingly turning was tried, but the general hardness of the uterus made that quite impossible. Nothing but embryotomy now remained. After perforating the head as near the occiput as possible, the left blade of Dr Charles' cephalotribe was placed carefully over the face, and the right directly over the perforation, the screw was applied, and the head at once reduced. As the instrument was tolerably straight, it was easily twisted round in the pelvis, so that the most prominent part of the head was made to fill the hollow of the sacrum, and little traction was required to bring it out. The extraction of the trunk, however, was rather more difficult, as its circumference was greater than that of the diminished head. The placenta was expelled immediately, then a little bleeding followed, the uterus for a few seconds relaxing and filling with clots, but at once contracting again, almost of its own accord, and expelling them. After delivery the pulse fell to about 100, but the abdomen seemed to be very tender. The following morning she was very much better, and examination of the abdomen, which was not painful, showed that the tumour was a large fibroid one, springing from the lower part of the body of the uterus on the left side, and occupying the position already described; its lower edge was thinner than the upper, and descended into the pelvis. The uterus was pushed over to the right side, but was of the usual size and properly contracted. She required no after-treatment, except for her breasts, when the milk came, and in fact, with this one exception, her recovery was quite normal. Her history is short. She was five years married before her first child was born, and the delivery was severe and protracted; fifteen months after she was confined again, but this time without difficulty. It is now seven years since the birth of her second child, and she has got very stout in the interval, but she was not aware of the existence of the tumour.

Two days after, I witnessed another case of cephalotripsy with Dr Macdonald, and learned much from the difficulties met with. In the first case everything went well, though the contraction seemed nearly, if not as great, judging from the comparative difficulty in the extraction of the bodies, and in it Dr Charles' pattern was used. The blades being tolerably concave, were easily inserted, and as they had not much antero-posterior curve, they were easily placed, one near the sacrum, and the other near the pubes. Their concavity, and the fact that they were armed with teeth, prevented the head from slipping out to either side when the compression was applied; and, besides this, the concavity allows the whole blade as well as the tip to act, so that a lighter instrument can be used. But though curved towards one another, the pelvic or antero-posterior curve is slight, and the tip of the blade well rounded. The advantage of this arrangement is, that, should the head bulge to the front of the blades, by a little management, the part bulging can be rotated into the hollow of the sacrum. In the subsequent case we used Sir James Simpson's cephalotribe, the blades of which are parallel, turning towards one another abruptly at the points, so that the end of each presents a broad surface for introduction at least half an inch thick and an inch long, instead of a thin curved slip of metal. This made the introduction very difficult, and, along with the great antero-posterior curve, prevented the left blade from being applied between the pubes and the head. As the blades were not concave, and did not fit the convexity of the head, when the screw was applied the head slipped out from between them, and the want of teeth also facilitated this slipping. The great antero-posterior curve I mentioned was also found to be an objection of some magnitude, and the squareness at the tip heightened this fault, for when the head bulged out in front of the blades, on account of the slipping, no management could bring the bulge into the hollow of the sacrum.

Dr Inglis exhibited the instruments which had been used in both cases, and remarked, that since his observations on the comparative value of Dr Charles' and Sir James Simpson's cephalotribes were written, Sir James had brought out another instrument, which approached so nearly to the form of Dr Charles' that at first he hesitated to comment on the merits or demerits of an instrument which might be considered as now superseded; but, so little was yet known of cephalotripsy, that he determined still to bring his views forward, in the hope that they might furnish a little information on a subject which as yet had not been fully worked out.

Dr Burn had seen Dr Keiller deliver a woman in the Maternity Hospital, whose sacrum jutted in, by means of Dr Simpson's instrument, and he had great difficulty in holding on by it. The woman recovered, although she had been in labour for three or four days.

Dr Milne had seen Dr Simpson operating with his own instrument easily. He thought the new one was an improvement on the old, and would be more easily introduced; its crushing power, too, he believed, was greater than that of Dr Charles'.

Dr Peter Young remarked that Scanzoni's instrument was a much more formidable one than either of those exhibited, which, he believed, were incapable of breaking up the base of the skull, especially in a case of contracted brim. It was easy to crush the flat bones, but very difficult to break up the base, so much so that Sir James Simpson had invented an instrument to effect this bit by bit. Dr Young thought Dr Charles' instrument was too light and not sufficiently elongated, and stated that the advantage of the sacral curve was that it admitted of the instrument being used when the patient was on her back, the position in which the Germans always operate.

Dr Inglis maintained that Dr Charles' instrument was quite strong enough: the blades were concave like the blade of a spoon, thus combining great strength with lightness. If it did not produce fracture, it could crush the base of the head into as small a compass as if the bones were fractured. The great sacral curve was objectionable, he thought, as it prevented the blades from being applied in the antero-posterior diameter, which he had considered the best in both the cases just related. In the one case the small straight cephalotribe succeeded, while in the other the curve of the instrument prevented it from succeeding.

Dr Bryce had seen Sir James use Scanzoni's cephalotribe in a case of contracted pelvis with the head above the brim. It was a long, thick, powerful instrument, and was introduced with great facility, Sir James not having been more than twenty minutes in the house altogether. Dr Bryce thought there was still room for improvement in the form of the cephalotribe, but did not consider that the actual thickness of the blades was the chief objection.

Dr Bruce suggested that the crushing power of the several instruments should be tested on the dead body.

Dr Macdonald, with the pelvis and model fœtus, explained the manner in which the cephalotribe was applied in his case, and stated that the patient died, in his opinion, from bronchitis, although there was also peritonitis to a slight extent.

There being no further public business, an animated conversation took place on the value of sulphurous acid as a remedial agent. The opinions expressed were conflicting, several members declaring that they had witnessed the most decided benefit from its use in diseases of the respiratory organs, while others maintained that in their experience its administration had been productive in some cases of no good, and in others of injurious results.

DUNDEE MEDICAL SOCIETY.

At a monthly meeting of the Society, held on Wednesday, 8th January—Dr Gibson, *President*, in the chair—Dr J. W. Miller read an interesting paper on the Thermometer in Typhus and Enteric Fever.

Part Fourth.

PERISCOPE.

MEDICINE AND SURGERY.

[In a recent number of the *Journal* we had occasion to notice with commendation the first volume issued by the Staff of St George's Hospital, and we beg now to lay before our readers the following condensed summary of some of the chief papers contained in the second volume of the series.]

THERMOMETRICAL OBSERVATIONS IN TYPHOID FEVER. BY DR R. E. THOMPSON.

Dr Thompson's observations extend over a period of three years, and are made from a careful study of forty-seven cases. They tend to confirm Professor Wunderlich's researches on the same subject. In typhoid fever, a decrease of temperature is not always a favourable sign, nor is a rapid fall symptomatic only of a crisis. As a rule, the nocturnal increase of temperature in this form of fever is very considerable, and amounts to two or two and a half degrees. During the latter half of the first week, the heat increases day by day, and varies between 102° and 103° in the morning, and 104° or even 105° in the evening. These high temperatures are quite sufficient to distinguish typhoid fever from tubercular meningitis or from peritonitis, these diseases seldom showing much increase over 102° Fahr. During the second week, the temperature varies between 102° in the morning, and 103° and 104° in the evening, the oscillations being influenced by the amount of diarrhœa. The thermometer does not enable one to say, by a longer prediction than twenty-four or forty-eight hours, whether the case is likely to be fatal or not; but a steady rise in the temperature will often indicate the danger of ulceration of the bowels twenty-four hours before the intestinal lesion is manifested by diarrhœa and hæmorrhage. The mode of termination of the fever is characterized by extraordinary oscillations in the temperature between morning and evening, the difference being sometimes as great as nine degrees. This feature distinguishes the thermograph of typhoid fever from that of almost all other diseases. In one case of perforation of the bowel, which was preceded by hæmorrhage, the heat of the body was reduced from 102° to 99·5°, and was followed by a rise to 102·2°, where it remained for forty-eight hours, before death, during which period perforation took place.

APHASIA AND AGRAPHIA. BY DR WILLIAM OGLE.

The object of this interesting paper is to examine how far the pathological records of our hospitals harmonize with the doctrine, that the faculty of articulate speech is inseparably connected with a certain limited portion of the left hemisphere, namely, the posterior part of the inferior or third frontal convolution.

It is stated by Broca, that the portion of the brain in question has never yet been found in a state of disease, without impairment of the faculty of articulate speech, or, as it is now termed, without "aphasia;" and that a like statement cannot be made of any other portion of the convolutions. Secondly, it is also stated that true aphasia never occurs unless either this part be itself diseased, or those portions of white matter by which it is connected with the parts below. The former of these two propositions Dr Ogle believes to be uncontradicted. There is no case, so far as he knows, on record, in which disease of the posterior part of the third frontal convolution on the left side has co-existed with integrity of speech. Still, it is not impossible, he thinks, that such a case may arise, and such an exception would not be a sufficient refutation of the theory. There is good reason to believe, he says, that in some few individuals it is on the right side, and not on the left, that the central organ of speech is situated, and, therefore, no single case would be a perfect refutation of this first proposition, excepting one in which there was a lesion both on the right and left side.

The second proposition is more open to question, for we do not know with precision how the fibres run which connect any given portion of the convolutions with the organs below. A series of most interesting cases is given in illustration of the subject, and in all of them, twenty-five in number, speech was lost or much impaired, while, at the same time, every one of the patients had disease of the left hemisphere. In the registers of St George's Hospital, there is not one single case in which speech was seriously impaired and the left side of the brain found sound. The left hemisphere, then, being specially concerned in speech, the next question is, Are all the parts of it equally so, or is this faculty located in some limited portion of it, as Broca would have us believe? Dr Ogle thinks that the organs of language are, if not in the exact position which Broca has assigned to them, at all events in close proximity to it; and this opinion is borne out by the post-mortem examinations in the cases quoted in this paper.

INFECTING AND NON-INFECTING CHANCRES. BY EDGCOMBE VENNING, ESQ.

Mr Venning has had a large experience of syphilis amongst the soldiers of the Life Guards, and seeks, in this paper, to point out some aids to diagnosis, in distinguishing between the two forms of primary sores, viz., that which is followed by constitutional symptoms, and that which is only a local disease. In the simple non-infecting chancre, he merely gives the iodide of iron, and uses some local application. In answer to the query, Is there any symptom by which we can, in all cases, recognise the presence of an infecting chancre? Mr Venning says, that while induration may exist without the sore being infecting, the result of his own observation seems to prove that the infecting form is always attended with the amygdaloid enlargement of the glands in the groin. He does not remember of ever having seen a case in which this symptom did not exist.

ON CROUP AND DIPHTHERIA. BY J. W. HAWARD, ESQ.

Is it correct to speak of croup and diphtheria as two diseases, or are the conditions to which the name of "croup" and "diphtheria" are applied, merely variations of the same disease? Mr Haward, in answer to this, states that it is his belief that there are two distinct combinations of symptoms, with their associated pathological changes, to one of which may be given the name of croup, to the other, diphtheria; that these combinations are constant, and that the elements or terms of the one are never mixed with the elements or terms of the other (though there may be certain added quantities which are common to both); that the constant combinations are of the important and essential elements in each quantity; and that these always occurring in the same and distinct association, there is sufficient ground for regarding each of the two diseases made up of these elements as distinct and non-identical.

Part Fifth.

MEDICAL NEWS.

CONVERSAZIONÉ AT THE ROYAL COLLEGE OF SURGEONS.

The first Conversazioné of the season was held at the Hall of the Royal College on the evening of Friday the 17th January. The company was received by Professor Spence, the President. A lecture on the "Atmospheric Germ Theory" was delivered by Professor Bennett, who detailed several series of experiments in opposition to the views of M. Pasteur. The lecture will appear in an early number of this Journal.

GOODSIR FELLOWSHIP—SUBSCRIPTION LIST *continued.*

Dr Millington, Wolverhampton,				£2	2	0
Per Dr Ivor Murray, Hongkong, (*second list*):—						
Dr Carnegie, Amoy,	$10	0	0			
Dr J. R. Somerville, Foochow,	10	0	0			
Dr J. A. Stewart, do.,	10	0	0			
C. M. Scott, Esq., Swatow,	5	0	0			
J. Dunwoodie, Esq., Assistant-Surgeon, R.N.,	5	0	0			
Dr William Patullo, Yokohama,	5	0	0			
Dr A. B. Messer, Surgeon, H.M.S. "Perseus,"	10	0	0			
Dr John Craw, Assistant-Surgeon, do.,	5	0	0			
Dr G. Monteath, H.M.S. "Basilisk,"	5	0	0			
Dr J. S. Coghill, Shanghae,	25	0	0			
Dr James Johnston, do.,	15	0	0			
Dr Sibbald, do.,	25	0	0			
	$130	0	0	27	2	10
A Pupil's First Fee, per J. Simpson, Esq., North Bridge,				1	1	0
Per Dr J. Abercrombie, junr., Capetown:—						
Dr J. Abercrombie, senr.,	£5	0	0			
Dr Alex. Abercrombie, Wellington,	1	1	0			
Dr Wright of Wynberg,	1	1	0			
Dr Landsberg, Capetown,	1	1	0			
Dr J. Abercrombie, junr.,	5	5	0			
				13	8	0
Dr Scholfield, Doncaster,				1	1	0

PUBLICATIONS RECEIVED.

W. Addison, M.D.,—The Co-existence of two Species of Inflammation. London, 1868.

W. G. Barker, M.B.,—On the Climate of Worthing. London, 1867.

I. Baker Brown,—On Ovarian Dropsy. London, 1868.

Brown-Séquard's Archives, No. 1. Paris, 1868.

James Dewar, M.D.,—On the Application of Sulphurous Acid. Edinburgh, 1868.

J. Matthews Duncan,—Researches in Obstetrics. Edinburgh, 1868.

Wilson Fox, M.D.,—On Dyspepsia. London, 1867.

Robert Galloway,—The First Step in Chemistry. London, 1868.

Half-yearly Abstract of the Medical Sciences. London, 1868.

Dr August Karst,—Die optischen Fehler des Auges, von J. Z. Laurence. Kreuznach, 1868.

W. J. Little, M.D.,—On Spinal Weakness. London, 1868.

Rawdon Macnamara,—Neligan's Medicines. Dublin, 1867.

John Marshall,—Outlines of Physiology: 2 vols. London, 1867.

The Medical Directories. London, 1868.

Robert Pairman,—The Great Sulphur Cure. Edinburgh, 1867.

S. Parsons Shaw,—Odontalgia. Manchester, 1868.

U. J. Kay-Shuttleworth,—First Principles of Modern Chemistry. London, 1868.

Henry Smith,—The Surgery of the Rectum. London, 1868.

Part First.

ORIGINAL COMMUNICATIONS.

Article I.—*Two Cases of Epilepsy.* By G. W. Balfour, M.D., F.R.C.P.E., Physician to the Royal Infirmary of Edinburgh, Lecturer on Practice of Medicine and on Clinical Medicine.

Cases of epilepsy are at all times fraught with the deepest interest to the medical man. The two I am now about to narrate are specially interesting, from the fact that both were under the constant observation of one medical man during the whole course of the disease—one of them for the long period of thirteen years—so that they present tolerably complete histories of the whole series of morbid phenomena, supplemented by the pathological cause, as ascertained after death; and illustrate, in a very striking manner, the ameliorating influence of remedies of a certain kind, even in cases which, from the nature of the cause, were certainly hopeless.

Case 1.—A. M., a female, æt. 36.—*Family history.*—The patient's father died some years ago from the effects of a fall, aged about 55. Her mother is still alive, and very robust, though for long ailing with chronic irritation of stomach and intestines subsequent to an attack of cholera. One brother died of consumption at the age of 11, and another from rupture of the middle meningeal artery, the result of a fall, at the age of 11½ years. An illegitimate brother by the father's side died from paraplegia, the first symptom of which was *retentio urinæ.* The only surviving member of the family, except the mother, is a sister, whose catamenia have never been regular—indeed scarcely ever present. She has no family, though long married, and is now about 45 years of age. Her health has always been delicate, and last year she presented some peculiar mental symptoms, depending probably upon her time of life; they yielded at once to full doses of bromide of potassium, and have not since recurred. A. M. herself was always more or less irregular in her catemenia as to time and quantity. She had a well grown beard, whiskers, and moustache, like a young man, and for years shaved regularly. She was, however, always healthy from her earliest youth, and to all appearance robust, with a ruddy complexion. About 8 P.M. on the 28th of April 1864, while selecting some eggs for a customer, she suddenly felt her left hand convulsively closed—as if by "the grasp of death," as she expressed it—so that the eggs in it were crushed. She called out "Oh, mother!" and fell insensible on the floor, cutting her head slightly. Her countenance was said to have become pale; she had slight convulsive movements of the extremities; and was unconscious for half an hour. I saw her about 9.30 the same evening: her extremities were slightly tremulous, her heart's action feeble, her tongue red, and the papillæ swollen and elevated (strawberry tongue); there was slight paralysis of motion and sensation, which continued for some hours. The nature of the fit was doubtful, and the treatment employed consisted in rousing her out of her depressed condition by repeated small doses of brandy and the application of a mustard poultice to the epigastrium; followed, as the bowels had been constipated for some time, by a dose of castor-oil next morning. Subsequently the bowels were regulated by aloes and iron, and the tone of the stomach improved by bitter infusions acidulated with nitric acid. Her health continued to improve somewhat till the 23d of May, at 9.30 A.M., when she had another fit. Her face was pale, there were slight clonic convulsions, and paralysis of her left arm persisted for about three-quarters of an hour; pulse very feeble. There being now little doubt as to the character of the fits, and as they seemed to originate in the stomach and bowels—for though no aura could be traced, they were the only organs at all deranged—and to be somewhat periodic in recurrence, she was placed upon full doses of oxide of zinc and quinine, given in the form of pill several times a day, with plenty of nourishment, and wine and brandy occasionally. For the most remarkable fact in the history of this case, was the sudden and extreme prostration which followed even the first attack, and increased after each subsequent one, so that an apparently stout and healthy young woman was rendered unable to walk more than twenty or thirty yards without feeling faint; and was, after only a few fits, compelled to keep the house altogether. On the 13th of June, at 3 A.M., while still in bed, she suddenly gave two loud cries. These were followed by convulsive movements and slight numbness in the left arm, which remained for some time, but there was no loss of consciousness. On the 7th of July, at 11.30 A.M., in the very act of telling me that she was much better, she suddenly screamed out, and fell into clonic convulsions, gradually becoming insensible and livid in the face. There was no frothing at the mouth, and the seizure passed off in about ten minutes, leaving her stupid and dull. The left side was most convulsed. On the 13th of July a seton was put into the nape of her neck, which was removed on the 16th of August, during my temporary absence from home, because she had at least two fits every week, all violent, since its introduction. I regretted its removal, because I fully acquitted it of all blame in regard to the numerical increase of the fits, while I hoped for aid from it in the further treatment of the case; and yet, from her nervous dread, I could not propose a re-introduction. The patient was now subject to a continual subsultus, as it were, of all the muscles of the left arm; her catamenia were regular, but small in quantity. She was now placed upon pills, each containing quarter of a grain of belladonna and half a grain of sulphate of iron, three times a day. She continued much quieter, without any fits, and with much less subsultus up to the 19th of August, after which it ceased, till the morning of the 24th August, when she took a slight fit, having had a quiet natural sleep the night before. Her bowels, which had latterly been natural, now became loose, so that the belladonna had to be combined with tannic acid; and this, in return, constipated them so much that it required (September 17) four grains of the compound colocynth pill mass, with quarter of a drop of croton-oil to open them; and this had to be repeated a second time. The tannin was now omitted, and the belladonna increased to the extent of one grain, thrice a day, combined with quinine, her pupils being thus kept constantly dilated. Her fits no longer produced unconsciousness, but she was much weaker, hardly able to be out of bed at all; and the fits, though less severe, were much more frequent, coming on whenever she wakes during the night, and always recurring several times during the day. Early one morning in the end of September, one very violent fit occurred; and, subsequent to this, the left arm and leg were found to be completely paralyzed as to motion, though not as to sensation; while the left arm felt warmer than the right one, both to herself and others. The leg shortly regained its power of motion, and the hand was following suit, the thumb and forefinger having recovered the power of motion, and the other four fingers seemed commencing to regain it, when the whole hand and arm suddenly began to be seized with continually recurring clonic spasms, which were certainly brought on by any attempt at voluntary motion, though they constantly recurred independent of that. Next, the hand began to be the seat of acute pain, although its position was carefully attended to, and the circulation through it continued both good and natural The slightest movement of the body gave her a feeling of sinking, and was liable to bring on fits, which were never accompanied with unconsciousness, although the convulsions were generally violent. Feeling convinced that the gradual progression of the symptoms was very probably connected with the gradual growth of a tumour, I determined to give her what little chance of its absorption might be derived from a gentle mercurial course. I therefore stopped her belladonna and quinine pills, and put her upon twenty drops of the *liquor ergotœ* three times a day to regulate the circulation, and gave her also three grains of *Hyoscyamus*, and one grain of the *pilula Hydrargyri* mass every night. Under this treatment the convulsive movements of the hand decreased, the fits became less numerous, and never seized her at night, so that she slept well, but she gradually lost strength. On November 10, she was seized with vomiting, which lasted for three days, and was subsequently liable to be brought on by everything but brandy and a little beef-tea or bread-berry, in small quantities. Her gums were slightly touched, and her bowels constipated, but she could retain no medicine. She had violent headache for several days; constant yawning and feeble pulse. She slumbered constantly, and her fits were very slight and very infrequent. Her arm still remained paralyzed, and occasionally affected with slight spasmodic convulsions (jumpings), but not so frequently as formerly. The pills and ergot were both stopped, and nothing but brandy and nourishment administered. On the evening of the 15th of November, she had two severe fits; her bowels were still confined, having now been so for an entire week. The headache, from which she suffered much formerly, when her bowels were constipated, was again constant and racking; her mind quite clear; her pulse slow and of moderate strength. She has been taking a little more food the last day or two, and was ordered to take a croton-oil pill, as formerly, at bed-time. On the 17th of November, it was found that she had vomited three hours after taking the pill; her bowels were not moved; the fits had not recurred, but her head was from time to time spasmodically jerked with great violence; her severe headache continued, yet she was constantly slumbering, though perfectly sensible when spoken to; her pupils were natural; her pulse slow, regular, and of moderate strength; she lay sunk in bed, and made no complaint of the paralyzed arm, which lay quite motionless. Pill to be repeated. November 18.—Her bowels were freely moved once last night; she had slight general convulsions about 5 A.M. this morning; her head was frequently spasmodically jerked, but without disturbing the somnolent condition in which she lay. She was, however, perfectly sensible when roused; her pupils were natural; her headache continued; her pulse was 64, and very feeble. On the 19th, she had slept soundly all night, and appeared stronger and more comfortable; her pulse fuller and of moderate strength; she had a slight tremulous fit this morning. She takes only a little wine and beef-tea. Bowels confined; to have another pill, as formerly, to-morrow morning. November 21.—Pulse of fair strength; face flushed; headache very severe; not apparently increased by noise, to which, though perceived, she was not sensitive, and cold applications gave merely temporary relief. She was perfectly sensible when spoken to, but wandered a little and was forgetful. Her bowels had not yet been moved. Her muscular system was very much reduced. Her paralyzed hand was still very painful; she had two pretty bad fits during last night; her eyes continued natural as to appearance and state of pupil. December 9.—She required several pills before her bowels were moved. She continued to slumber and to wander during her short waking intervals for about a fortnight; this day she was lively and felt better; her headache better, her bowels regular, her fits frequent but very slight, hardly more than a mere passing tremor. She had slept but little for the last day or two, and was taking more food. She lay upon her side, her back being slightly abraded. She continued much in the same condition until the evening of December 20, when, after taking two teaspoonfuls of porridge, she suddenly seemed to faint away, and had to be laid down, as if asleep, but with a pale face. A couple of hours after this, while getting a teaspoonful of wine and water put to her lips, she said: "I'm for no more;" and never spoke again, nor swallowed anything, but continued to lie as if asleep, breathing gently like a child; with a good colour and pupils natural, her pulse continuing good till the 24th December, when it began to get rapid. On the afternoon of the 25th she seemed to rouse a little for a moment or two, and recognised her mother and sister. Again, in the evening, she recognised her clergyman's voice, but did not seem able to see him. After that she slumbered on till 7 A.M. on December 26, when she quietly departed. During all this time the fits were very frequent, but amounted to nothing more than mere tremblings.

The body was examined on December 28 by Dr Grainger Stewart, pathologist to the Royal Infirmary, from whom I obtained the following report:—

"The body was moderately well nourished; only the head was examined. The skull was natural. At the upper and posterior part of the right hemisphere, at half an inch from the falx, the dura mater and other membranes were firmly adherent to the substance of the cerebrum, and on attempting to separate the dura mater and arachnoid, a little yellowish brown liquid escaped from a laceration in the brain substance. The fluid was found to have escaped from a cyst about the size of a pigeon's egg, which contained some yellowish red serum and imperfectly decolourized coagulum, and partially surrounded a cancerous tumour. This tumour was of the size of a walnut, dense and fibrous in its centre, loose and soft at its surface. Anteriorly and externally it was bounded by the apoplectic clot or cyst. Posteriorly, and towards the median line, as well as on its deep aspect, it passed into the brain substance. Its texture was partly soft and partly indurated, and on the softened parts there were some points of extravasated blood. No part of the basal ganglia was involved. The cavities were not distended, and there was no excess of serum on the surface. The brain substance was for the most part natural, very slightly indurated at the posterior part of the left hemisphere. There were cysts and calcareous concretions in the choroid plexus of both hemispheres. On microscopic examination, the central portion of the tumour was found to be composed mainly of connective tissue, with a number of cells scattered throughout the stroma. Towards the borders the cellular element prevailed, and in connexion with the fibres there were numerous spindle-shaped cells which contained one or more nucleated bodies. There was no brain substance in the tumour, but throughout its whole extent there were many cells presenting the ordinary characteristics of cancer cells. At the margins of the tumour there were some crystals of hæmatoidin, and a considerable number of soft laminated (amyloid) bodies, numerous granular corpuscles, and abundance of broken-down nerve substance. In no part of the tumour was there any predominance of the vascular element. In the choroid plexus there were numerous laminated bodies, many of them arranged in groups. (Signed) T. G. S."

CASE 2.—The next case I shall give in the words of one who devoted her life to the unwearied though painful task of endeavouring to alleviate the sufferings of a brilliant intellect undergoing a slow and gradual extinction, and painfully conscious—at times at least—of his anomalous position.

"The first time I saw my nephew was on his arrival from India, in December 1854, when he was 3 years and 4 months old. He looked exceedingly delicate, and had lost the power of his right side in an illness he had on board; the history of which, as far as we know it, is this:—

"His mother being confined to her cabin by bad health, the care of the children was confided to European nurses. One day when the ship was becalmed about the equator, the nurse took the boy, then in perfect health, on deck, and in a short time brought him back in a state of unconsciousness, his eyes being turned much to the right side, and quite fixed, and some time after twitching began, and continued for some days; this took place on a Monday, and on Friday he had a second similar attack.

"The medical man on board could not discover what had caused the illness, but prescribed mustard on the back of the neck and leeches on the feet. When at last the boy awoke to consciousness, he was asked if he had any pain, and he put up his hand to the left side of his head. It was then discovered that there was a peculiar hollow on the skull at that spot. By degrees the patient recovered, but his right side remained paralyzed; and on his arrival in England he could neither walk nor open his right hand. Besides this symptom, there was an anxious expression of countenance, great irritability of temper, and a flow of saliva from the mouth, and his pulse intermitted greatly, especially in sleep. He rallied immediately on coming to Scotland, and in less than a week his fingers opened. In a few weeks more he was able to walk, and much of the irritability had passed off. The flow of saliva and the intermitting pulse continued all the winter, and even longer. He got gradually stronger, however, and hopes were entertained of his complete recovery.

"Next spring, 1855, he was taken for change of air to the Bridge of Allan, and it was there the first faint trace of his ailment was observed. This was merely a little paleness, which looked like the effect of slight nausea, and as he complained of a "bad taste" in his mouth, it was thought to proceed from acidity or a disordered stomach. These "bad tastes," as he always called them, would come on several times a day for perhaps three or four days successively, and then he would be days quite well. They only lasted a few seconds, and he thought no more about them. This state of matters continued all that summer. By the advice of the medical men the greatest care was taken of his general health, and galvanism was used for the weak arm with good effect.

"Next winter (1856–57), he had hooping-cough, and got over it easily, having had little of these "sicknesses" during the time. In spring he had remittent fever, which came on with very frequent attacks of the sickness; and though the fever was soon subdued, he was long in getting up his strength, and the "bad tastes" were ever after more frequent and severe.

"It was in the autumn of that year (1857), that I observed a slight contraction of the upper lip and a lifting of the right hand while the left hand was pressed closely on the mouth. There was a little confusion of thought also, and a slight difficulty in articulation after the attack was over. He generally slept for an hour or so, and awoke quite well; still, with all these interruptions, it appeared as if his health was improving. He could walk miles without fatigue. The saliva had disappeared and his sleep was more tranquil. His mind developed rapidly and displayed uncommon talent. He was never pressed with lessons, but he learned everything as if by intuition. He taught himself the multiplication table, spelling, and the derivations of words, indeed everything his elder brother was learning, and likewise committed long pieces of poetry to memory, no one knew how. He came to write also with the weak hand; at this time, too, his dispositions were not less admirable than his abilities. He was high-spirited, manly, generous, and affectionate.

"It was in May 1859 that he had the first regular fit, and it was the longest he ever had, lasting an hour and a half. It was followed that day and the next by a great many of the slighter kind, and the effects of it continued all that summer. He was taken to Moffat, and rode about a great deal on a donkey, looking stronger and better by degrees, but the complaint was evidently progressing. All the usual symptoms of epilepsy became more marked. Several medical men saw him, but no one could do him any good.

"In 1861, we went to London to consult Dr Brown-Séquard. He at once said he thought there had been a fracture of the skull, and, besides prescribing belladonna, he ordered a blister to be put on the side of the head every fortnight or three weeks as he could stand it. At this time the weakness of the side was scarcely perceptible, and his mind was in full vigour, and Dr Séquard was confident of his case. There did seem to be a considerable improvement at first, and at the end of five months the fits were reduced by one-half, but in February 1862, while we were still continuing Dr Séquard's system, the complaint returned more violently than ever.

"In June he was again taken to be under Dr Séquard's system, but this time he received no benefit: on the contrary, it was just then a more rapid downward progress commenced. His mental faculties began to be impaired, the flow of saliva, which had stopped for years, returned, and towards the end of this year his head began to droop. It was at this time, too, that the symptoms which caused the most suffering appeared. He had always had occasional twitchings in his right hand, but now it began to twist, and this would continue for many hours, preventing sleep or rest. Nothing but ergot was found to do any good, but this generally mitigated the contraction after two or three doses of ten drops, at intervals of three hours. These painful symptoms returned every two or three months, and at times all his members were so contracted and twisted that it was difficult to keep him in bed. At these times his face had an expression of great suffering, and when long continued they left him greatly exhausted.

"In 1863, the patient had inflammation in the side, a sharp perityphlitic attack, from which he recovered perfectly; and during the fortnight it lasted he was quite intelligent, and had no fits. It seemed, indeed, as if any severe pain always stopped them, for when subsequently he had first a seton on his neck, and then an issue on his head, he had no *severe attacks* while the pain lasted.

"In 1865, a swelling was observed over the spot where the hollow in the skull was, but it seemed to produce no effect whatever on the fits. A month later, however, he had a more than usually severe series of attacks, accompanied by twisting and great rigidity of the right side. It was then the operation of trephining was performed. He was apparently unconscious for the first ten days, but able to swallow. The rigidity of the side was succeeded by total paralysis, and the other leg was constantly but not very severely contracted. The fits were very slight during the first few weeks, but they gradually returned, and in three months he was much as before.

"In November 1866, there was a little improvement in every respect, which continued for five months, and the fits came generally in the night; but in spring the progress of the disease began again, and the attacks became as before, only increasingly severe.

"The first fortnight of October 1867, he was better than usual. He spoke more distinctly, and could count the length of fifty. He also walked well, and was placid and happy. On Saturday the 12th, he took a violent hiccough, which often preceded a bad time. During the night he had thirteen fits. Next morning there was a rattling sound in his breathing, which alarmed us, but it soon passed off. All Sunday and Monday he was free of attacks, but was restless and troubled, with contraction of the limbs. On Monday evening he sat up on the sofa, and was cheerful and collected, and it seemed as if the bad times were over. At 10 o'clock the fits returned at intervals of about half-an-hour or twenty minutes, and continued till 9 in the morning; his breathing at the time much affected. From 9 till 1 the attacks ceased, and he lay quiet, but exhausted. At 1, the fits returned with the same frequency, and continued till within an hour of his death, which took place at 5 o'clock, without a sigh, or a struggle, or the movement of a muscle. This was Tuesday the 15th October; on Monday he had taken all his meals as usual.

"I do not think I can add anything to what I have written, except that his constitution was of the first order, all his organs acting with perfect regularity, notwithstanding the strain they were subjected to for so many years.

"It is long since we gave up keeping a record of the attacks, but when we did so the number averaged from 20 to 25 in the month. Latterly there might be a few more, but all along they increased much more in intensity than in number. For the first seven years the fits were far more frequent during the day than during the night. For three or four years they were about equally divided between the day and night, and for the last two years they were far more *frequent during the night.*

"In regard to the origin of the malady, I may state that his father was always inclined to attribute it to sunstroke, rather than to a blow.

"*P.S.*—I have omitted to mention in the right place, that for the last five years the attacks were almost always accompanied by a sudden emptying of the bladder, an effect which never followed in the earlier stages of the disease; and also that, the last two years, the saliva became increasingly thick, heavy, and viscid, resembling the mucus which usually comes from the nose."

This interesting narrative requires little addition from me. There are, however, one or two little omissions which require to be supplied to complete the case, which have been partly derived from the same authority as the foregoing narrative, and partly from my own notes of the case. The *family history* of this poor boy, unexceptionable as regards the mother's side, betrayed on the father's side a strong proclivity to cerebral disease. His father himself died in India from apoplexy after a few hours' illness, when scarce past middle age. His paternal grandfather died of cerebral disease, after being paralytic for two or three years. His paternal grandmother also died of brain disease, after having lost her speech by an apoplectic stroke some time previously. Both of these were, however, past middle life, as were likewise two aunts, who died, one suddenly from supposed brain disease, and the other from gradual paralysis. An uncle also died suddenly in the pulpit while delivering his first sermon, from supposed cerebral disease. So far as can be ascertained, however, no member of the family ever had epilepsy.

From the very first symptoms of his malady, its possible connexion with the peculiar depression, which felt like a crack running across the left parietal bone, was never lost sight of; but as the symptoms were apparently decidedly gastric, it was thought proper at first, at all events, to treat them as such. Indeed, it was not till after the lapse of many long years, and not until the symptoms peremptorily demanded energetic interference, that I could obtain medical sanction for that operative interference which was then had recourse to. And even now, when all the circumstances of the case are fully known, I do not know whether to regret that trephining was not had recourse to early in the complaint, as I always wished, or to rejoice that it was reserved as a *dernier ressort*,—a something to fall back upon to relieve the symptoms temporarily when everything else had failed, and thus delay the lapse into brutish imbecility so surely coming, and keep open for a little longer the possibility of improvement which might take place from changes in the cranium and constitution at the period of puberty. No doubt, trephining was a trump card, which it was safe to keep, but I am inclined to think, that by leading it a little earlier, we might have exposed our adversaries' weakness and gained the game. No one can tell the possible benefits which may arise from trephining early, even in such a case as this, while the evils of delay—in injury to the brain, and especially the medulla oblongata—are only too patent and well known. As it was, however, the treatment at first consisted in improving his health by regulating his diet—in regard to which various systems were successively adopted without advantage—by regulating his bowels, and by improving his secretions by occasional small doses of gray powder and rhubarb, and, subsequently, by giving full doses of oxide of zinc,—a remedy whose sedative action is occasionally so beneficial, and specially so in epilepsy arising from intestinal irritation. The additional advice of various medical men in Edinburgh was also obtained, but without any important alterations being suggested. Perhaps I may also mention here that I saw this patient in his first well-marked fit in May 1859, and that it was both severe and well-marked, and though there was not so much asphyxia and cerebral congestion, the convulsions were more violent than in the former case just related. I may also mention that, for the first three or four years, he suffered much from pains in his limbs during the night, and that he always complained of cold in his back whenever a bad time was coming on; latterly both of these symptoms ceased.

At last, after a steady increase of the disease, without one check for six years, the little patient was, in May 1861, taken to London for the benefit of the advice of Dr Brown-Séquard, than whom there is probably no higher living authority upon nervous diseases. Brown-Séquard's attention was at once attracted to the peculiar sensation apparent on passing the hand over the left side of the head. He fancied the bone must have been fractured, and he ordered a blister to be applied over this part of the head every fortnight or three weeks. He also fancied that he had detected an aura extending from the right hand upwards, and to check this a blister, one inch wide, was applied round the arm above the elbow; this was done twice, but without effect. Subsequently he recommended the use of ligatures applied suddenly on the first symptom of an approaching fit; these were at first applied only to the arms, and when this did not succeed so constantly as he expected, he ordered them to be applied to the legs too, and this was done with a partial success, checking the slighter fits, but not the severe ones. For a long time the strings were kept round the limbs, looped and so arranged as to pull quickly,—for, unless applied at once, they were of no use; but as the patient disliked this, and as he was always in the family, and they did not like it either, latterly the strings were retained in readiness in the pockets of his friends, and applied at once on the first symptom of an attack becoming apparent. For the last two or three years, however, the fits were as a rule too violent to yield to this measure, and as they also occurred oftener in the night, the strings were less used, but never entirely given up. Brown-Séquard also remarked the peculiar depressed condition of the patient's vital powers, which, to a certain extent, was due to his parentage, partly to his birth and early residence in a tropical country, and partly also to the influence of the disease, which, in his case, as in the former one, produced, and did not arise out of, this debilitated condition. To improve this languid state of the circulation, the patient was recommended a cold douche on the spine, but as this could not be had in his lodgings in London, the spine was ordered to be well slapped with a wet towel. On his return to Scotland, this was replaced by a shower-bath, and along with this the citrate of iron and sulphate of quinine were combined in various doses with Brown-Séquard's great stand by the extract of belladonna. To give an idea of the manner in which these remedies were employed, the best plan is perhaps to copy the prescriptions now lying before me.

℞ Extr. belladonnæ, gr. ⅙, ⅕, ¼ (successive alterations.)
Ferrit, citrat, gr. ¾.
Quinæ disulph., gr. ½.
Ft. pilula—mitte xxx.
May 22, 1861.—Two pills morning—three evening.

This was exchanged on the 21st of June for the following:—

℞ Extr. belladonnæ, gr. ½.
Ferri citrat, gr. 1.
Quin. disulph., gr. ¾.
Ft. pilula—mitte xxx.
Two at bed-time, one in the morning, and one at tea-time.

This treatment was so fully pushed, that for three entire weeks the patient took no fewer than nine pills in the day—four grains and a half of the extract of belladonna. He subsequently returned home, still taking the same pills in sufficient doses to keep his pupils constantly dilated, and taking along with them two pills every night containing each two grains of quinine, and three grains of lactucarium (Extr. Lactucæ Virosæ). Dr Brown-Séquard was very hopeful of success in this case, and indeed the improvement was at first most remarkable, the number of the fits having been reduced one-half at the end of five months; but, alas! while still continuing the same treatment, they returned with greater violence than formerly. I naturally suggested having again recourse to Dr Brown-Séquard's kind offices, and accordingly, in June 1862, he returned to London to be under his care. By this time, Dr Séquard himself seems to have become less sanguine, and his treatment more tentative. Founding upon the old idea of the curative influence of ague in epilepsy, and coupling that with Hahnemann's theory of the possibility of producing an ague by large and repeated doses of quinine, Dr Séquard prescribed the following remedies, with the avowed intention of producing, artificially, a curative ague.

℞ Quinæ disulphatis, ʒ i.
Acidi. sulph. dil., f. ʒ i ss.
Aquæ distillatæ, f. ℥ iii.
Tinct. hyoscyami, f. ʒ i.
Six teaspoonfuls every other night.
℞ Tinct. hyoscyami, f. ʒ ii ss.
Tinct. opii., ♏ L.
One fourth part every other night.

This treatment was, however, unavailing; it neither produced an ague, nor did it cure the epilepsy; on the contrary, just at that time a more rapid downward progress commenced, and the patient returned home worse than when he left. The severe tonic spasms, which about this time commenced to annoy him, and which gave him far greater suffering than any other symptoms of his complaint, were always checked by ten-drop doses of the *liquor ergotæ*, repeated at regular intervals; but this remedy had not the slightest influence upon the fits. About this time a seton was introduced into the upper part of the neck and kept discharging for many weeks, with some slight temporary alleviation of the disorder at first—an alleviation always observed to follow almost any change of treatment, and specially any operative interference, but the disease generally revenged itself for any temporary check by recurring with increased violence. After the removal of the seton, Chapman's spinal ice-bags were constantly worn for ten weeks, at first with apparently a good effect, but latterly without any result whatever, the paroxysms recurring just as often and as severely when the bags were on as when they were off. At last, in November 1863, it was resolved to adopt a modification of St Dunstan's mode of treatment, which consisted in making the sign of the cross, with a hot iron, on the patient's head, and accordingly I made a longitudinal incision in the course of the apparent injury to the left parietal bone, right down to the periosteum, and as far as possible through it. This was done while the patient was under chloroform, which I may mention readily produced its usual effects, and did not bring on a fit, when given in the recumbent position, though, when subsequently administered in the semi-erect posture for the extraction of a tooth, it had to be stopped from the advent of a paroxysm. I mention this, because the administration of chloroform has been asserted to afford the possibility of a diagnosis between real and simulated epilepsy, always bringing on a paroxysm when the disease is real; this, however, we see, is not the case. Further, I may mention, that in this case any operative interference whatever was always followed by an attack of erysipelas, readily checked by the saturation of the system with the tincture of the muriate of iron; while this affection never occurred when the system was placed under the influence of iron previous to undertaking any such operation. The scalp was found to be very vascular, and secondary hæmorrhage to a considerable extent took place from the wound on the setting in of inflammation, but it was readily checked by pressure. The wound was kept open by an issue, and allowed to heal as slowly as possible; its immediate effect was beneficial, and the rapidity of the downward progress was checked for the time. Presently, however, the disease re-asserted its supremacy, and continued to gain in strength and severity till, in the end of 1865, the patient was in a most hopeless condition; and upon consultation with Sir James Simpson, Dr Begbie, senior, and Dr P. H. Watson, it was determined to afford him the chance of relief which trephining over the seat of the apparent fracture might perchance bring him; and the exact spot selected for the operation was chosen in the line of apparent fracture, and as near as possible opposite that part of the brain found affected in the case just related, which had presented many symptoms similar to those present in this patient. On the 3d of January 1866, Dr Watson trephined the patient in the presence of Sir James Simpson, Dr Begbie, senior, and myself. The operation was performed under chloroform, and succeeded perfectly. Strange to say, upon cutting down upon the apparent crack which was so evidently felt through the scalp, nothing was found to account for the sensation, the surface of the bone being quite smooth; the bone was, however, as is not unusual in such cases, thicker than usual, especially at its upper edge; but no spiculæ were on its internal surface, and nothing unusual was observed on the dura mater. The immediate result of the operation was a complete cessation of the tonic spasms of the right side, which had previously been so severe as to be with difficulty controlled by the ergot, and, but for the relief afforded by it, would have forced the patient out of bed. Next morning he had two slight fits, and his pulse was 120. On the day following he had one bad fit, but his pulse was only 96, and there was a slight oozing of pus from the wound. He still, however, lay listless and apathetic, only taking food when put into his mouth, as was done at regular intervals; but by the 9th of January, six days after the operation, he began to wake up and to be more intelligent, and this improvement continued to progress, notwithstanding the repeated recurrence of the fits, which came on in a series of one or two every second or third day, but which were usually slight, while any retention of the discharge, which was ordinarily copious, was always accompanied by increased drowsiness, and sometimes by irritation and clonic spasms of the right side, particularly of the arm. The wound healed favourably, and the patient, instead of being a bed-ridden imbecile, began to recover his intelligence and power of progression, so that ere long he was able to walk about the garden occasionally as formerly; and though the great and immediate improvement was not permanent, yet he never again fell so low or was so completely clouded. About the end of the year (1866) he was put upon full doses of bromide of potassium. At first twenty grains were given twice a day, and in about a month or six weeks the dose was increased to forty grains thrice a day; but this quantity disagreed with the patient—probably from not being able to get him to dilute it sufficiently—producing uneasiness of the stomach and pyrosis. He continued, however, to take ninety grains a day for at least three months. This medicine seemed to give him much relief, and it was while taking it that the remarkable rally, mentioned as beginning in Nov. 1866, took place, —only the improvement had commenced a fortnight before the powders were given, and the fits returned in force long before they were discontinued. He took the bromide of potassium daily for about seven months in all; and besides the great relief, which we cannot but think was at least partly due to it, it had also the special advantage of keeping the bowels regular without the use of any other medicine. After this he continued much in his usual state till the middle of October 1867, when he was so well that his friends were actually tempted to canvass the possibility of his ultimate recovery; but, alas! on the night of Saturday the 12th, he had a severe series of fits; on Sunday and Monday he was free of fits, though restless and troubled with tonic spasms, for which he had to get ergot. On Monday he took his food well, and at night sat up and was cheerful, as if the attacks were over for the time; at 10 P.M., however, they returned with great violence, and continued every half hour or twenty minutes till 9 A.M. on Tuesday the 15th, when they ceased, leaving him much exhausted, and with a considerable amount of effusion into the bronchi. In the course of the forenoon I saw the patient, but he was then looking better, and his breathing easy and free; pulse somewhat feeble. He was ordered to have an occasional dose of spt. ammon. arom., in claret, to keep him up, as he was quite unable to take other nourishment; at the same time his friends were warned that any speedy recurrence of the fits, even in a less severe form, would probably prove fatal to him. The fits did recur at 1 P.M. of the same day, and continued with same frequency and severity till 4 P.M., and he died at 5 P.M., from asphyxia produced by bronchial effusion (pulmonary œdema), which, in his exhausted condition, he could neither cough up nor re-absorb. On the afternoon of the 17th, assisted by Mr Strethill Wright, assistant in the University Anatomical Rooms, I made a post-mortem examination of the head. The scalp was vascular, and somewhat difficult to separate at the part where the operation of trephining had been performed. The skull was irregularly thickened, and rather posterior to the centre of the left parietal bone there lay the opening made nearly two years previously by the trephine. The bone close to the upper and anterior edge of this opening was somewhat nodulated exteriorly—internally it was quite smooth and natural, and no trace of a fracture could be perceived either externally or internally. The edges of the trephine aperture were thinned off, and, as it were, fringed with a new growth of bone; the aperture itself was filled up by a piece of tough cartilage, which was adherent to the dura mater. There was a considerable amount of sub-arachnoid effusion, and there was a good deal of congestion of the meninges and surface of the brain,—the brain itself being somewhat softened, as was to be expected from the lapse of time since the death, but free from bloody points. Upon the superior surface of the left hemisphere, a small round aperture, about the size of the point of the little finger, was observed in the centre of one of the cerebral convolutions, about an inch anterior to the parieto-occipital fissure. This opening led to a cavity in the brain substance, about the size of a walnut, and which was formed partly by the destruction of a small part of the brain substance, and partly by the compression and opening up of the neighbouring convolutions; it contained a clear yellowish serum, and was lined by a somewhat tough membrane, which, on being scraped, yielded numerous crystals of hæmatoidine. In the course of our short and somewhat limited inspection, nothing else abnormal was discovered.

Thus the whole of this interesting case resolved itself into a fit of cerebral apoplexy, which had occurred thirteen years ago—transformation of the clot into a cyst, and permanence of that cyst, by no means an unfrequent occurrence in peripheral apoplexies. Whether the apoplexy had been produced by a sunstroke, or by a blow with or without a consequent fracture of the skull, all traces of which were removed prior to death, cannot of course now be determined. To me it seems more probable that a blow had been inflicted, the results of which, on so young a skull, thirteen years were sufficient to obliterate, than to suppose that an almost momentary exposure, even under the tropics, could in so young a child—making of course no strenuous bodily exertion, but carried in his nurse's arms—produce so serious a lesion. It was, I may add, extremely interesting to observe that our ideas as to the probable place of origin of the disease were fully confirmed by the inspection; for the piece of cartilage, which with its upper surface filled the trephine hole, closed with its under one the superficial opening of the apoplectic cyst. I will not speculate as to what might have been the result had this cyst been exposed at an earlier date; I am content to think that everything was done under the best advice, and with the best intentions; and I will also confess that we never looked for an apoplectic cyst because it was not at all what we expected to find,—for the most probable cause of the disease in this case was supposed to be bony spiculæ projecting inwards, or a bony nodule, resembling what really existed outside, and which in another but less fortunate case which I had subsequently the privilege of seeing operated upon, proved to be the actual exciting cause. The removal of such a cause would of course be followed — or at least it has been in many cases followed—by the immediate or gradual cessation of the fits, and the restoration of the patient not only to life, but to a life of active utility. One of the earliest and most remarkable cases of this kind is related by Dr Rogers, of New York; in it a protracted epilepsy was cured by the elevation of a portion of the os frontis, which had been depressed for fourteen years. Of course I do not advocate trephining as a cure for all cases of epilepsy, but chiefly for those whose history points to some cranial injury, accompanied by some apparent trace of such injury, such as a depression or irregularity of the bone. In these cases the operation ought, of course, to be performed at the spot affected; and, in my opinion, it ought in them to be one of the earliest modes of relief attempted,—because epilepsy is no more a disease than apoplexy or jaundice, it is merely a symptom; and to treat palliatively a mere symptom with an evidently removable cause staring us in the face, is to show ourselves to be unworthy of the name of physicians. But I may be told the risks of the operation are so great. So they are; but the certainties of the disease are more terrible still, both for the patient and his friends; and I hold that the risks of the operation are more of a bugbear than a reality. Modern surgery, which now saves 80 per cent. in an operation formerly almost so universally fatal as ovariotomy, can surely, and my own experience confirms that it can, perform with safety once, or perhaps twice, on one poor head, an operation which the rude and rough surgery of former days actually performed twenty-seven times on one head, the wearer of which survived to tell the tale and express his gratitude to his surgeon.[1] Further, in cases in which the epilepsy is accompanied by paralysis of one side, and especially of one arm, but without any history of cerebral injury, under certain circumstances and after the failure of other treatment, it might be worth while to perform this operation on some part of the parietal bone, on the side of the skull opposite to the paralyzed limb,—not because paralysis of the arm is usually accompanied by affections of that part of the brain, but because paralysis of the one side, accompanied by epileptic convulsions, has been found associated with disease of that part of the brain in the two cases just related, as well as in a third, the only similar case of the kind with a dissection which I have fallen upon. It was communicated by Dr Beilby to Dr Abercrombie, and is worth relating:—"A boy, aged 16 months, formerly healthy, was seized with an attack of convulsions. It affected chiefly his right side, which was very strongly convulsed. He had a second attack on the evening of the same day, after which he had fever, blindness, and loss of the power of deglutition. In this state he continued ten days, when he had another fit, and after this he gradually recovered his sight and deglutition. From this time there was a degree of paralysis of the right side, but in other respects he enjoyed good health for four years. He then had epileptic paroxysms, which at first occurred once in two months, but gradually increased in frequency. After a much longer interval than usual, a few weeks before his death, the fits returned after a fright, recurred with great frequency, and were fatal. He was then twelve years of age; his right side had continued weak; and, at the time of his death, the right lower extremity was three inches shorter than the other; his intellect had been weak, so that he never could be taught to read. On the *surface of the brain*, under the *left parietal bone*, there was a tumour the size of an egg, situated between the pia mater and the arachnoid; internally it was of a white and somewhat gelatinous appearance, but very firm, and when cut into, some serous fluid was discharged from it; no effusion."[1] The resemblance of the symptoms and morbid appearances of this case, and the two I have just related, is too marked to be merely casual; therefore I think that in similar cases, even without any history of cranial injury, trephining over the parietal bone might be resorted to, in the hope that the irritant cause might perchance be removable. There can be no doubt that whether this were the case or not, the operation would be at least temporarily beneficial. And this leads me to remark that, in every case in which epilepsy occurs before puberty, trephining must be kept in view as a *dernier ressort*, after the failure of everything else, to check at least temporarily the downward progress, so as to leave the case still open to the possible improvement which may perchance take place as the result of those changes in the constitution and cranium which take place at puberty. Of course, in older subjects, this reason for trephining is wanting; and in them the operation is not advisable, unless undertaken to remove some definite cause, as the temporary improvement, which is all that can be promised in other cases, is scarcely worth the risk run. While thus arguing for the propriety of trephining as a justifiable operation in certain exceptional cases of epilepsy, I am far from regarding it as a remedy applicable to all. I hold that, at present, we do not possess any sufficiently accurate knowledge of the causation of epilepsy to authorize us to lay down any definite system of treatment for this affection. The only constant element in epilepsy is an exalted excitability of the medulla oblongata as the centre of reflection. In some cases we can remove that eccentric cause which, by persistent action, has produced this undue excitability, by operative measures in exceptional cases, in others by sedatives to the intestinal mucous membranes, such as nitrate of silver or oxide of zinc; and in still others, by improving the secretions, and restoring the healthy tone to organs in which it may be defective,—allowing that undue excitability of the centre of reflection gradually to die out under the restored influence of normal nutritional changes. While, in other cases, it is of importance to combine this tonic treatment with remedies which act as direct sedatives to that undue excitability of the special nervous centre. The most important of these, according to present experience, being belladonna and bromide of potassium, both of which require to be given in pretty full doses for a length of time, and the latter in not less than half-drachm doses twice a day; its effects in these doses, in a certain class of cases, being sometimes truly surprising. Opium and other sedatives have a similar effect, and are only to be avoided because they interfere with the normal processes of secretion—and thus do as much harm in one way as they do good in another. From the decided action of ergot on the tonic spasms, and its total want of influence on the fits, while we positively know that it does produce contractions of the unstriped muscular fibres, it would seem that these spasms are probably the result of arterial excitement, which the drug modifies; while it would also appear as a corollary from these facts that Brown-Séquard's idea that belladonna acted by producing arterial contraction, is probably erroneous, and that it acts solely as a sedative. Esquirol states that every new remedy suspends the attacks for a time—fifteen days to three months are the terms he gives; and Chomel makes a similar statement,—a result which must be familiar to all who have had anything to do with the treatment of epilepsy. To me it seems highly probable that this effect is the result of inhibition; and that just as a galvanic current producing spasmodic action of the limbs of a rabbit stops the movements of the intestines, so do these remedies, by temporarily exciting other parts of the nervous system, inhibit the action of the medulla oblongata, which usually revenges itself for its enforced quiescence by a subsequent more than usually severe outburst. The action of sternutatories, which were employed in Case 2, with temporary benefit, and the action of ligatures on the extremities, seem to me to belong to the same category,—some of these remedies acting apparently directly upon the nervous system, and others producing their inhibitions through the mental faculties, whose action is just as often inhibiting as not. Painfully deceptive, therefore, as the treatment of epilepsy often is, it apparently deceives us only in accordance with certain laws, the full comprehension of which will hereafter open up the way to more perfect success where that is possible, and will also teach us both when and why we should not look for that success, as well as how such hopeless cases may best be palliated. In the unravelling of the physiology of the nervous system lies our only hope of understanding its pathology, and our surest foundation for a successful treatment of its maladies.

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[1] Which he did by giving him the following certificate: — "Ego infra scriptus, attestor me, ab Henrico Chadborn, Chirurgo Neomagnesi, postquam vigesies septies mihi caput perforasset recte fuisse sanatum. Datum Weichemi 12mo Augustii anni 1664." The patient was Philip Count of Nassau. *Vide* Bell on the Nature and Cure of Wounds. Edin. 1795, p. 135. By referring to J. Bell's Surgery, Edin., 1801, vol. ii., pp. 750-52, etc., it will be seen that the singular operation just referred to was neither unusual nor exaggerated, one Lassus having gone so far as to calculate how often the skull could be trepanned,—this he found to be *forty times!*

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[1] Abercrombie on the Brain, etc. 3d ed. Edin. 1836, p. 444.

ARTICLE II.—*Gunshot Wound; Amputation at the Hip-Joint.* By J. FAYRER., M.D., F.R.S.E., Surgeon, Bengal Army; Professor of Surgery, Medical College of Bengal.

ON the 25th September 1867, a consultation was held at the Officers' Hospital in Calcutta, on the case of Lieutenant H., who had been accidentally wounded on the 12th of the same month by a rifle shot, which had penetrated the gluteal region, and fractured the head and neck of the femur.

The patient had been a passenger on board a troopship, and met with the accident when out shooting at night with a friend, in the Island of Ceylon. The circumstances were briefly as follows:—

In pursuit of large game in the jungle by moonlight, they had taken different paths, and believed they were widely separated. About two or three in the morning, the attention of one of the gentlemen was called by his guide to a herd of wild animals 40 or 50 yards distant. He raised his rifle and fired, and to his horror found, on approaching the object, that he had wounded his companion, whom he believed to be distant several miles. The injured man, when struck, was dressed in a gray shooting suit, and was crawling on all-fours, probably to approach the game without being seen. The bullet from a Snider rifle struck him just over the left tuber ischii, and lodged. He lost a considerable quantity of blood whilst being carried to the nearest habitation, some miles distant, and there, when the medical officers arrived, the nature of the wound was examined. It was found that the ball had grooved the tuber ischii, passed on towards the neck of the femur, and there lodged. There was no evidence of its having entered the pelvis, and the fracture of the neck of the femur was not then detected, doubtless owing to the fact that the capsular ligament had not been much injured by the passage of the bullet. He was taken on board the ship, as, owing to the heat and damp of the climate, and for other reasons, it was deemed better that he should go on to Calcutta; and after a stormy voyage, he arrived at Fort William on the 24th September. He had been tended with the greatest care and skill, but his sufferings had been great, the discharge very profuse, and had acquired the use of frequent doses of opium. There had been considerable irritative fever, occasional delirium, and consequent wasting and debility, on the passage. Some small fragments of bone, and the basal cone of wood of the Snider bullet had been removed from the wound. I saw him first at 10.30 A.M., in consultation with several medical officers, and, on placing him under the influence of chloroform, an exploration and thorough examination of the wound were made. His pulse was then 128; he was slightly delirious; and the discharge from the wound was fetid and profuse. The constitutional disturbance was well marked: the rapid pulse, profuse perspiration, and quickened respiration were suggestive of the commencement of pyæmic mischief, though no rigors had, as yet, occurred. The respiration is said by the medical officers who had seen him during the previous few days, to have been at times quick and catching. He had also a clay-coloured diarrhœa. I observed, on exposing the limb, that the foot was everted and the leg shortened about two inches, with a marked round prominence of the left hip about the trochanter; the skin was smooth and glazy, with the superfinal veins well marked. On grasping the head of the bone, and making extension and rotation by the foot, crepitation was apparent; and on introducing the finger into the wound, now a large suppurating sinus, fragments of bone were detected, and the fractured neck of the femur with part of its head, detached and left in the acetabulum, were apparent. The ischium was found to have been grooved by the ball.

It is to be observed here, that the state of matters had entirely changed since the wound was inflicted; the capsular ligament had given way in the suppuration, and sloughing had occurred in the track of the ball, thus allowing the shortening of the limb to take place,—for there was none, it is reported, when the first examination was made,—and the portions of dead and comminuted bone to move more freely on each other, thus making the crepitus apparent. The wound also had become much enlarged by sloughing of the track of the ball, and allowed the finger to enter and examine freely.

The nature of the injury having been so far determined, consultation was held as to the treatment, and it was decided that the only hope of saving life lay in an operation, which,—considering the extent of injury, not only to the neck of the femur but to the ischium, the cancellated texture of which was opened, and to the soft parts, from sloughing and suppuration about the head and neck of the bone, and in the track of the ball,—should be amputation at the hip-joint. As no pelvic injury seemed to have been inflicted, his urine was natural. As no pyæmic visceral complication had yet occurred, and considering his youth and previous healthy constitution—he was only 20 years old—it was thought that this operation offered the only chance of life, though it was admitted that it was at the best a poor one. His consent and that of his friends having been obtained, it was determined that the operation should be performed as soon as possible, and 3 P.M. was the time appointed.

Accordingly at 3.30 P.M. he was brought under the influence of chloroform by Dr Dale, Garrison Surgeon, and the operation performed.

Entering the point of a long amputating knife about two inches from the anterior superior spine of the ilium, in a line leading to the great trochanter, I transfixed the anterior part of the thigh, the point grazing the bone as it passed, and emerging about three inches from the anus. The artery was commanded by Professor Partridge, who, following the knife as the anterior flap was cut, seized and compressed the main trunk as soon as it was divided, the limb meanwhile being depressed by Dr Hume, C.B., V.C., no difficulty was found in passing the knife between the broken fragments of the neck of the bone in cutting out the posterior flap and detaching the limb; the proceeding occupying altogether between 20 and 25 seconds. There was considerable oozing from small arteries and veins. The large vessels, femoral and gluteal, were secured, and then the portion of the head of the femur, still attached by the round ligament, was removed from the acetabulum. All bleeding points having been secured, the flaps were brought together with silver wire sutures. He became very low towards the latter part of the operation, the bleeding points being so numerous that many ligatures were required; but under the influence of stimulants he rallied, and was put to bed.

His pulse rose, and reaction commenced; but although he was closely watched, hot bottles and sinapisms applied to the limbs and over the heart and stomach, champagne and ammoniated stimulants given, he never thoroughly rallied. About an hour and a half after being put to bed, his breathing became much accelerated, and he sank, and died quietly at 6.40 P.M., about three hours after the operation.

The quantity of blood lost during the operation was small, and he did not lose a drop after its completion. His constitutional power were exhausted, and he never completely rallied from the shock. Death, I believe, was accelerated by the rapid formation of fibrinous coagula in the right side of the heart.

I should note that, after removing the limb and the fragment of the head of the bone left in the acetabutum, my finger entered a large sloughing cavity above, internal to, and behind the neck of, the bone; from this, the bullet, split into three longitudinal pieces, with some fragments of bone, were removed. Before bringing the flaps together, the remains of the sloughing capsular ligament and some shreds of sloughing muscle were also dissected away. The external portion of the margin of the acetabulum was found to be roughened and partially stripped of periosteum.

The body was examined the following morning at 11.30 A.M.; decomposition had not perceptibly commenced. The thorax was opened, and the lungs were found to be partially collapsed. There were patches of lobular hypostatic congestion, but for the most part they were pale and exsanguine. In the lower lobes there were one or two small patches of commencing pyæmic mischief, but they had not advanced to disorganization.

The pleuræ and pericardium were quite healthy.

Heart natural in size and healthy in structure; valves all competent; but the right ventricle was nearly filled with a firm adherent fibrinous ante-mortem clot, and besides it there was a post-mortem coloured coagulum. The fibrinous clots extended far into the pulmonary arteries. The right auricle also contained a small fibrinous clot. The left ventricle and its valves were quite healthy.

The left auricle contained a small fibrinous clot. The exsanguine state of the lungs corresponded to the obstructed pulmonary circulation.

Abdomen.—The liver healthy, pale, and rather fatty; spleen and kidneys quite healthy; intestines healthy. The peritoneum covering the bladder and the pelvis had a darkened appearance, probably from decomposition; no trace of any injury in the pelvis; but, corresponding to the portion of the acetabulum, denuded of periosteum, there was a small collection of pus in the subfascial areolar tissue of the pelvic cavity.

In this situation the soft parts, external to the peritoneum, were matted together as if by inflammation. I removed the left os-innominatum for more perfect examination, and found that the tuber ischii had been injured by the ball; it was grooved, and its cancellated tissue laid open. It was also broken across near the spine, and the ramus was also fractured; the fracture ran up to the top of the acetabulum, the outer portion of which was rough, and denuded of periosteum. There was also a fissure in the acetabulum itself. These fractures were all fissures, and involved no separation of the fragments.

Remarks.—This is the only instance in which I have seen the effects of the Snider bullet, and I should have expected, knowing the great force and velocity with which that projectile travels, that it would have passed right through the limb, struck at a distance of only 50 yards, notwithstanding the obstacle it met with in the neck of the femur. The force of the ball, however, seems to have been spent in traversing the gluteal region, grooving the ischium, and breaking the head and neck of the femur into three pieces. The part of the neck actually traversed by the bullet presented a comminuted groove, and some small fragments knocked off lay in the wound or in the foyer of pus, and sloughing tissue, from which the ball was removed after the amputation.

With reference to the operation, and why amputation was preferred to excision. The soft parts were so much disorganized, the pelvic bones were injured, and the patient's constitution had been so much tried already by suppuration and irritative fever, with evident indications of incipient toxæmia, and knowing the great tendency in this place to osteo-myelitis in divided bones, it was considered that, if he could survive the shock of amputation, the subsequent chances of recovery were better than after excision, which would have left the suppurating cavity, and a divided long bone, as a probable source of future pyæmic poisoning, should it, as was very probable, become the subject of osteo-myelitis. He bore the operation very fairly, and his condition when it was undertaken, although low, was not by any means such as to render success impossible. The loss of blood was not very great, nor was there any difficulty or delay in the amputation.

Reaction commenced, but as I believe the rapid supervention of the formation of fibrinous coagula in the right side of the heart, or rather aggravation of it in an already incipient condition, prevented its perfect re-establishment, and he sank, as many other surgical patients do, not only from the shock and exhaustion, but from the sudden interruption in the pulmonary circulation.

ARTICLE III.—*The Examination of the Bladder*. By J. MATTHEWS DUNCAN, M.D.

ALTHOUGH the diseases of the bladder have been the subject of much observation and study, and much has been written concerning them, I am not aware that in any quarter the physical examination of the bladder is described with the care and fulness which it appears to me to deserve. I am satisfied that attention only requires to be called to it to secure for it the consideration it merits, forming, as it does, a valuable addition to our means of diagnosis, that may be available in many difficult cases.

No special instrument is required for the purpose. A rigid female catheter is scarcely long enough, but may suffice, especially if it is well closed by its stilet. I use a metallic vesical sound that has, about one inch from its extremity, a curve which joins the two parts separated by it at an obtuse angle. The instrument must be rigid so as to be pushed with a little force without yielding. Its point should be rounded and blunt so that it may not easily injure the walls of the bladder, or cause pain in a healthy organ. It is convenient to give it a flat button-like handle, and a total length of about twelve inches.

The conditions which this instrument, used by the fingers of a skilled practitioner, should discover or investigate are the following:—

1. The tenderness of the bladder.
2. The length of the bladder.
3. The state of the walls in respect of softness or distensibility.

Each of these subjects will require some separate consideration. When the whole are correctly made out in any case, the results cannot fail to be of practical value to the practitioner who knows how to make use of them.

The operation of conducting this investigation must always be made with tenderness and care, and in a case where much pain is anticipated, only after a decision, duly arrived at, and founded on a just appreciation of the features of the case, that the investigation will probably yield results justifying the infliction of the pain and alarm usually excited.

1. *The tenderness of the Bladder.*

This tenderness must be, if necessary, distinguished from tenderness arising from pressure by the instrument on the neighbouring inflamed parts. It is seldom difficult to do so; for, first of all, such inflamed parts can be previously and otherwise made out, and their position avoided in manipulating the instrument, or their presence allowed for; but, secondly, it is found in practice that even the compression of the bladder by neighbouring inflamed masses does not prevent the careful examiner from investigating the state of the bladder without interferences from such sources of error. This last statement is often remarkably verified by the absence of vesical tenderness under such circumstances.

In cases of gonorrhœa, or in any others in which there is urethritis or urethral tenderness, a greater difficulty is introduced by this complication than by the former. For the pain of passing the instrument is great; if present, it anticipates the eliciting of vesical tenderness, and accompanies it when elicited; it causes expressions of pain even when vesical tenderness is absent, and the patient is generally too intent on her feelings to submit to questionings regarding any difference in locality of the urethral, and of the vesical pains. But this difficulty is to a great extent overcome by remarking the great increase of pain produced in touching the vesical walls in such cases as those of gonorrhœal cystitis.

Catarrh, or irritability and inflammation of the bladder are the conditions in which tenderness is always great, sometimes intense; every touch of its walls, every motion of the instrument causing a cry of agony. This is what is naturally expected; and it is, I believe, invariably found. But it is not a rare occurrence to discover total, or nearly total, absence of tenderness in cases where catarrh or inflammation was believed to be present. In such instances, nothing can be much more important than to find the absence of tenderness. The diagnosis is corrected. The practitioner points his inquiries in a new direction, finds out the hidden seat of disease, or at least has a more accurate idea of the nature of the case under his care.

Cases illustrating this statement have frequently occurred in my practice. For example, a patient was recently sent to me from the country with irritable bladder. It had every symptom of this disease, including the presence of pus in small quantity in the urine. The woman had been recently confined without any difficulty. I treated the case for a few days as one of irritable bladder, but with no success. Then I made an examination of the bladder, and found it quite without tenderness; and then I further investigated the case, and discovered superficial abrasions or ulcerations of the vestibule and of the internal surfaces of the nymphæ, which were tender. Attention to these, including washing, and the use of red lotion, soon healed them, and the patient was quickly cured. Here the absence of vesical tenderness showed the error of diagnosis, proving that the bladder though irritated was not irritable in itself. This led to the discovery of the source of irritation and of the small quantity of pus in the urine.

It is only necessary to name the difficulty, often occurring in urinary cases, of deciding between the kidney and the bladder as the source of the symptoms, to show the valuable aid that may be acquired by ascertaining the state of the bladder as to tenderness and other conditions.

The healthy and many diseased bladders can be examined by the sound fully without any complaint of suffering being made, far less any cry of pain. It is needless to say that many women, easily frightened, can scarcely believe that they are not suffering when under the examination, and disquiet themselves greatly. On the other hand, I can say that I have often examined the bladder in women without their being aware that anything had been done. I have been asked when I intended to begin, when the examination was already completed.

I have examined a bladder which had no notable sensitiveness, which was believed to be only compressed and slightly irritated by neighbouring disease, but which was really the subject of cancer, having several large and bleeding projections in its interior. These were indistinctly felt; but the absence of pain on examination misled me as to the condition of this bladder. At that time I did not know that a bladder so extensively diseased and disorganized could be examined by the sound with so little suffering.

2. *The length of the Bladder.*

By the length of the bladder, I mean the length of the sound which passes into it. I measure the length from the external orifice of the meatus urinarius; and, in giving measurements, I understand the point of the instrument applied to the top of bladder to be one extremity of the measured line, and the external orifice of the meatus urinarius to be the other limit. Of course, for anatomical exactness' sake, the length of the urethra should in every case be subtracted from the measurement. But as in no case can the length of the passage be ascertained, I do not attempt to subtract it, and therefore give the whole length of bladder and urethra.

This length, besides its own immediate value, which I shall attempt to display, is also of use as contributing to an estimate of the capacity of the organ, a quality which it is very difficult justly to appreciate,—for it is very erroneous to suppose that the quantity evacuated, when the bladder is said to be full, gives, in every case, a reliable measurement of the capacity of the organ. The quantity evacuated from a healthy bladder varies under a variety of circumstances, especially the condition of the retentive power of the abdomen. It is generally, like the air expelled in an expiration, only a certain overflow produced by vesical contraction and the bearing-down effort, and may be a greater or less amount, according to the intensity of these actions. Sometimes, even while there is a vesicovaginal fistula of no small dimensions, and a relaxed vagina, a large quantity of water accumulates in the bladder. After delivery, it not rarely happens that, in a case of retention, the urine ceases to flow through the catheter when the recently distended bladder is not half emptied. At such a time, the cessation from the bearing down which has half-emptied the bladder may lead to air regurgitating through the catheter into the bladder. The further emptying of the bladder, in such a case, is effected by continued powerful straining, or pressure by the hand on the brim of the pelvis substituted for it. It is not my purpose to discuss fully the measurement of the capacity of the bladder. I have said enough to show that the amount evacuated is not, in general, a reliable indication of capacity. I believe that, in cases of irritation and inflammation, it is more to be depended on than in health; but in all cases, the length of the bladder will afford important aid in estimating the bulk of contents.

When a woman passes a large quantity of water at once, she must have a capacious bladder; but when a woman only and always passes a small quantity, that circumstance is in itself not sufficient to prove that her bladder is of contracted dimensions.

When, from so-called paralysis of the bladder, the urine is retained, the bladder assumes enormous dimensions. In such circumstances it is well known that, besides involuntary dribbling, the urine is in many cases passed at regular intervals, as in health, but in small and insufficient quantity. The circumstance of the bladder being habitually and naturally only partially emptied, which I now point out, occurs in cases both of health and disease. Partial emptying is itself no sign of disease, nor a sign of exaggerated capacity.

It is, then, necessary to keep in mind that it often happens that a woman is frequently micturating, and yet never nearly emptying the bladder. It is this circumstance that, in certain cases, especially those where the urine is decomposing in the bladder, gives great value to the operation of washing out the bladder; and the opposite condition of very complete evacuation, sometimes renders the operation of washing unnecessary, and, it may be, injurious.

The healthy bladder cannot be measured as the uterus is, or as the hard diseased bladder may be,—for the sound on reaching the summit of the cavity meets, in a healthy organ, with a soft wall, which can be pushed away before its point, sometimes to the extent of an inch; but a healthy bladder, gently pressed by the sound, measures from 5 to 6 inches. The measurement cannot be more precisely given, on account of the distensibility just mentioned.

In cases of disease, the bladder is often found very greatly exceeding this length, as in retention. Such well-known occurrences I shall not dwell upon; but I may mention one interesting case, not the only example of the kind I have met with, where, in the midst of difficulty, the discovery of great length of the bladder was of much service in guiding treatment:—A young girl, not yet arrived at the age of puberty, had all her life wetted her bed involuntarily. At no time had she the natural feeling of desire to make water, and it was only by emptying of the bladder by straining that she avoided wetting herself in the daytime. Nothing, in the way of treatment, had hitherto been of use. A careful physical examination discovered no disease,—only the bladder was much larger than natural, and extremely distensible. The sound could be, without pain, pushed up to very near the umbilicus. From this circumstance I was led to suppose, that thinness and weakness of the vesical walls led to their never resenting the presence of even a large or excessive quantity of urine, and that evacuation therefore took place, either involuntarily when the retentive power of the abdomen was, by repletion of the organ, passively destroyed, as in sleep; or voluntarily, when the effort of straining led to the same result. Further, I argued that if I could increase the strength of the walls by reducing their dimensions, thus also diminishing their easy expansibility, I might restore to the bladder the natural feeling of desire to pass water, produced by repletion of an organ now in a normal state. I therefore ordained that the bladder should be emptied, by catheter and straining of the patient, about six times daily. Whether the theory of the case be right or not, the treatment founded on it was successful. The dimensions of the bladder were diminished, and its natural sensibility restored.

In ordinary cases of catarrh or inflammation of the bladder, examples are found of diminished length of the bladder, and a very common dimension is 4 inches. A case having a less measurement is surely a grave one on that account, and *vice versa;* a case having a measurement above 4 inches is, so far, a favourable one, inasmuch as cure will be more easily and more rapidly attained.

In cases of severe acute cystitis, the capacity of the bladder is sometimes entirely destroyed. There is nought but a potential cavity, or only one capable of containing a few drops of urine, which are soon expelled with acute pain. The sound cannot be passed further than the urethra, that is, about an inch. If, while the patient is under the influence of an anæsthetic, a moment of vesical relaxation is waited for, the sound will pass to the top of the bladder, entering little more than 2 inches.

In some cases of urinary calculus, the bladder has no capacity for urine, or only the very smallest, the stone filling completely the irritated organ.

In cases of inveterate chronic cystitis of the worst kind, I have found the bladder to measure little above 2 inches.

Besides indicating the actual state of the bladder at any given time, measurements at successive periods, showing increase or decrease of dimensions, are valuable and very reliable indications of gradual aggravation or amelioration of the cases. And this indication I have frequently availed myself of in practice, with great satisfaction.

In illustration of this department of the subject, I shall give a few details of an illustrative case which was observed by many physicians and pupils.

M. G., æt. 28, unmarried, has already had one child, and is now six months advanced in pregnancy. She has a copious purulent vaginal discharge. Four weeks ago she was taken with the symptoms of irritable bladder. These have gradually become aggravated. Now her water incessantly passes from her, with the pains of strangury and bearing down. She is manifestly an intense sufferer. The urine contains pus and phosphates. The orifice of the urethra is so dilated as to admit the tip of the finger, and in the urethral cavity a bright red strawberry-like polypus can be seen. It is not sensitive to touch. It is subsequently cut off with scissors. The catheter cannot be passed into the bladder, permeating only the urethra. The finger examining per vaginam, to find an explanation of the obstruction, discovers in the situation of the bladder an ill-defined tumour of the size of a hen's egg, which is evidently the bladder itself in a state of persistent strong contraction. The patient being anæsthetized, the catheter is passed 2½ inches, and the vesical wall is found to be hard and indistensible. Ordinary treatment is resorted to, and within a few days there is decided diminution of the severity of the symptoms; the water on the fourth day of treatment being retained for about an hour. On the twenty-fourth day of treatment, amelioration still progresses, the bladder measures 3½ inches; and the walls of the bladder are softer and more distensible than they previously were. She is prematurely delivered of a male child on the forty-fifth day of treatment. From the lying-in she makes a good recovery. Before she passed from my care, her bladder measured nearly 6 inches, was scarcely more sensitive than is natural, and was soft, smooth, and distensible.

3. *The state of the Walls in respect of Softness or Distensibility.*

Examination by the sound informs the surgeon as to the shape of the bladder. It sometimes also gives information regarding the condition of the walls as to smoothness or roughness, regularity or irregularity of surface. A bladder, the subject of malignant disease, may present nodosities on its surface, which may be more or less distinctly felt by the examiner. A bladder, the subject of chronic cystitis, may present an irregular surface from the projections of bands of muscular fibres lying in various directions.

But a more generally discoverable quality of the walls of the bladder is their condition as to distensibility. The inflamed bladder soon becomes not only small but also hard or indistensible, and this condition can be appreciated by the sound. It is well marked in all cases of contracted bladder. It is remarkably absent in a healthy organ.

J. B., a woman of middle age, was one of the greatest and most heroic sufferers I ever knew. She was a worthy unmarried woman when she first became my patient. Her living she procured by occasional services in families where she was known. She had had many and various illnesses, but I never treated her except for chronic cystitis and its consequences. Neither I nor any other of the physicians whom she consulted found it possible to do her any real service. For the first year of the disease her sufferings were more intense than subsequently. A great renal abscess which she had one winter, and which I opened in the back, seemed to make her bladder sufferings less for a time. Then, for a year, I lost sight of her. But she turned up again, having now only one leg. She said she had been taken with ulcer of the leg, which proved so painful, and was so inveterate, that she got the member cut off. After this she was wooed and married by a man who, she said, was a good husband to her. But she had not been much above two years married when she died. Of the cause of death I know nothing. I give this sketch of J. B.'s case in order to mention that her bladder varied in length at different periods. Sometimes it was little more than 2 inches long; and sometimes, when she was in her better states, it was 3½ inches long. It was always hard, and had a rough surface, giving the examiner, by sound, the impression of a cavity of the size and roughness of the inside of a walnut. More than once, when her sufferings were extreme, and she implored relief at any risk, I dilated the urethra by tents to allow of the digital examination of the bladder, and to produce incontinence of urine. But the operation caused much suffering, and did no good. I repeatedly proposed, when consulting regarding her case, to make a vesico-vaginal fistula with the view of producing incontinence of urine so as to obviate spasm, but I never carried the plan into execution. When I last saw her, within a year of her death, she told me she had now almost no suffering in her bladder as she had before; for now the urine spontaneously and incessantly ran away. She never even attempted to make water. Thus nature had produced the incontinence, the artificial production of which I had only contemplated.

ARTICLE IV.—*Organic Stricture of the Urethra, and its Treatment by Holt's Method.*—Essay for the Fellowship of the Royal College of Surgeons by Examination. By A. G. MILLER, M.D.

IT is now six years[1] since Mr Barnard Holt of the London Westminster Hospital gave to the profession his most interesting and valuable work "On the Immediate Treatment of Stricture of the Urethra by the employment of the 'Stricture Dilator.'" Since then, many surgeons have tried and subsequently adopted his method; and, about two years ago, Mr Holt published the "Opinions and Statistics" of some of these gentlemen. The few cases mentioned in this paper, add to the rapidly accumulating evidence that this operation is, in all respects, satisfactory.

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[1] December 1861.

The efficacy of the operation, under most varied circumstances, is clearly proved by the statistics already published. Out of 742 cases, we might almost say none were fatal from the operation, for though 11 died during observation, most of the deaths could be accounted for otherwise; but even (as Mr Holt says),[1] "supposing, for the sake *of argument, and that only*, that all these deaths are admitted as being attributable to the operation, can any other operation, undertaken for so serious a disease, show such favourable returns" as 1 death in about every 67½, or 1½ per cent.?

Mr Holt's instrument is introduced with, at least, as much ease as any other, especially if the modification proposed by Dr Patrick Heron Watson,[2] be used. The subsequent passing of the dilator along the guide is the simplest part of the operation, for, the instrument once introduced, it requires but a few moments to restore the strictured portion of the urethra to its natural calibre.

These qualities of rapidity and facility of application, combined with efficacy and safety, constitute this operation the most serviceable to all surgeons, more especially those who may be deprived of professional assistance and the advantages of an hospital. No other operation can be reduced to the single passing of an instrument through the stricture, except that proposed by Sir Henry Thompson,[3] which, however, requires considerable care and trouble during the dilatation process, besides being inapplicable to the tighter strictures.

That the urethra may become *impermeable*, there can be no doubt, for there are preparations in our museums which show the canal completely obliterated from loss of substance and long disease.[4] But in most of the cases ordinarily met with, urine has recently been passed in greater or less quantity. To such this operation is applicable, though the number will necessarily be modified by the condition of tightness of the stricture and the skill of the operator. The tighter strictures do not permit Mr Holt's instrument to pass till they have been dilated to the size of No. 4 or 5 bougie. But Dr P. H. Watson has introduced into his practice a modified instrument,[5] which, from the smallness of its point and the rigidity of its shaft, can be used with perfect safety and success on the tightest strictures. By this instrument, therefore, we are enabled to employ Mr Holt's method earlier and more frequently than formerly.

Retention is the condition in which we see many cases, especially in hospital practice; to such, also, Dr Watson's instrument is applicable. The ordinary mode of treating these cases by drawing off the urine with a small catheter and tying it in, leaves the cure of the stricture to after consideration; but if we use the small-pointed instrument, it can be passed at once,—*first*, rupturing the stricture, and *then* drawing off the urine through a full-sized catheter, which need not be tied in, and thus the patient is relieved at the same time of his distress and its cause. Dr Watson's practice in this manner, both hospital and private, has been uniformly successful.

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[1] Opinions and Statistics, p. 54. [2] Fig. 2. [3] R. M. C. S. Trans., 1863.

[4] Edin. Coll. of Surg., 2139, XXXII. F. Edin. University, 1409. [5] Fig. 2.

Fig. 1.

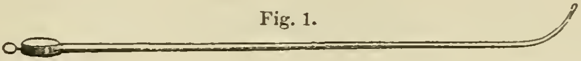

Fig. 2.

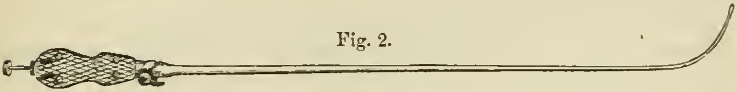

Fig. 3.

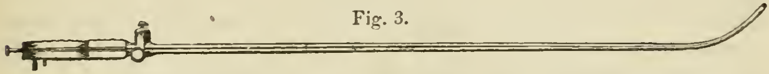

Fig. 1.—Dr P. H. Watson's "Steel Catheter," described in the *Edinburgh Medical Journal* for July 1863.

Fig. 2.—Dr Watson's Improved Stricture Dilator. The peculiarities and advantages of the instrument are:—1*st*, The smallness of its point, owing to the extremity being unperforated, the canal being brought out at the commencement of the curve, as in Fig. 1. 2*d*, The strength and rigidity imparted to it by its construction, the halves being placed laterally, thus opposing their *edges* and *greater diameter* to the principal direction of resistance.

Fig. 3.—Mr Barnard Holt's Dilatator, as described in his Work on Stricture, second edition, 1863, page 7.

There is another condition of the patient rather than of the stricture, to which I would draw attention, namely, where the patient consults the surgeon at his own house. Is it safe to operate at once under such circumstances? If preliminary constitutional treatment be unnecessary, and the results of the immediate operation successful in cases of retention, surely we may with confidence operate in such cases, if there be no serious constitutional disturbance.[1]

In all these instances, however, where preliminary treatment has not been practised, it is well immediately after the operation to administer a large dose of quinine and morphia, as recommended by Mr Holt.

I shall now venture to draw attention to the following cases which have been immediately under my own observation. The first seven of these, Professor Spence has kindly permitted me to quote from the ordinary reports of the wards in the Royal Infirmary, which were under my charge as his Resident Surgeon, during the winter session of 1865–66.

Case I.—James Watt, æt. 48, admitted into the Royal Infirmary, into surgical ward 23, on the 8th December 1865, for retention of urine and stricture of the urethra. Patient has suffered from stricture for 15 years; has been operated on by perineal section and rupture, but, having neglected to return and have instruments passed, has had a relapse. The size of his stream has been diminishing for some weeks. He has been unable to pass water for about twelve hours. His constitution is broken down by intemperance, and he confesses to having indulged to excess lately. His tongue is foul and dry, pulse rapid and feeble, expression anxious, and general appearance unfavourable.

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[1] Dr Watson tells me he operated a week ago upon a patient with very tight stricture, whom he saw for the first time, and who travelled home 40 miles after the operation. This was followed by no inconvenience or ill consequences.

On admission, it was found that the stricture permitted a No. 3 catheter to pass, by which the bladder was relieved. An abscess was discovered in the perineum, which Professor Spence opened freely, the patient being under chloroform. Having introduced his finger, and ascertained that the urethra was not opened into, nor apparently implicated by the abscess, the Professor passed Holt's dilator at once, and subsequently a full-sized catheter, by which the remainder of the urine was drawn off. The patient was sent to bed, and was ordered a quinine mixture, iron, and stimulants.

For the first few days progress was favourable almost beyond expectation. Tongue clean, bowels open, appetite and pulse good. No. 10 catheter was passed regularly, and the urine drawn off for the first forty-eight hours; after that the patient was permitted to pass water himself, which he did without experiencing any difficulty or inconvenience. On the 19th, eleven days after the operation, he had a severe rigor, and an abscess formed in the prostate. From this time he began to get worse, and died on the 22d December, fourteen days after admission and operation.

There are many points of interest in this case, but I would direct special attention to this, that notwithstanding the irritability of the urethra and stricture, manifested by the necessity for chloroform during instrumentation, the rupture of the stricture seemed rather to soothe than otherwise, for, except during the first two days when I had occasion to pass the catheter, he never complained of any uneasiness, and even then the pain was slight and transient. The fatal issue certainly was not accelerated by the operation,—on the contrary, the wonder is that, considering the patient's condition on admission, life was prolonged for fourteen days.

Case II.—John Mackie, æt. 34, was admitted 28th November 1865. He had suffered from stricture for two years. He was operated on 7th December, and dismissed 21st December. I saw this patient in January 1868, more than two years after, when he was perfectly well.

Case III.—John Dow, æt. 41, was admitted 13th December 1865. He had suffered from stricture for several years. He was operated on 20th December, and dismissed 29th January 1866. He died of phthisis in the spring of 1867, more than a year after operation, up to which time his urethra gave him no trouble.

Case IV.—John Cumming, æt. 48, admitted 5th January 1866. He had suffered from stricture for fourteen years. He was operated on 2d February, and absconded 25th February, at which time the stricture was fully dilated.

Case V.—James Wilson, æt. 27, admitted 7th February 1866. He had suffered from stricture for six years. Operated on 16th February, and dismissed 25th February. I saw this patient in January 1867, about a year after operation, when he was perfectly well.

Case VI.—William Steedman, admitted 27th February 1866, was operated on 8th March, and dismissed 21st March. On the 23d April he returned, and No. 12 bougie was passed easily.

Case VII.—Andrew Owenson, admitted 16th April 1866, operated on 17th April, and dismissed 4th May.

Case VIII.—Mr —— is a retired captain, R.N., and has seen some severe service. He has suffered from stricture of the urethra for about twelve years, and has frequently been treated by bougies. The stricture is about an inch in length, and situated about an inch and a half from the external meatus, and permits only a No. 4 bougie to pass; is very hard, almost "cartilaginous," and is complicated by a fistula in perineo, through which a considerable quantity of urine passes during micturition. The patient, his bladder, urethra, and especially the stricture, are very sensitive and irritable. The patient is unable to retain his urine above half an hour or an hour at a time; no constitutional treatment gives any relief, the stricture and fistula keeping up constant irritation. He is also suffering from bronchitis, and has recently had an attack of gout. He is 67 years of age, and his general condition is feeble, and not favourable for any operative procedure.

Such was the condition of the patient when I saw him in the spring of 1866. At first I hesitated about operating on a man of his age and weak constitution, but, after consultation with his ordinary medical attendant, we determined to recommend an operation, being of opinion that this debility and irritability arose from the existence of the stricture. Accordingly, after a week's delay, that the bronchitis might be somewhat recovered from, the stricture was ruptured on the 23d of March.

The only peculiarities in the operation were that chloroform was given on account of the great irritability of the patient, and that some difficulty in passing the dilator was experienced from so hard a stricture being situated in so flexible a portion of the urethra. The only thing which interfered with the patient's rapid and complete recovery was the formation of a small abscess from the induration which existed at the time of the operation. His medical attendant now tells me (twenty-two months after operation), that the stricture is perfectly patent and cured.

Case IX.—On the 8th February 1867, I ruptured a stricture of eighteen years' standing, evacuating the bladder with No. 11 catheter. The patient, a man of 58 years of age, had had no previous constitutional treatment. Immediately after the operation he received a dose of quinine and morphia. After two days, No. 11 bougie was passed, and this repeated every second day for about a week. The patient never complained of uneasiness, nor had any bad symptom after the operation. I have not seen him since.

Case X.—On the 4th May 1867, I operated on John Sprott, æt. 52, for a stricture of several years' standing. He had frequently had retention, but had received no treatment.

In this case, as in No. IX., I used Dr Watson's instrument. The urine was drawn off by a No. 10 catheter, and a quinine and morphia mixture administered. The patient went home, returning about every second day to have a bougie passed, and suffered no inconvenience, except on the 7th, when he had a slight smarting during micturition, after the passing of the instrument. He progressed favourably and rapidly till the 15th, or eleven days after the operation, when he complained of some abdominal pain, which, however, disappeared after a free motion of the bowels. On the 17th, the pain returned more severely, accompanied with inability to make water, and (abdominal) pain in the effort. This pain became localized to a spot about three inches diagonally to the left, and downwards from the umbilicus, and ultimately all the symptoms of obstruction of the small intestine became developed. The ordinary treatment was adopted, opium, etc., intusussceptio being suspected, but the patient sunk very rapidly, and died on the 21st.

On the 23d, about thirty-six hours after death, along with Dr William Rutherford, I examined the body. A portion of the ileum near the cæcum was found tightly tied down by old-standing inflammatory adhesions, the proximal end of which was contracted so as barely to admit the point of my little finger, and at this portion the mucous membrane of the bowel was congested so as almost completely to close the passage. Besides this, the upper and free portion of the bowel, distended with flatus, lay over the adherent portion with a double twist, thus effectually closing and obstructing the gut. There was evidence of slight peritonitis.

The bladder and urethra were removed. In so doing an abscess, probably connected with Cowper's glands, and lying close upon the membranous portion of the urethra, was opened into. The bladder was hypertrophied, and the mucous membrane thickened, as is usual in cases of long standing stricture. The urethra, on being cut open, was without a trace of rupture or cicatrix (*vide* Plate). The membranous portion was attenuated, owing to the abscess formerly mentioned. A preparation was made of the bladder and urethra, which I had the honour of showing before the Medico-Chirurgical Society in December last.[1]

Though the general pathology of stricture has been most carefully and successfully investigated, there is one direction in which research has been limited—namely, as to the condition of parts *after operation*. This is perfectly accounted for by the small number

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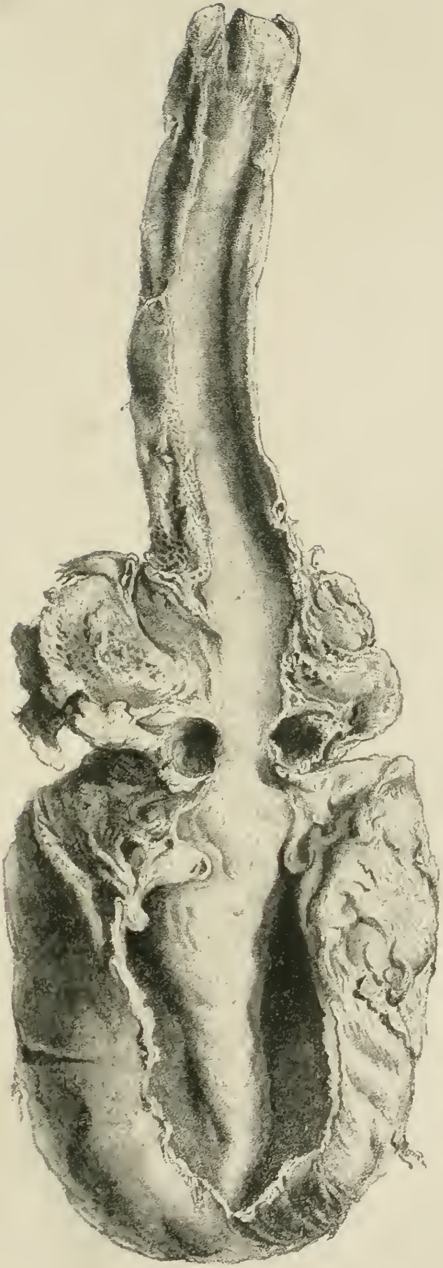
[1] Edinburgh Medical Journal for February 1868.



of patients who die with their strictures in a condition favourable for such investigation.

Mr Holt has recorded[1] a case where he ruptured the stricture and urethra on the dead subject, in which the mucous membrane was torn, "the rent being directly in the median line, and limited to the extent of the obstruction." In his "Opinions and Statistics,"[2] three cases were examined after death. In one (page 21), twelve days after operation, the mucous membrane was "of a red colour, but unbroken." Another, "shortly after" the operation, showed the mucous membrane only "slightly inflamed at the seat of stricture" (page 28). The third was examined twenty-four hours after the operation. In this case sinuses were noticed communicating with the urethra (page 38), and if the mucous membrane at the strictured point had been torn or inflamed, it would surely have been reported. Mr Holt has also kindly informed me of a case which he examined ten days after the operation, when, "beyond some congestion of the urethra at the seat of stricture, there was nothing noticeable." Death, in this case, "was the result of perforation and abscess of the prostate previous to the operation." In my own case, the mucous membrane was "without a trace of rupture or cicatrix," seventeen days after operation.[3]

We perceive, then, that in four cases at any rate, if not *five*, examined from one to seventeen days after operation, the mucous membrane, though "inflamed," was untorn. A difficulty, however, may here be raised: Might not the urethra have been cut open along the line of the rupture? In Mr Holt's case, operated on after death, the rent, though "in the median line," was so extensive and irregular as hardly to have escaped notice, even though the urethra had been opened along the line of it. Besides, it is not likely that when the rent was so visible in this and Dr Watson's case (*vide* Note 2), it would have entirely escaped notice in the five others. It seems to me that the absence of any appearance of a tear can only be explained by the fact (which I believe it to be in most cases) that the mucous membrane *was not torn.* If this be admitted, it is easily explained. The stricture proper being caused by a "deposit of lymph *around the canal*"[4] in the *sub-mucous* tissue, the mucous membrane is generally free from pathological change, as I found it in forty-one out of fifty-four preparations in the Museum of the Royal College of Surgeons, Edin., several of them having been examined microscopically; the membrane is frequently thrown into folds or rugæ by the constricting power apparently of the sub-mucous deposit, as I found in nineteen of the forty-one preparations above mentioned. In *four* cases only of the fifty-four[1] was there any apparent thickening or alteration of the mucous membrane. Thus, the mucous membrane being considerably elastic (probably more so in the living than dead subject), and generally healthy, yields during the operation, and so escapes injury, the abnormal tissue alone being ruptured. But if it be urged that the mucous membrane must be and is ruptured, it seems a great marvel, when we look at the amount of violence done in Mr Holt's preparation, that so little trace was left in the other five cases, and also that so little irritation and constitutional disturbance should follow the *rupture* of a stricture.

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[1] Holt on Stricture, pp. 103, 104, and Plate.

[2] Pp. 27, 28, 38; 50, 51, and 52.

[3] Dr Watson had a case, several years ago, of very tight stricture with retention, in which he found it impossible to introduce a catheter until he had effected the dilatation by means of passing three of the smallest sized bougies in succession. Here a distinct lateral split was observed, the patient dying twelve hours after from uræmic poisoning.

[4] Sir Henry Thompson's Lecture, *Lancet*, 18th January 1868.

The condition of the urethra shown by the five cases above quoted speaks volumes for Mr Holt's method of treatment, and requires no farther comment from me.

I have here recorded ten cases, of which only one can be called unsuccessful, inasmuch as the operation was ineffectual in saving the patient's life, though it enabled him to live in tolerable comfort for fourteen days. These cases, such as they are, I have much pleasure in offering, as a small addition to the number of patients who have benefited by Mr Holt's method of treatment.

ARTICLE V.—*The Atmospheric Germ Theory:* A Lecture delivered to the Royal College of Surgeons in Edinburgh, January 17, 1868. By JOHN HUGHES BENNETT, M.D., F.R.S.E., Professor of the Institutes of Medicine and Senior Professor of Clinical Medicine in the University of Edinburgh, etc., etc.

MR PRESIDENT AND GENTLEMEN,—The subject which I have the honour of bringing before you this evening is one which has engaged the attention of physiologists since the days of Aristotle. That distinguished man, as well as most of the naturalists who lived previous to the time of Harvey, were of opinion that dust, decomposed flesh, and other dead substances might, under the influence of heat, air, and water, give rise to living beings independently of parents. This mysterious process, by a singular perversion of language, has been called spontaneous generation. We all know the story of the shepherd recounted by Virgil, who thought he saw bees spring up in the dead body of an ox, which was implicitly believed by Pliny; and we have read accounts of rats and frogs which were supposed to be engendered by the fruitful earth or teeming waters.

Francis Redi, a physician of Florence, was the first who clearly demonstrated, in 1638, that the larvæ and worms found in a dead body were not produced by putrefaction, but originated from flies' eggs deposited in the flesh.[1] Even he, however, says that he is inclined to believe the entozoa to have a spontaneous origin. The researches into generation, which alone would have conferred undying honour on the name of William Harvey, had it not been associated with the discovery of the circulation, led him to announce the law, "Omne vivum ex ovo." Since his day the belief has been general, that all animals and plants are derived from eggs or seeds; that vitality is always transmitted, and never created; and that, where these fundamental principles cannot be recognised, the minuteness of the germs and their wide diffusion throughout nature, and more especially in the atmosphere, offer a sufficient explanation of what may appear mysterious. Nature, it was argued, must be uniform in her operations, and analogy warrants our supposing that the same law of generation which applies to the higher animals and plants, is equally applicable to the lower.[2]

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[1] In nine I was unable to see anything distinctly, from the condition of the preparations.

The cell theory of growth, which was brought forward by Schleiden and Schwann in 1840, gave a new impulse to investigation by means of the achromatic microscope. With this instrument, physiologists have now traced the changes which ova and seeds undergo during their development in a vast number of animals and plants, and have even followed the former in their wanderings from the body of one animal where they originate, into that of another where they are fully developed. In this way we have an alternate generation, as explained by Steenstrup. In the mouse, for example, we find a cystic worm which, in its body, would be no further developed, but when the mouse is eaten by the cat, the cystic is transformed into the tapeworm, which infests the last named animal. But it has also been shown that certain insects are produced from others without generation by parents. This interruption in descent Owen has called Parthenogenesis, from *παϱθηνεια*, virginity. Thus, two winged insects will produce an animal without wings, from which ten or twelve generations of individuals may be derived without a fresh act of conception, until the last in the chain gives forth another winged insect, when the process is repeated—as in the case of the aphis.[3] These generalizations, by clearing up some very obscure points in embryology, were supposed to have given the *coup de grâce* to the old notion of spontaneous generation.

The ova, seeds, and primary cells, however, which were supposed to be floating in the atmosphere, though constantly looked for, could nowhere be found, and more careful investigation of the numerous forms of life which spring up in putrescent and fermented fluids utterly failed in connecting them with pre-existing germs. It is perhaps unnecessary to remark, that our modern microscopes have reached such perfection that we can examine with the greatest accuracy particles of only one fifty-thousandth of an inch in diameter, which are much more minute than the smallest recognizable ova or seeds. As all efforts, therefore, to discover the supposed germs in the atmosphere with our best instruments have failed, many scientific men who had personally investigated the subject were once more led into the belief of, at least, an equivocal or doubtful generation of the lowest forms of animal and vegetable life. This belief was strengthened by the appearance, in 1859, of a remarkable work by M. Pouchet, Professor of Natural History at Rouen, and a corresponding member of the French Academy of Sciences, on what he called "Heterogenesis, or Spontaneous Generation," as distinguished from Homogenesis, or Generation from Parents. This book contains not only a full history of all that had been previously written and thought on the subject, but gives an account of numerous original experiments and observations made by the author, proving, as he thinks, that infusoria originate in a finely molecular, or, as he calls it, proligerous pellicle on the surface of decomposing fluids, without pre-existing cells or germs of any kind, and therefore independently of parents.

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[1] Experimenta circa Generationem Insectorum.

[2] Most of the arguments on the other side, that is, in favour of Heterogenesis, will be found well stated by Burdach (Physiologie, tome l. p. 8, *et seq.*)—arguments admitted by Allen Thomson "to throw the balance of evidence in favour of the spontaneous production of Infusoria, mould, and the like." (Todd's Cyclopædia, article Generation, vol. ii. p. 430, 1839).

[3] Owen on Parthenogenesis.

The publication of this book has led to a controversy which has continued up to the present moment. The theory of atmospheric germs, or that of the Panspermatists, has been ably sustained by M. Pasteur, who, by new experiments, has revived the doctrine that fermentation and putrefaction are not chemical processes, as has been maintained by Liebig, but physiological phenomena dependent on living germs derived from the atmosphere. These experiments gained for him, in 1859, the Grand Prize for Physiology annually awarded by the French Academy of Sciences. They have all, however, been since repeated by Pouchet and others, and their accuracy as well as the correctness of his conclusions are by them utterly denied. An extraordinary amount of research, ingenuity, and talent has been displayed by the advocates on either side, the results of which will be found recorded in numerous communications printed in the "Comptes Rendus" of the proceedings of the Academy.

Such, then, is an outline of the history of this matter. Before stating more particularly the arguments advanced by the controversialists, on one side or the other, I propose describing shortly the results of some investigations undertaken by myself, with a view of determining precisely the facts of the case, and the nature of the phenomena which have excited so much discussion.

My attention was originally directed to the origin of fungi in 1841, when examining the parasitic plants found growing on living animals, an account of which was published in the Transactions of the Royal Society of this city in 1842. In October 1863, after studying the memoir of M. Pasteur, I commenced a series of observations, with the aid of my former assistant, Dr Argyll Robertson, which were directed (1st) to determine with exactitude, by means of the microscope, the changes which occurred on the surface of infusions during the development of plants and animals there; and (2d) to ascertain what influence the air treated in various ways exercised on such growths. These observations were carefully repeated and extended in October 1864. They have again been recently repeated, and numerous other experiments have been performed, with the aid of my present assistant, Dr Rutherford. These investigations naturally divide themselves into (1st) observations by means of the microscope as to the development of infusoria; and (2d) experiments directed to destroy the supposed germs in the atmosphere so as to prevent putrefaction.

1. *Mode of Development of Infusoria.*

On making a cold or hot infusion of any vegetable or animal substance, covering the vessel with a piece of paper so as to exclude the dust, and then watching it every twelve hours, the first change visible to the eye is a slight opalescence, and the formation of a thin scum or pellicle that floats upon the surface. This appears at times, varying from a few hours to several days, according to the temperature of the atmosphere or the nature of the infusion. On examining the pellicle or film under high magnifying powers,[1] it is seen to be composed of a mass of minute molecules, varying in size from the minutest visible point to that of one thirty-thousandth of an inch in diameter. These molecules are closely aggregated together, and must exist in incalculable numbers. They constitute the primordial mucous layer of Burdach,[2] and the proligerous pellicle of Pouchet.[3] The same pellicle, examined six hours later, shows the molecules to be somewhat enlarged, and these separated by the pressure of the upper glass are already seen here and there to be strongly adhering together in twos and fours, so as to form a little chain. Many twos, also, have apparently melted together so as to form a short staff or filament—*bacterium*. (Fig. 1, *b*.) Twelve hours after this, it may be seen that the grouping of the molecules in twos, threes, and fours has become more general, and that several of these form new groups of eight lengthways. Many of them have melted together to produce longer bacteria. At the edges of the molecular mass, and in the fluid surrounding it, may now be seen a vibratile movement in the shorter bacteria and a serpentine movement in the longer ones, whereby they are propelled forwards in the fluid—*vibrio.* (Fig. 1, *c*, *d*.) From the second or third to the fifth or seventh days, the vibrios are lengthened, evidently by apposition of groups of other molecules, to their ends. These melt togther to form a filament, which may extend a third or half, and in a few cases entirely across the field of the microscope. (Fig. 2.)

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[1] The powers I have employed for the investigation were an excellent lens of $\frac{1}{8}$th of an inch focus, by Ross; but I also frequently used a $\frac{1}{12}$th of an inch by the same maker, and the immersion lens No. 10 of Hartnach, with varying powers of from 600 to 800 diameters linear. Occasionally I confirmed my observations with a lens recently made for me by Messrs Powell & Lealand, of $\frac{1}{25}$th of an inch focus, whereby I obtained an enlargement varying from 1250 to 2000 diameters linear.

[2] Physiologie, par Jourdan, tome ii. p. 123.

[3] Hétérogénie, p. 383.

Fig. 1.

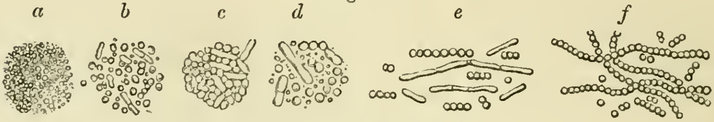

Fig. 1.—*a*, Molecular structure of the proligerous pellicle on its first appearance in a clear animal infusion. *b*, Molecular structure of the same, six hours afterwards. The molecules are separated, and several are seen grouped together in twos and fours. Some of these have melted together so as to produce bacteria, which exhibit a trembling movement. *c*, The structure of the proligerous pellicle on the second day. *d*, The same separated. The molecules are coalescing in rows and melting together to form longer bacteria or vibrios, which move rapidly across the field of the microscope. As their development proceeds, they present the appearance seen in *e*, and in Fig. 2. *f*, Long filaments composed of adhering molecules, 800 *diameters linear.*

The movements visible in the molecules and filaments vary according to the amount of development. At first the molecules which float loose in the fluid exhibit gyrations which cannot be distinguished from Brunonian movements. When short bacteria are formed, these exhibit peculiar vibrations,—often turn round on their own axis in various directions, and are slowly changing their place. They rarely dart rapidly through the fluid, or exhibit a serpentine motion. But when the vibrio is formed, the filament is pushed forward with greater or less velocity, at first presenting a wriggling, but, as it becomes longer, a more decided serpentine motion. A distinct flexure can be seen at certain points in the filaments, between the groups of molecular chains or filaments. Dumas says he has seen the molecules and bacteria uniting endways, a statement the correctness of which Pouchet doubts.[1] On two occasions, however, I was fortunate enough to see this occurrence as represented in the accompanying figures. (See Figs. 3 and 4.)

Fig. 2.

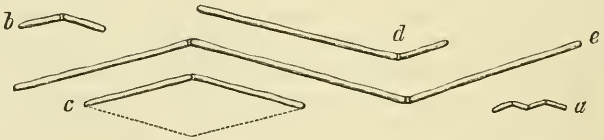

Fig. 2.—(*a*) Vibrio with a serpentine movement. (*b*) Vibrio with one flexure, evidently formed by the union of two bacteria. (*c*) Elongated vibrio with one flexure, the area of the movement marked by a dotted line. (*d*) An elongated vibrio, not moving; a bacterium evidently added at one extremity. (*e*) An elongated vibrio with two flexures, moving rapidly across the field of the microscope. An observation of these vital structures evidently indicates aggregations of bacteria and vibriones of a certain length endways, the flexures occurring at the points of junction, 800 *diameters linear.*

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[1] Nouvelles Expériences, p. 115.

Fig. 3.

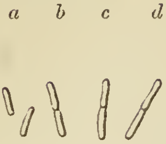

Fig. 4.

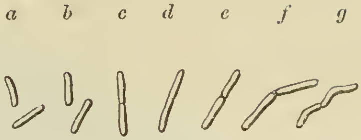

Fig. 3.—*a*, Position of two short bacteria. *b*, The lower bacterium was seen to sink down and unite itself to the upper, and then the two turned round in unison as in *c* and *d*.

Fig. 4.—*a*, Position of two bacteria. *b*, Altered position of the same. *c*, The lower one adhering to the upper. *d*, The two turning together to *e*. *f*, Vital flexure at the middle; and *g*, four flexures, when I saw the vibrio so formed move forward out of the field of the microscope. 800 *diameters linear.*

Pouchet thought that the vibriones exuded a mucous matter, whereby one stuck to the other. If so, such exudation can only be poured out at their extremities, as they only unite lengthways, never crossways. I feel satisfied, however, that the reason the actual union has so seldom been seen is, 1*st*, That it only occurs at certain periods of development, and can only be followed by the eye, when the movements are slow; 2*d*, That amidst such a multitude of minute moving bodies it requires a long time before two can be found exactly on one plane, and can be brought so accurately into focus that they can be watched for a sufficient time. Having, however, in the two instances described and figured, actually seen the coalescence, I can have no doubt whatever that such is the true method of elongation. Numerous other facts seen among elongated vibriones support this view (see Fig. 2).

It may frequently be seen, on again examining the fluid in which these bodies have been moving actively, that they are all motionless, evidently dead. This occurs at various periods. They now rapidly disintegrate, and thus a second molecular mass or pellicle is formed. In this, rounded masses may be seen to form, which strongly refract light not unlike pus corpuscles, or the colourless corpuscles of the blood. These soon begin to move with a jerking motion dependent upon a vibratile cilium attached to one of its extremities—*Monas lens.* In a day or two other cilia are produced, the corpuscle enlarges, is nucleated, and swims through the fluid evenly. Varied forms may now occur in the molecular mass, dependent on the temperature, season of the year, exposure to sun-light, and nature of the infusion, all having independent movements. They have been denominated *Amœbæ*, *Paramecia*, *Vorticellæ*, *Kolpoda*, *Keronæ*, *Glaucoma*, *Trachelius*, *etc.*, *etc.*[1]

Pouchet describes the *Paramecium* as originating in the proligerous pellicle, formed by the breaking down of the primar bacteria and vibriones. It is the secondary histolytic mass of molecules which arrange themselves as seen in Fig. 5.

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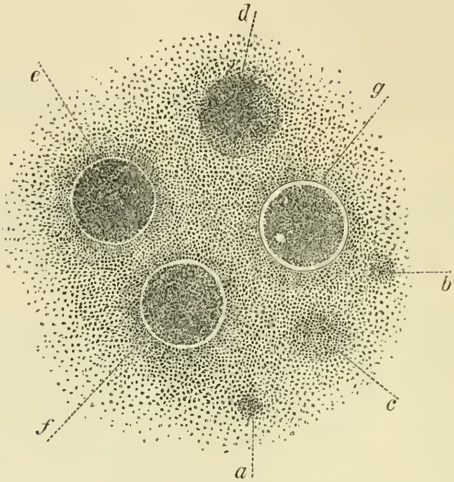
[1] See Ehrenberg's Infusoria.

Fig. 5.



Formation of Ova in the proligerous Membrane.

a, Coalescence of molecules. *b*, The same more advanced. *c*, Still larger mass. *d*. The same assuming a rounded form. *e*, A membrane formed externally. *f*, A nucleus apparent, and complete differentiation of the now perfect ovum from the surrounding molecular mass. —*Pouchet*. 250 *diameters*.

It would occupy too much time to follow the development of all the forms that may arise. They originate always long after the primary vibrios are produced, in the secondary, tertiary, or even later molecular masses, resulting from the disintegration of previous forms.

It frequently happens that soon after some of these higher infusoria are seen, that the pellicle falls to the bottom of the fluid, where it constitutes a dense precipitate, and slowly breaks down; then another scum forms on the surface, and molecules, bacteria, and vibrios are again produced.

These different forms are spoken of by Ehrenberg and other naturalists as being different species;[1] but I think it will be found that the laws, not only of molecular but of alternate generation and parthenogenesis, prevail among them, and one frequently passes into another. Their production is largely dependent on temperature, state of the atmosphere, light,—especially the sun's rays,—and other physical conditions.

At other times, it happens that the molecular mass, instead of being transformed into animalcules, gives origin to minute fungi. In this case the molecules form small masses, which soon melt together to constitute a globular body, from which a process juts out on one side. These are *Torulæ*, which give off processes which are soon transformed into jointed tubes of various diameters, terminating in rows of sporules (*Penicillium*), or capsules containing numerous globular seeds (*Aspergillus*). Occasionally filaments are formed from the direct melting together of molecules arranged longways. (See Fig. 1, *f*.)

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[1] See Ehrenberg's Infusoria.

Here also I think various so-called plants pass into one another, especially torulæ, which are only embryonic forms of higher fungi. In all these cases no kind of animalcule or fungus is ever seen to originate from pre-existing cells or larger bodies, but always from molecules.

That we should sometimes have animalcules, and at others fungi, is a well-known fact, the exact causes or conditions producing which are not yet explained. The Panspermatists, of course, are of opinion that the germs in the atmosphere are of different kinds, and that as they fall into different infusions they produce different results, in the same manner that varieties in ova or seeds develop themselves in peculiar localities or special soils. This assumption, however, seems to me opposed by the following experiment:—

If an infusion be placed in a deep glass vessel, which again stands in the centre of a shallow vessel containing the same infusion, and the whole covered with a large bell glass, it will be found in eight days that on the surface of the former are numerous ciliated animalcules, while on that of the latter only bacteria and vibrios exist. The experiment may be reversed, for if the shallow vessel be filled to the brim, and the deep vessel has only its bottom covered, then the ciliated microzoa will appear in the former, and the non-ciliated in the latter.[1]

As a result of these experiments, Pouchet has formularized a law to the effect that the production of ciliated animalcules is in an inverse ratio to the square of the surface, and that the production of monads is in a direct ratio to the cube of the mass of the same fluid.[2] To this law I have met with some exceptions, animalcules having been produced in some of our recent experiments in the shallow dish, and vegetations in the deep vessel, and *vice versa*.

It is difficult to explain how germs falling from the air on the same infusion, under identically similar conditions, with the exception that the fluid is in vessels of different forms, can vary the results. Whereas the fact that the higher infusoria are formed secondarily out of the disintegrated mass of the simpler ones, which can only take place where that mass is considerable and floating on the surface of deep fluids, directly confirms the molecular theory of growth, and offers an illustration of how successive disintegrations give origin to different formations.[3]

That the infusoria originate and are developed in the molecular pellicle which floats on the surface of putrefying or fermenting liquids, has been admitted by all who have carefully watched that pellicle with the microscope, more especially by Kutzing,[1] Pineau,[2] Nicolet,[3] Pouchet,[4] Jolly and Musset,[5] Schaafhausen,[6] and Mantegazza.[7] The question therefore is, are the molecules that constitute that pellicle derived from the air or the fluid,—are they precipitated from above, or do they float to the surface from below, like the globules of the milk which produce cream?

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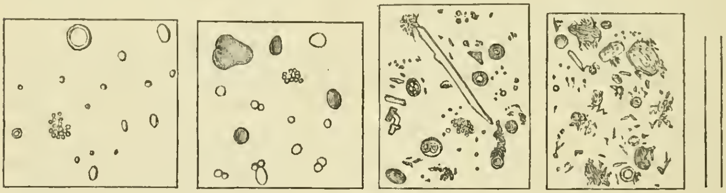
[1] Pouchet's Nouvelles Expériences, etc., pp. 135, 243–245. Paris, 1864.

[2] Nouvelles Expériences, p. 134.

[3] See On the Molecular Theory of Organization, by the Author. Proceedings of Royal Society of Edinburgh, 1861.

Now, it was in consequence of having professed to demonstrate what had escaped all previous observers,—viz., the germs in the air,—that M. Pasteur has made his name so famous. He tells us[8] that he did this by causing a current of air to pass through a glass tube in which a pledget of gun-cotton had been placed. This was then dissolved in ether, and the sediment allowed to collect in a watch-glass. This sediment, after being repeatedly washed, and allowed to remain in distilled water for twenty-four hours, is allowed to dry. A portion of the dried matter is then put upon a slide moistened with a weak solution of potash, and, being covered with another glass, is examined with the microscope. The results he has figured; and, very properly, he has given the scale of magnifying power under which they were drawn (Fig. 8), and which, by careful measurement, I have ascertained to be 180 times linear. I show you his drawings, carefully copied.

Fig. 6. Fig. 7. Fig. 8. Fig. 9. Fig. 10.



Exact copies of the figures given by M. Pasteur of the dust he collected on gun-cotton, magnified 180 diameters. These should be compared with Fig. 1, magnified 800 diameters, showing what is seen to take place when infusoria are forming. Fig. 10, scale of one hundredth of a millimètre.

He says Figs. 6 and 7 represent organized corpuscles from dust collected in twenty-four hours, from the 16th to 17th November 1859. The manner in which these drawings, giving the volume and outline of the bodies, were made, is as follows:—" After the dust has been prepared in the manner described, I took a portion of it from the watch-glass, and diluted it with a solution of potash, consisting of 5 parts of potash in 100 of water. As soon as I perceived a globule evidently organized under the microscope, I drew it. This is how figure 4 was drawn."[1] This description leaves it uncertain whether an exact copy was taken of any portion of the field of the microscope, and, therefore, whether the figure represents the exact number of corpuscles present, and their relation to each other. It only gives their form. But, assuming that the same kind of demonstration was made in each case, we have the relative numbers of these bodies taken from the gun-cotton in Fig. 6. Fig. 7 is another demonstration of the same after the addition of an aqueous solution of iodine. Fig. 8 represents the organized corpuscles associated with amorphous particles obtained on the 25th and 26th of June 1860; Fig. 9, the dust of an intense fog in the month of February 1861. In all these demonstrations he admits the organized corpuscles are comparatively scarce, because, he observes (p. 31), it is frequently necessary to change the field in order to see one of them, whilst at other times several could be seen together.

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[1] See Schaaffhausen, Comptes Rendus, tome liv. p. 1046.

[2] Annales des Sciences Naturelles, 3me série, tome iii., p. 182. This observer thinks he saw disintegrated fibres of meat and of other substances formed directly into vibriones,—in this he was incorrect.

[3] Arcana Naturæ, tome i. p. 2.

[4] Hétérogénie, p. 353. Nouvelles Expériences, p. 111.

[5] Comptes Rendus, tome l., p. 934. [6] Ibid. tome liv. p. 1046.

[7] Institut Lombard, 1852, tome iii.

[8] Annales des Sciences Naturelles, 4me série, tome xvi. p. 25.

M. Pasteur thinks that these drawings indicate the number of organized corpuscles that may be arrested in a small mass of cotton through which 1500 litres of air, in one of the less-frequented streets of Paris, have passed in twenty-four hours, about three or four yards from the ground. These he estimates at several millions in a litre (p. 29).

Now, it must be remembered that M. Pasteur is a chemist, and it will be admitted by every histologist that no method could be more unsatisfactory for determining either the nature or the number of the corpuscles than the one he adopted. The solution of the cotton in ether, the frequent soakings in water, the desiccation, and then the addition of a solution of potash, must completely alter the character of any living corpuscles in the atmosphere. Then the forms he assumes to be organic, are not necessarily so. They are exceedingly frequent among mineral substances, and siliceous rounded forms are common, which of course resist sulphuric acid.

Numerous investigations have been made, both before and since M. Pasteur wrote, to determine the nature of dust floating in the atmosphere—of that dust, for example, which a ray of sunlight reveals to us, when admitted into a chamber. It consists, for the most part, of different kinds of starch corpuscles; the debris of clothing, especially filaments of cotton, silk, and wool; the results of different kinds of combustion, whether of coal or of wood; various mineral bodies, globular or ovoid, amorphous or crystalline; and minute fragments of insects and vegetables; very rarely small seeds and microscopic animalcules.

These constituents vary to such an extent in different localities, as to enable the observer, in some cases, to determine whence the dust was collected. Starch corpuscles abound in the neighbourhood of flour-mills and bakeries; fragments of clothing where there have been crowded assemblies of persons, cotton and wool being predominant if the persons belong to the poorer classes, and silk if the upper classes have been present; the products of combustion predominate in smoky localities; mineral particles on the roads and highways; seeds, fragments of vegetables and insects, in market places, gardens, etc., etc. But although these constituents of the air vary in different places, infusoria, produced in all of them, are identically the same.[1]

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[1] Annales des Sciences Naturelles, 4me série, tome xvi. p. 25.

This has been tested in various ways. The dust has been ransacked to discover organic germs,—collected and carefully examined with the microscope, near the soil, and on the summits of the highest buildings, not only in frequented, but in desert places; in crowded assemblies, as well as in empty Gothic cathedrals and ancient vaults—in the ancient palace of Karnack, on the banks of the Nile; in the tomb of Rhamses II. at the extremity of the Desert; as well as in the central chambers of the great pyramid of Gizeh. The chief element of the dust collected in these places has been found to be starch corpuscles.[2] Large quantities of air have been drawn through tubes by aspirators, and collected on cotton, in distilled water, or projected on glass. The feathery snow, which, falling through the atmosphere, may be well supposed to collect its contents, has been melted, and the precipitate carefully collected. The emanations of marshy places, such as those of the Maremma in Tuscany, have been specially investigated.[3] The larynges and mucous pulmonary surfaces of numerous animals have been explored, even to the inmost bone cavities of birds. On the summit of Mont Blanc, amidst eternal snow; on the glaciers of the Jura and of the Pyrenees, and in the deep crevasse;[4] on the burning plains of Egypt, and in the markets of Constantinople, the dust of the atmosphere has been microscopically examined,—and in all with a like negative result as to the existence of germs. Nowhere could they be seen, or if a few, in the opinion of some, were visible, could they in any way account for the multitude of minute infusoria, which, in all these localities, not only readily spring up in putrid fluids, but in every instance are identically the same.[5]

Indeed, on examining the drawings of M. Pasteur (see Figs. 6 to 9), let us suppose that the few bodies he has figured are truly spores, as he believes them to be, which have preserved their form—after the action of ether, several soakings of twenty-four hours each in water, the desiccation, and subsequent mixture with a weak solution of potash.

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[1] Pouchet's Nouvelles Expériences, p. 73, *et seq.*

[2] Pouchet's Hétérogénie, p. 446.

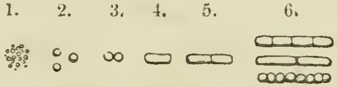
[3] L. Gigot's Récherches expérimentales sur la Nature des Émanations marécageuses, Paris, 1859. Récherches sur l'Air des Maremmes de la Toscane, par M. E. Bechi. Comptes Rendus, tome lii. p. 852.

[4] Comptes Rendus, tome lvii. p. 558.

[5] Nouvelles Expériences, p. 75.

How, it may be asked, could these bodies produce the incalculable millions of minute molecules in the smallest fragment of the pellicle we can transfer to our microscopes, in which, as we have seen, the infusoria originate? It has been supposed that, on falling from the air, they undergo rapid division, and spread over the surface with the greatest rapidity; but no one has ever seen this remarkable phenomenon, and the slightest consideration must show that such an assumption is completely adverse to what can be readily demonstrated on the surface of every infusion. Thus, there can be no doubt that the minute molecules are formed first, and the bacteria, vibrios, and filaments, last. Supposing that the primary molecules, figured No. 1, enlarge to a certain point, No. 2, and then divide, how is it possible to explain the formation of elongated filaments at all? Surely the idea of their rapid multiplication by division is opposed to that of their power of elongating into bacteria and vibrios, whether by aggregation or growth from their extremities. It may frequently be seen that No. 3 is composed of molecules of exactly the same size as No. 2, which are floating loose,—a fact in favour of their coalescence rather than of their division, as then they would be reduced to half the size. It is more probable that although the smaller molecules may increase by imbibition of fluids, they have yet a constant tendency to aggregate together and melt into one another. No. 3 is not a proof of No. 2 dividing, but of two molecules coalescing; and when they unite, they form No. 4. Two or more of these uniting, form Nos. 5 and 6. When a similar process to this goes on in mineral bodies, as shown by Mr Rainey,[1] it cannot suggest division, but union; and this for the obvious reason, that the former would lead to disintegration, whereas, it can be seen in one case as in the other, that development is the result. In short, in the same manner as a tube is formed by a coalescence of cells, so is this minute vibratile vibrio formed by the coalescence of molecules. It may be argued, however, that each molecule elongates itself—that is, No. 2 is converted into No. 4; this into Nos. 5 and 6; and that No. 3 are sporules or ova, caused by the disintegration of No. 6. But this view is opposed by the fact that Nos. 1, 2, and 3 are seen before Nos. 4, 5, and 6 are produced. Of this all have satisfied themselves who have examined animal and vegetable infusions; and the conclusion, therefore, cannot be resisted, that the vibrios are derived from the molecules, and not the molecules from the vibrios.

Fig. 11.



Stages in the Development of Vibriones.

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[1] On the Mode of Formation of Shells, etc., 8vo. London, 1855, p. 12.

But it may also be supposed, that while some have the property of dividing, others are capable of elongating or aggregating; but this view is not only opposed to observation, but is at variance with all that we know of embryonic development in plants and animals. When a plant consists of a single structural element, such as a cell or a tube, it will, I think, be admitted that growth in the sense of increased bulk, and growth in the sense of multiplication of parts by division, do not proceed at the same moment of time. Every plant and animal follows, in this respect, the same law. Nutrition is carried on up to a certain point of maturity, and then, and not till then, does generation, or the separation of parts to form new creatures, take place. When plants and animals are complex in their structure, one organ or segment may be growing, while another is disintegrating; but in individual organs there is a period for growth and reparation, and a period for division or separation. Hence, it seems to me, I am correct in thinking that if the primary molecules in the fibre of an infusion possess the property of dividing, they cannot also, at the same moment, possess the property of elongating and forming filaments. The one function is subversive of the other. While, then, a cell or a vibrio may possess the property of growth and division, these two functions must be exercised at different periods of time,—so that, in reference to the early stage of formation, if the molecules divide, bacteria, vibrios, and filaments could not be formed. A mass of vibrionic molecules is not a compound organism; it is a mere aggregation of similar simple elements. Each of these in passing through certain phases of development may be arrested, or reach maturity at various periods, so that we frequently see different forms present at one time; but that the same forms and the same stages of growth should exhibit directly opposite functions, is surely not in accordance with physiological knowledge.

The conclusion we must arrive at therefore is, that the molecules seen on the surface of infusions out of which animalcules and fungi are produced, are not derived from the air.

Neither can they be supposed to pre-exist in the fluid, as then they would be readily seen, which they never are at the commencement. On this point nothing can be clearer than the microscopical evidence, so that it results from the facts and arguments I have brought before you, that the more simple infusoria do not originate from cells or minute germs at all, whether in the atmosphere or in the fluid. This is the almost universal conviction of histologists who have carefully investigated the matter.

2. *Chemical Experiments which have been directed to destroy the supposed Germs in the Atmosphere, so as to prevent Fermentation and Putrefaction.*

Schutze, in 1837,[1] after heating an infusion to the boiling point, connected it with two of Liebig's bulbs, one containing sulphuric acid, and the other concentrated solution of potash. The air forced through these liquids he thought capable of destroying the atmospheric germs.

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[1] Poggendorf's Annalen, 1837, p. 41; and Edinburgh New Philosophical Journal, 1837.

Schwann also, in 1837,[1] forced air, with the same view, through metallic tubes heated to redness; and found, when so calcined, it occasionally prevented infusorial growth. He thought that oxygen alone was not the cause of fermentation, but some substance in the air capable of being destroyed by heat.

Schræder and Dusch, in 1859, filtered the air through cotton before bringing it in contact with organic fluids. They found that some did and others did not undergo putrefaction, and were induced to believe that the presence of oxygen, and the formation of an acid, were the cause of fermentation. Schræder afterwards found that the yolk of an egg, milk, and the juice of meat without water, putrefied in air filtered through cotton, and supposed it to contain an active substance, the nature of which was unknown.[2]

The experiments of Schutze, Schwann, Schræder, and Dusch have been frequently repeated without preventing the growth of fungi.[3]

Again, it is almost universally considered that the heat of boiling water or cold at zero will destroy all kinds of animal and vegetable life. Indeed, to imagine that the minute molecules or vibrios of which we have been speaking, or small ova and sporules consisting of oleo-albuminous matter without any envelope, would remain in boiling water for hours and retain their vitality, must be regarded as a violent assumption. Three or four minutes' boiling of a hen's egg not only kills it, but converts its whole substance into a hard mass. There is no seed known which, when taken out of its indurated shell or case, is capable of germinating after being boiled for a short time.[4] Yet nothing is more certain than that long ebullition of various infusions has wholly failed to prevent the formation in them of animal and vegetable growths.

Pouchet and others have frequently performed the following experiment:—An open flask was plunged into and filled with a decoction of barley which had been boiling for six hours. A stopper was introduced into it below the liquid, and on taking it out the whole neck of the flask was immediately plunged into melting sealing-wax, and hermetically closed. In six days some yeast was observed in it, at a temperature of 18° Cent. The following day the temperature was raised suddenly to 27°, when the flask burst, and then it was seen by the naked eye, and by the microscope, that it contained a notable quantity of yeast.[1] Now, yeast is a plant, which was thus proved to have grown in an infusion that had long been boiling, and from which all atmospheric air had been expelled. As, therefore, neither calcined air, sulphuric acid, liquor potassæ, gun-cotton, or a boiling temperature have failed to prevent the production of infusoria, or destroy the supposed germs in the air or infusion, I determined, in 1863, to try the effects of all these destructive agents, with the exception of the first, at once, and with the greatest possible care.

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[1] Poggendorf's Annalen, 1837, p. 184.

[2] Quoted by Pasteur, *opus cit.*, p. 16.

[3] See Pasteur, *opus cit.*, pp. 34, 35. Pouchet's Hétérogénie, pp. 252, *et seq.*

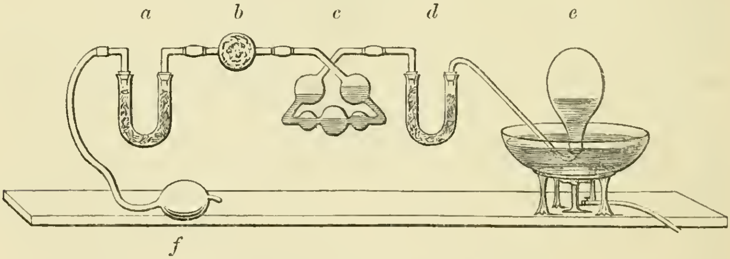
[4] See some conclusive experiments recently performed on this subject by Meunier. Comptes Rendus, tome lxii. p. 992. See also Pouchet's Experiments on the Seeds of Medicago from Brazil. Comptes Rendus, tome lxii. p. 941.

First Series of Experiments.

On the 17th and 18th of October 1864, I performed the following experiments in my laboratory, with the assistance of Dr Argyll Robertson:—

Decoctions of liquorice root, of tea, and of hay were kept at the boiling temperature in a porcelain basin, over a gas flame. Flasks filled with and inverted in the boiling fluid, had air pumped into them to the extent of three-fourths of their volume, which had passed through (1st) a U-shaped tube containing liquor potassæ; (2d) Liebig bulbs, containing sulphuric acid; (3d) a hollow glass ball containing gun-cotton; and (4th) another U-shaped tube with sulphuric acid. All the bent tubes were filled with fragments of pumice-stone, to break up the air, so as to prevent the possibility of any germs passing through in the centre of bubbles. The bent glass tube leading from the last U-shaped tube, filled with sulphuric acid and pumice-stone, was also filled with the acid, so as to destroy any germs that might be supposed to adhere to the interior. After the air so prepared had entered the flask, corks, which had been for some time boiled in the infusion, were by means of iron forceps inserted in the necks of the flasks, and the entrance of fresh air prevented. Further, on removing the flask from the boiling infusion, the cork and neck were hermetically closed by plunging them

Fig. 12.



Arrangement of the Apparatus employed.

a, Bent tube containing liquor potassæ and pumice-stone. *b*, Hollow glass ball containing gun-cotton. *c*, Liebig's bulbs, containing sulphuric acid. *d*, Bent tube containing sulphuric acid and pumice-stone. *e*, Infusion kept at the boiling point, when air was pumped into it. *f*, Caoutchouc pump.

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[1] Hétérogénie, p. 629.

into melted sealing-wax. At the same time bottles or flasks containing the same infusion, but having a similar proportion of ordinary air, were sealed or corked up, so as to be contrasted with the influence of the prepared air.

The results were that the infusions in all the flasks in contact with ordinary air, five in number, were rendered turbid, or covered with fungi in from six to twelve days; whereas all the infusions, with one exception, which were exposed to the prepared air, eight in number, also became turbid and contained fungi, but at periods varying from four to nine months.

Some of these flasks I have recently opened (January 7, 1868), and found that they contained deposits of bacteria and vibriones, with molecular debris of an indeterminate character.

Second Series of Experiments.

About a year afterwards, on the 3d and 13th of October 1865, the experiment was repeated in exactly the same manner, with separate decoctions of liquorice and dulcamara roots, and with tea. Again, flasks holding similar decoctions with ordinary air were contrasted with those which held the prepared air. In these last, three in number, fungi appeared in from six to twelve days; and in the former, all, nine in number, with one exception, contained fungi, or were very turbid, in periods varying from four to nine months.

The exception, which I now show you in the first series of experiments, was a decoction of hay; and in the second series, a decoction of dulcamara root. It will be observed that both these infusions in the flasks remain clear and without the slightest sediment or pellicle, although the prepared air they contain, the fluid, the flask itself, and the mode of closing and sealing it, were identically the same as in all the other flasks which were filled and closed at the same time. To suppose that these two flasks were the only ones from which atmospheric germs were excluded, must appear to those who will examine them all, a strong assumption. Neither can it be true that in all the flasks, except one in each series, air could have entered since they were closed. I have placed the whole in vacuo under the bell-glass of an air-pump, and from none could any air whatever be extracted.

Three of the flasks in the second series of experiments I have opened, and found in two the same molecules and indeterminate debris as in the others; but in the third, a white cottony fungus, the size of a small pea, was floating in a tolerably clear decoction of dulcamara, which, on microscopic examination, was composed of minute jointed filaments forming a dense network.

Third Series of Experiments.

A third series of experiments was performed with the assistance of Dr Rutherford, October 14, 1867. In this case glass stoppered bottles were employed, it having been suggested that sporules or germs might have been concealed in the corks formerly employed, although they had been well boiled. In this case we employed weak decoction of tea, of beef, and of hay, taking care as before, that one bottle having ordinary instead of prepared air introduced, should be kept for the purpose of comparison. In all the bottles with common air, six in number, fungi appeared in from one to two months and a half. In one bottle air was passed through carbolic acid only, and you see that it presents a distinct proligerous membrane; all the others as yet remain unchanged, but we cannot as yet regard the experiments as complete.

Fourth Series of Experiments.

In all these cases it will be observed, that the prepared air was passed through a boiling fluid and was consequently rarefied. It occurred to me to allow the fluid in the inverted bottles to get cold before introducing the prepared air. This was done in four cases, December 28, and I show you that a fungus has appeared in all. It would seem, therefore, that rarefaction of the air greatly influences the result. It may easily be conceived that air subjected to a boiling temperature is so expanded as scarcely to merit the name of air, and that it becomes more or less unfit for the purpose of sustaining animal or vegetable life.

Fifth Series of Experiments.

A fifth series of experiments was performed on the 4th of the present month (January 1868), with the same precautions, and with an infusion of dulcamara only. We filled sixteen bottles. Into four of these we pumped ordinary air into a cold infusion. Into four others ordinary air was pumped into a boiling infusion, and we repeated this experiment with four bottles each of the boiling and of the cold infusion with prepared air. The result, up to the present moment is, that the fluid has become turbid only in the four which contained ordinary air not rarefied.[1] This experiment also is not yet complete, but serves to show the great influence of rarified air.

Sixth Series of Experiments.

A sixth series of experiments was performed on the 11th of the present month. It having been asserted by M. Pasteur[2] that passing air through bent tubes, by hindering the access of germs and allowing them to be deposited on the sides of the glass, prevented the growth of infusoria, twelve bottles were prepared, four of which contained an infusion of dulcamara, another four a decoction of putrid meat, and a third four, yeast water. The bottles were plunged into and filled with the infusion when boiling, inverted in it, and allowed to get cold. The air was pumped gently through a bent tube, five feet in length, having fourteen sharp bends, each four inches long, into three bottles of each series, and ordinary air was admitted to the fourth. The result is, that already, January 17, the fluids in all are turbid, with the exception of two bottles containing infusion of dulcamara,[1] so that bent tubes appear to intercept none of the supposed germs.

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[1] A fungus has since appeared in one of the bottles containing non-rarified prepared air. (Feb. 12).

[2] Comptes Rendus, tome l. p. 306.

These experiments, on the whole, appear to me to be totally adverse to the atmospheric germ theory, and to indicate that the production or non-production of infusoria depends, for the most part, on the temperature, chemical constitution, density, and other physical properties of the air, rather than on living organisms there, which are developed in the fluid. Still, in every series of experiments, it will be noticed that there are one or two exceptions. This has also been observed by Pasteur in most of his experiments, and he attributes them to some currents or limited portions of air being rich in germs, whilst others are free from them.[2] But that this explanation applies to my laboratory, in which all the experiments described were made, is not probable.

It is now admitted by M. Pasteur that the boiling temperature, that is 100° Centigrade, does not prevent the growth of the supposed germs in the atmosphere ; but instead of considering this fact hostile to his theory, he concludes from it that the germs have the power of resisting that amount of heat, and of being most tenacious of life ; but he says, 130° Centigrade always destroys their vitality. M. Pouchet, however, has shown that the air, and the organic matter placed in boiling water, will germinate after they have been exposed to a heat of even 150°, and he says it may be raised to 200° Centigrade, and yet animalcules and fungi will develope themselves.[3]

In the same manner, air and infusions exposed to intense cold still produce animalcules, but, according to Pasteur, not so readily. Twenty flasks containing boiled infusions, and from which the air was expelled, were opened by him with excessive precaution on the Mer de Glace at Montanvert on the Jura. Notwithstanding the purity and extreme coldness of the air, infusoria appeared in five of his flasks.

As an illustration of the manner in which the controversy on this subject has been carried on in the Imperial Academy of Sciences in Paris, I may give a short account of that portion of it referring to the Glacier experiments. M.M. Pouchet, Jolly, and Musset opened eight similar flasks used by M. Pasteur at Montanvert, on the Glacier of the Maladetta, in the Spanish Pyrenees, 9000 feet above the sea, and 3000 feet higher than that of Montanvert, using all the precautions required by M. Pasteur. In addition, before cutting off the ends of their hermetically sealed tubes with a file, previously heated by a lamp, they held the flasks above their heads.

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[1] These also have since become turbid. (Jan. 30).

[2] Comptes Rendus, tome li. pp. 350, 351.

[3] Ibid. tome l. p. 1015.

Notwithstanding, infusoria appeared in all the infusions a few days afterwards.[1]

To this communication, presented to the Academy, Sept. 21, 1863, M. Pasteur replies, Nov. 2,[2] saying that he is rejoiced that his learned adversaries have gone to such an altitude to repeat his experiments; but observes that they did not take the necessary precautions. They only had eight flasks, whereas he had twenty; they shook their flasks before opening them, which he took care not to do; and they had the imprudence to use a file, instead of a pair of pincers with long branches, heated in the flame of a lamp. He says that the thumb and fingers holding the file were too near the opening into the flask, and may have conveyed germs there, especially as they were not passed through the flame, as the file was.[3] He defies them, if they take sufficient precautions, to obtain infusoria in all their flasks.[4]

MM. Jolly and Musset accept the defiance of M. Pasteur, Nov. 16,[5] and, in fact, on the 13th June following, they send a memoir to the Academy, stating that they had returned to the Maladetta, this time with twenty-two flasks—that is two more than were used by M. Pasteur—fulfilled all his conditions, not forgetting the pincers with long branches, properly heated, and found that infusoria appeared in every flask without exception in four days;[6] and so ended this part of the controversy.

Numerous other important questions have been debated before the Academy; among these are the changes which take place in the air confined in the flasks, founded on numerous analyses;[7] the observations of Jolly and Musset with regard to vibrios living in distilled water;[8] the statement by M. Pasteur that neither free oxygen nor atmospheric air are necessary for the growth of infusoria,[9] and that they will develop themselves in carbonic acid gas only. Lastly, the same chemist declares that, notwithstanding his often declared opinion that ferments are living beings and not dead matter, that he can produce fermentation with the ashes of yeast.[10] Some of these statements are confirmed by Donné,[11] who found that hens eggs became putrid without the formation of vibrios or other infusoria. This observation, while it might serve to prove that atmospheric air passing through the egg-shell separated the germs by filtration, is wholly opposed to the idea that putrefaction is necessarily caused by such germs.

The only conclusion I can draw from the numerous contradictory and ingenious communications presented to the Academy of Sciences during the last eight years on this matter is, that not the slightest proof is given by the chemists, with M. Pasteur at their head, that fermentation and putrefaction are necessarily dependent on living germs existing in the atmosphere. They rather tend to show that these are phenomena of a chemical nature, as was ably maintained by Liebig.[1] Did we indeed confine our reading to the papers of M. Pasteur—that is, to one side of the case—we could easily persuade ourselves of his correctness;[2] but every one of his experiments has been repeated by several independent investigators, who have shown his imagined proofs as to the existence of atmospheric germs to be altogether erroneous. We may conclude, therefore, that living germs are not necessarily the cause of putrefaction and fermentation; neither is it necessary to believe that ferments are living at all—they may be dead. This, if not admitted, seems to be implied by Pasteur himself, who tells us he can now excite these processes, not by fresh yeast only, but by the ashes of yeast.[3] That they may be induced by dead organic matter, which has been subjected to a direct temperature of 150° or 200° Centigrade—a heat utterly incompatible with the existence of life—we have seen to have been proved by Pouchet, Jolly, Musset, and others.

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[1] Comptes Rendus, tome lvii. p. 558.
[2] Ibid. p. 724.
[3] Ibid. p. 725.
[4] Ibid. p. 726.
[5] Ibid. pp. 842–845.
[6] Ibid. lvi. p. 1122.
[7] Ibid. pp. 734–739.
[8] Ibid. lv. p. 491.
[9] Ibid. li. pp. 345, 346; liv. p. 267; lvi. p. 1191.
[10] Ibid. lvi. pp. 418, 419.
[11] Ibid. lviii. pp. 951, 952; lxv. p. 602.

The idea that these imaginary germs were the cause of putrefaction, of disease, of blights among vegetables, and other evils, originated with Kircher and the pathologists of the seventeenth century. It has been frequently revived, but always shown to be erroneous. In 1852, cholera was supposed to be occasioned by a fungus that really existed in the dejections, but which Mr Busk pointed out was the uredo segetum of diseased wheat, which entered the body in the form of bread. Certain well-known parasitic diseases are spread by contact, such as scabies, which, as it depends upon an insect burrowing in the skin, may be understood to crawl from one person to another. Favus, also, I succeeded, in 1841, in proving might be made to grow on diseased surfaces of otherwise healthy persons; but many of our unquestionably infectious diseases, such as smallpox, scarlatina, measles and typhus, have no such origin. It has been attempted to be proved, indeed, by Lemaire,[4] that in the condensed vapours of hospitals and other putrid localities, vibrios may be found; but that vibrios are the cause of these various diseases, is not only not proved, but from what has been stated, is highly improbable.

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[1] Letters on Chemistry, letters 18 and 19.

[2] This is what unfortunately seems to have been done by the Commission of the Academy, which made a report on this subject, Feb. 20, 1865 (Comptes Rendus, tome xl. p. 384). No histologist was on the Commission, which refused to enter upon any kind of microscopical inquiry; Messrs Pouchet, Jolly, and Musset, under such circumstances, very properly took no part in the investigation, which, consequently, was altogether one-sided, and of no scientific value.

[3] Comptes Rendus, tome lvi. pp. 418, 419.

[4] Ibid. tome lix pp. 317–428.

What, then, it may be asked, is the origin of the infusoria, vegetable and animal, that we find in organic fluids during fermentation and putrefaction? In answer to this question, I say they originate in oleo-albuminous molecules, which are formed in organic fluids, and which, floating to the surface, form the pellicle or proligerous matter. There, under the influence of certain conditions, such as temperature, light, chemical exchanges, density, pressure, and composition of atmospheric air, and of the fluid, etc., the molecules, by their coalescence, produce the lower forms of vegetable and animal life.

Hallier, describing the development of Penicillium crustaceum, tells us, that after all movement in the primary molecular mass has ceased, the molecules arrange themselves in long lines, which he calls Leptothrix chains. (Fig. 1, *f.*) From the melting together of these, the delicate filaments forming Leptothrix buccalis are evidently produced; and these, according to him, by further development, pass into Penicillium crustaceum.[1] Why the molecules should sometimes arrange themselves in long rows, and at others into rounded masses (compare Fig. 1, *a*, and *f*), is probably dependent on varying degrees of limpidity and viscosity. But why both these germs should sometimes possess an inherent power of contractility, and at others not, it is impossible as yet even to surmise. But on the determination of this point, the variations existing between the different kinds of fermentation and putrefaction are evidently dependent.

But the ultra vitalist will ask, If so, what becomes of the doctrines —"Every living thing from an egg;" "Every cell from a cell;" and "All life from life?" To this I reply, these expressions are formulæ, which only impose upon the understanding, check the search after truth, and are already completely overthrown by the advance of science. A few words with regard to each of them will, I think, show this.

I.—After what has been said, I need not dwell upon the first of these views, viz., *omne vivum ex ovo*, for the facts we have described are quite hostile to it.

II.—That every cell comes from a cell, is a recent doctrine, which, from its first enunciation, was rejected by almost all the first histologists of Europe. The illustrious founders of the cell theory, Schleiden and Schwann, clearly showed that primary cells originated in a molecular mass which they called a blastema. Max. Schultze in 1861,[2] and Bruecke in 1862,[3] demonstrated that a cell wall had nothing to do with the development of molecular masses, and now it is not considered essential to the conception of a cell by the most inveterate cellulists. Having thus discarded from this farfamed elementary organ its external wall or boundary—which can alone entitle it to be called a cell at all—they have only to carry their conceptions a step farther, and banish the nucleus also. There will then remain an organic fluid or molecular material, the true potential substance, in which all growth takes its origin.

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[1] Gährungserscheinungen, p. 51, and fig. 12 . Leipzig, 1867.

[2] Reichert and Du Bois-Reymond's Archiv., 1861, p. 9.

[3] Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften, 1862, p. 382.

That molecular, nuclear, and cell forms are not necessarily organic has been proved by the researches of Ascherson,[1] Rainey,[2] and Montgomery.[3] They have succeeded in producing from various viscous substances, more especially oil and albumen, different kinds of gums,[4] and a material obtained from yolk of egg called protagon, appearances resembling almost every kind of elementary texture. The idea that there are such things as histological organic forms, which are necessarily indicative of life, must now be regarded as fallacious. It has deceived many, and among the rest, as we have seen, M. Pasteur. What now interests us are the transformations which these forms undergo, and the passage of one into the other. This inquiry must lead us to the molecular theory of organization, which inculcates that all the tissues are formed by the successive production of histogenetic or formative, and histolytic or disintegrative molecules;[5] and it appears to me that its correctness is singularly confirmed by the investigations that have been brought before you this evening. In short, we have first molecular depositions in organic fluids; these aggregate to form nuclei, cells, and other tissues; and these again, by constantly breaking down and re-forming, gradually elaborate and build up the higher organisms. It would appear that nature is constantly operating in the organic and inorganic worlds by the process of molecular coalescence; that the notion of every particle being necessarily derived from its like is erroneous; and that the law of descent from parents, which we recognise in the higher animals, changes, as we descend in the scale, first to *parthenogenesis*, whereby this direct descent is broken, and ultimately to *heterogenesis*, in which it is lost.

This theory irresistibly recalls to our recollection the philosophical speculations of the Greeks, and especially of Democritus, Anaxagoras, and Empedocles, and suggests to us that history in science, like history in morals or in politics, often repeats itself, so that our predecessors may often be regarded not only as the originators of speculative opinions in the past, but also as prophets, who have foretold in the cycle of events the deductive truths of the future.

III.—The doctrine that life must necessarily spring from life is essentially connected with the view of direct descent from parents. Previous to the time of Harvey, life was considered to be an independent principle, capable of being added to or removed from inert matter. Such was the opinion of the ancient philosophers, as allegorically explained by the fable of Prometheus, who animated the marble statue by fire stolen from heaven. In later times, Buffon imagined life, like matter, to be indestructible. According to him every living molecule had a life of its own, and the method by which it manifested its function depended on its association with other molecules. Thus, the body of an animal or a plant was the aggregation of a multitude of minute living beings arranged in a particular way. The death of the complex compound was simply a dissolution of one of these associations, and the organic molecules thus set at liberty wandered about until they once more combined with a plant or animal—here with a monad, there with a quadruped. The *materia vitæ diffusa* of John Hunter was something similar.

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[1] Muller's Archiv, 1840.

[2] Mode of Formation of Shells, etc., London, 1858.

[3] The Formation of so-called Cells, etc., London, 1867.

[4] Rainey in Medical Times, January 1868.

[5] See the author's paper on the Molecular Theory of Organization. Proceedings of the Royal Society of Edinburgh, April 1, 1861; and Microscopical Journal, 1861.

Our modern view of life is, not that it is independent of matter, but a condition of matter: in other words, that material substances found in the atmosphere, and in plants and animals, influenced by certain forces, have peculiar properties communicated to them. These properties are contractility, sensibility, the power of growth in certain directions, and mental acts; the exercise of any one of which constitutes life. That accidental causes are capable of communicating one or more of these properties to tissues that did not previously possess them, is certain. Thus, exposure to light may influence the movement of the pigment molecules in the skin of the frog and other animals, so that it at once becomes dark or light.[1] The entrance of a spermatozoid into the ovum—that is a vibratile fibre, much like a vibrio, pushing a molecule before it—excites those changes in the yolk which produce an embryo. An unimpregnated uterus is not contractile, but if impregnated, its fibres have a vital property communicated to them for a purpose, and they expel the fœtus. I think no one can doubt that an aggregation of molecules produces a vibrio, which, at first motionless, has contractility communicated to it, and thereby lives.

M. Onimus has shown, by careful experiment, that if the clear serum from a blister be placed in a bag of gold-beater's skin, and put below the skin of a rabbit, so as to be exposed to warmth and endosmotic currents, bodies like pus or the colourless cells of the blood, called by Robin, Leucocytes, are formed. He has even filtered the fluid, and shown that they arise spontaneously by molecular deposition, so long as the fibrin it contains is not coagulated. Hence, he says, all substances which produce coagulation—such as alcohol, corrosive sublimate, iodine, to which we may add carbolic acid—prevent such purulent formations. Their production however, he points out, is identical with that of molecular growth. He further ascertained, that when white of egg or fresh blood was placed in a glass vaccination tube, or vessel deprived of air, they did not putrefy; but if placed in a sac of gold-beater's skin, although not exposed to fresh air, they shortly putrefied, and contained numerous vibrios. In the first case, he says there is no exchange of air between the organic substances and the fluid, under which circumstances no organic forms are produced. He therefore maintains that vibrios and fungi in fluids are not the result of atmospheric germs, but of conditions necessary to the putrefaction of organic fluids.[1]

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[1] See Professor Lister on the Cutaneous Pigmentary System of the Frog.—*Phil. Trans.* 1858.

These conclusions are confirmed by the numerous facts which have long been recognised in pathology,[2] and indicate that it is to a knowledge of these conditions that science must now apply itself. So long as the origin of infusoria was ascribed to atmospheric germs, we rested contented, and all inquiry was stopped. I shall be satisfied if, in supporting another doctrine this evening, I have stimulated even one inquiring mind to investigate the truth.

Gentlemen, I see by the public papers that a great legal authority in this city has been lecturing on the contradictions which exist among scientific men, and the want of proper evidence leading to their conclusions. He complains, also, that he everywhere sees a tendency to place new scientific views in opposition to religious beliefs.[3] I shall not reply to his good-natured criticisms by dwelling on the glorious uncertainties of the law, but shortly point out, in conclusion, that the theory I have placed before you this evening should recommend itself to the Biblical student. Man, he learns, was created from the dust of the earth, and he may well ask,—why should such molecular matter have been chosen for the purpose? How many histogenetic and histolytic transformations it underwent before the form was perfected, he is not told; but being made, it is distinctly said that living properties were added to it. He is nowhere taught that life invariably springs from life. On the contrary, the great doctrine everywhere impressed upon him is, that life springs from death. "Thou fool," says St Paul, "that which thou sowest is not quickened unless it die." In quoting this remarkable passage, I do not seek from it scientific evidence at all, but I believe it contains a deep philosophy consistent with all kinds of development—religious, moral, political, and certainly physiological. There is a manifest inconsistency in supposing that the same kind Providence which alone can create life, should have done so once only in ages past. The idea that a seed can retain vitality for thousands of years has hitherto been a stumbling-block to physiologists. What it really possesses is a certain arrangement of dried molecular matter, which, when exposed to peculiar conditions, enables it to exhibit the vital property of growth. The truth seems to be, that new life springs from the molecular death of pre-existing tissues and organisms. The vibratile and germinal molecules concerned in the process are not new productions, but old ones, and these, submitted to conditions which it is the office of the physiologist to investigate, undergo a rejuvenescence, and enter into and constitute the living substance of the new being. This theory, I venture to think, is not only consistent with all known facts, but unites in one grand conclusion the physiological science of the present day, the intellectual speculations of ancient Greece, and the direct teachings of our sacred records.

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[1] Journal de l'Anatomie et de la Physiologie, 1867, p. 47.

[2] See the author's Clinical Lectures on the Principles and Practice of Medicine, 4th edition, more especially pp. 164, 182, 233, 689.

[3] Address by the Right Honourable James Moncreiff, Dean of Faculty, etc., 14th January 1868.

ARTICLE VI.—*Successful Operation of Lithotomy for the Third Time on a Patient Eighty Years of Age.* By Dr SCOTT, Surgeon to the Dumfries and Galloway Royal Infirmary.

WILLIAM R., labourer, Annan, was admitted into the Infirmary on 3d October 1867, with undoubted symptoms of calculus of the bladder. Patient stated that sixteen years ago, and again four years ago, he had lithotomy performed by Professor Syme in the Edinburgh Royal Infirmary, where he remained four weeks and eleven weeks respectively.

On the present occasion, as formerly, the lateral operation was performed, and on introducing the finger into the bladder, a fibrous polypus, of the size of a cherry, was discovered, which was easily twisted and removed by forceps. Three calculi were removed without any difficulty, two of which were of the size of marbles, and the third about the size and shape of a walnut. The patient made an excellent recovery, and left the house six weeks from the date of the operation, suffering, however, from incontinence of urine, which he had done since the second operation.

The object in publishing the foregoing case is to show that the operation may be undertaken at an extreme old age, with prospects of a favourable issue.

Part Second.

REVIEWS.

On the Pathology and Treatment of Albuminuria. By WILLIAM H. DICKINSON, M.D. Cantab., Fellow of the Royal College of Physicians, Assistant Physician to St George's Hospital, and to the Hospital for Sick Children. London: Longmans, Green, & Co.: 1868. Pp. 255.

THE great outlines and the chief details of the renal affection now known as "Bright's disease" were so clearly and correctly laid down by the discoverer, and by Dr Christison in his valuable treatise, that succeeding writers have done little else than confirm their original statements. But while this is true of the disease as a whole, certain subordinate questions of great importance, which were only touched upon by Dr Bright, have exercised the ingenuity of pathologists and physicians up to the present time, and have not yet received a final solution. These questions are sufficiently definite. In Bright's disease the kidneys are found sometimes large, sometimes small and wasted; sometimes soft, sometimes hard; smooth or granular, fatty or waxy. Are all these various conditions only different stages of the same lesion, or are they the result of several distinct morbid processes? And if the morbid processes are several and distinct, are there correspondingly distinct groups of symptoms during life, which may serve to distinguish the particular morbid process in the kidney, and even the particular stage of that process? In a word, are there clinically and pathologically distinct species of Bright's disease? For many years the attempt has been made to answer these questions affirmatively. But authors are neither agreed as to the number and nature of the distinct morbid processes, nor as to the exact clinical history to be attached to each. The work before us by Dr Dickinson is a contribution towards the solution of these difficulties. By Albuminuria, in his title-page, the author merely means "Bright's disease." We confess we prefer the latter name, which is less equivocal, and is understood and used in other countries as well as our own. But that is a matter of taste.

Dr Dickinson has already been favourably known by various researches, and particularly on the subject of renal disease. His present volume, within a moderate compass, treats comprehensively of the morbid anatomy, the clinical history, and the therapeutics of what he considers as the different forms of Bright's disease. In regard to all of these he contributes important observations; and on the pathology of the less known waxy or amyloid disease of the kidney his views are remarkably novel and original. It is obvious throughout that the work has been the result of much independent research and personal observation. The descriptions are good, and the author's views are clearly and succinctly, though somewhat dogmatically stated. Well executed chromo-lithographs and engravings representing diseased kidneys, and microscopic sections, tube-casts, etc., form very instructive illustrations. Altogether it is an excellent book, well worthy the attention of our readers; and we purpose, therefore, to notice some of the chief points of interest which it presents.

We may pass briefly over the sketch of the normal anatomy of the kidney, with which the author commences. He points out that certain conditions of the renal epithelium usually esteemed morbid are met with in perfectly healthy kidneys. So far as morbid processes are concerned, three structures require to be specially distinguished in the kidney: the tubules, the fibrous tissue uniting these (intertubular or insterstitial tissue or matrix), and the bloodvessels. We notice that no allusion is made to Henle's tubules, which are still an obscure point in renal anatomy.

Why the urine becomes albuminous in certain affections of the kidney is discussed in Chapter II. The explanations are given which are usually considered satisfactory. The exact relation of the albuminous discharge to the morbid processes in the kidney, however, is not, we think, so conclusively ascertained as is often assumed. There are certain difficulties to which the author does not refer. Why, for example, does the amount of albumen vary so frequently, and why is it sometimes absent? The various kinds of "tube-casts" are briefly described. It is obvious that particular tube-casts are not distinctive of the different forms of Bright's disease, as was at one time supposed.

In his classification of renal diseases, Dr Dickinson divides them into three groups, in correspondence with the principal anatomical constituents of the kidney already mentioned: the tubules, intertubular tissue, and bloodvessels. Thus we have, 1*st*, *Tubal nephritis*, in which, the epithelium being inflamed, the tubules become distended by their morbid contents, and give rise to the "large, smooth kidney" of Bright. 2*d*, *Granular degeneration*, produced by disease of the intertubular fibrous tissue. Here the morbid process is analogous to cirrhosis of the liver: an atrophied hard kidney, with a nodulated surface, the contracted granular kidney of Bright, is the result. These two forms are local, and either inflammatory, or closely allied to inflammation, in their nature. The last form is, 3*d*, The *waxy*, *amyloid*, or, as the author calls it, *depurative disease* of the kidney. This is not a local but a constitutional affection, essentially chronic in character; in fact, a peculiar cachectic degeneration common to the kidneys, with certain other glandular organs. In the kidney, it takes its origin in the bloodvessels, but afterwards extends to the other textures. This three-fold distribution of kidney diseases recalls the similar division by Virchow, who names them respectively the parenchymatous, the interstitial, and the amyloid forms of Bright's disease. The special object of Dr Dickinson is to contribute to the more accurate delineation and discrimination, clinically as well as pathologically, of these different forms.

It will be noticed that Dr Dickinson does not enumerate the fatty degeneration of the kidney as a special form of Bright's disease. Pathological vicissitudes are curious. Early in the study of renal disease by the aid of the microscope, the fatty degeneration or stearosis of the kidney was regarded as *par excellence* the specific lesion in Bright's disease. Mr Simon was, we think, the first to show that fatty epithelium might exist in the kidney without producing any symptoms of Bright's disease,—indeed, without producing any symptoms at all. His well-known observation that most of the London cats, although in apparent health, had the renal epithelium fattily degenerated, was a curious illustration of the innocuity of simple stearosis. Accordingly, although some authors may still retain the obsolete views, nearly all modern pathologists are agreed to reject simple fatty degeneration of the renal epithelium as a form of Bright's disease. The apparently fatty kidney which really belongs to Bright's disease is, as Dr Gairdner long ago demonstrated, that in which the inflammatory exudation contains a large amount of oil granules. This so-called fatty kidney is really the result of the chronic "parenchymatous," or, as our author calls it, tubal nephritis, and it is the inflammatory and not the fatty process which is pathologically significant.

The account which Dr Dickinson gives of *tubal nephritis*, both acute and chronic, requires few remarks. The causes producing this form he considers to act as renal irritants stimulating the kidney to excess: cold indirectly by checking perspiration; the poison of scarlet fever, and other contagious diseases more directly through their materies morbi; turpentine, alcohol, etc. The chronic forms he traces to cold and intemperance chiefly. In these cases especially the epithelium tends to become fatty, a change not observed, he says, in scarlatinal dropsy, which is alleged to be more curable in consequence. Tubal nephritis, acute or chronic, is, it is well known, usually distinguished by the urine being scanty in amount and highly albuminous, while at the same time dropsy is almost invariably present, and is often excessive. Such is the general rule, and the exceptions are rare. A singular case is recorded by Dr Dickinson, where the albumen was absent from the urine.

The acute and the less severe cases of the chronic tubal nephritis are often susceptible of cure under very simple measures. The author recommends especially the diuretic treatment—especially the mildest diuretics,—of which water, he says, is "the most valuable," digitalis, the potash salts, etc. It is well known that the diuretic treatment, which met with much criticism and opposition on theoretical grounds, has, under the influence of Dr Christison's recommendation, been generally the staple treatment in Edinburgh practice. But we need not insist on this point, as Dr Dickinson formerly brought his method of treatment under the notice of the readers of this Journal (Sept. 1864).

Dr Dickinson agrees with those pathologists who maintain that the large smooth kidney which results from chronic tubal nephritis remains large and smooth to the last, and does not pass into the small contracted kidney, which some have considered a later stage of the disease. He believes that this atrophic change does not occur unless the tubular disease becomes complicated by the waxy degeneration.

The chapters on *granular degeneration*, the second form of renal disease, contain many observations of interest. Here the kidney becomes small and indurated, and presents a rough nodulated (granular) exterior. Dr Johnson, who traces the origin of renal affections exclusively to the epithelium, ascribed this form to desquamation of the tubules and their consequent collapse. Dr Dickinson maintains the opposite view, that the intertubular fibrous tissue is exclusively the seat of disease, the epithelium being primarily unaffected. The contraction of the diseased fibrous tissue, as in chronic inflammation or in cirrhosis of the liver, compresses the tubules and obliterates them, especially at certain parts, producing the atrophy, induration, and irregular surface of the granular kidney. The process which leads to the granular kidney is essentially chronic, insidious in its commencement, latent and protracted in its course, differing remarkably in these respects from the usual history of the "tubal nephritis." While tubal nephritis occurs mostly in individuals under thirty, granular degeneration is a disease of middle life, almost unknown before twenty, common at forty, most often terminating fatally at fifty.

The causes of granular kidney disease are particularly obscure. Dr Dickinson considers that the climate of the temperate zone exerts an undoubted predisposing influence. The other conditions to which he ascribes the affection are, 1*st*, Renal congestion, especially from valvular disease of the heart and from pregnancy; 2*d*, The gouty habit; 3*d*, A general tendency to fibroid degeneration, as shown by the concurrence of similar changes in the liver, lungs, and other organs. The association with gout, first pointed out by Dr Todd, and confirmed by Dr Garrod, is a favourite subject with writers in England, where special opportunities exist for observing it. In gouty kidneys, urate of soda was found by Garrod in the intertubular fibrous tissue, and Dr Dickinson confirms the observation. Further, Garrod has shown that the gouty condition is one of the results of the absorption of lead; and Dr Dickinson has made the interesting observation, that painters, plumbers, compositors, and others, who work in lead, exhibit this form of renal disease in a very large proportion. He asserts "that, of painters, at least one-half eventually die of granular degeneration of the kidneys, while, as compared to other external circumstances, the influence of lead is a more fertile source of this disease than any other with which we are acquainted." (P. 124). In Edinburgh our experience is different. Among the class of persons from which our hospital patients are derived, there is, of course, a large number of workers in lead; yet gout is a disease extremely rare in our hospital wards. It will be curious to observe whether the absorption of lead can produce granular kidney independently of gout. But when we consider on the one hand that Dr Dickinson disputes the influence of alcohol in the production of granular kidney, as stated by Christison, and, on the other hand, ascribes so much to gout and lead, we cannot help thinking that he forgets that other causes must concur with the absorption of lead in the production of gout, and that possibly intemperance or the effects of intemperance may have more influence in the causation of granular kidney than he is at present inclined to admit.

Dr Dickinson further remarks, "It appears that where the gouty condition has resulted from alcoholic liquors, it tends chiefly to the joints, when from lead, to the kidneys. The rich man enjoys long life with gout at his extremities; the artisan perishes, perhaps, before his limbs are touched, from changes of the same nature in the kidneys." This is a curious observation; it is very certain, however, that the gouty rich are not exempt from kidney disease. In regard to gout, also, although in Scotland the opportunities of examining it where an autopsy can be procured are comparatively few, yet in several instances the waxy degeneration has been observed occurring both in cases of pure gout and of so-called rheumatic gout or chronic rheumatism. We have not, so far as we remember, found this complication noticed by English observers. The term "gouty kidney," which is assumed as synonymous with granular kidney, is very apt to make observers neglect the examination of the organ in regard to the occurrence of the waxy change.

The characteristic symptoms assigned to distinguish the granular kidney from tubal nephritis are, that the urine is copious and of low specific gravity. At first the albumen is absent, afterwards it increases, but is usually in small amount. Dropsy is often entirely absent; vomiting, dyspepsia, bronchitis, hypertrophy of the heart, are frequent, etc.

We have not space, nor is it necessary here, to enter on a discussion of the differences of opinion which are entertained in regard to these two forms of kidney disease. In maintaining that the large smooth kidney (tubal nephritis) is entirely distinct from the small granular one, and that the latter is not merely a later stage of the former, Dr Dickinson supports the view which has long been held by English in opposition to most Continental pathologists. With this our own experience agrees. It has appeared to us, however, that mixed forms of disease, in which both tubular and intertubular structures are involved, are much more common than Dr Dickinson seems to admit. It is rare to find an atrophic Bright's kidney without marked evidence of tubular change of long standing, and a large proportion of the apparently new fibrous tissue can be shown to be really due to the empty, contracted, and nearly obliterated tubules.

In the third and last form of Bright's disease, the kidney, in common with other organs, especially the liver and spleen, becomes the seat of a peculiar infiltration or degeneration, which has been called the *lardaceous*,[1] *baconny* (*speckig* of the Germans), *waxy* or *amyloid* disease. This is a constitutional affection of a grave and cachectic kind. Although of frequent occurrence, and often a cause of death, it is singular that this morbid change has been slowly, and is still imperfectly, recognised,—so that, although the waxy lesion is known to pathologists, many physicians are still ignorant of it; and in France and America it has scarcely yet been studied. The progress of knowledge in regard to it well illustrates the advantage of investigating organs the study of which is commonly neglected. In the spleen the waxy deposit occurs sometimes as a diffuse infiltration of the whole organ, which, in consequence, becomes enlarged and indurated, sometimes in the more conspicuous form of small circumscribed deposits which appear as round transparent nodules scattered in the opaque red pulp of the gland, like the grains of boiled sago in soup, to use the comparison of Christensen. It is particularly from the study of the latter form —the sago-spleen—that advances of our information in regard to the waxy degeneration have at various times proceeded. This peculiar condition of the spleen was first pointed put by Hodgkin in connexion with disease of the lymphatic glands (*Med. Chir. Trans.* 1832). He did not name it, and gave no opinion as to its nature. Carswell and Bright, who also observed it, thought the deposits were of a malignant kind. But the subject was soon forgotten. It was Rokitansky who first (1842) discovered the pathological relations of the diffuse forms of waxy degeneration, to which he gave the name of the "lardaceous or baconny (speckig)" disease, ascribing it to an infiltration of the affected organs by a "crude albuminous blastema," by which they were hypertrophied and hardened. He described particularly the lardaceous liver, spleen (not noticing apparently the sago-spleen), and kidneys, noted that these various organs were often simultaneously affected, and traced the origin of the disease to cachectic or dyscrasic states of the system, induced by the advanced stages of rickets, syphilis, ague, mercurial, tuberculous, and other exhausting diseases. The kidney affection he particularly set down as a special form of Bright's disease.

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[1] It may be proper here to notice that the lardaceous (speckig) disease of Rokitanksy is the true equivalent of what we here call "waxy." Rokitansky and Budd apply the term "waxy" to an entirely different affection, viz., a species of *fatty* liver. This confusion of terms has misled many writers, not only at the early periods of the inquiry, but even down till quite recently; *e.g.*, a writer so distinguished and well informed as Dr Pavy (see Guy's Hosp. Rep. 1864, p. 316.)

Unfortunately, these important observations of the great Viennese pathologist did not attract the attention they deserved. The subject again fell into nearly complete oblivion. Even in Germany, Frerichs (1851), although he describes a cachectic form of Bright's disease, takes no notice of the lardaceous or waxy kidney.[1] Budd, however, in England, had recognised Rokitansky's description in what he calls the scrofulous enlargement of the liver (1845). In Edinburgh, it appears that the epithet "waxy" had been applied to the liver in the time of Home, and gradually crept into use in regard to this and other organs. The term was employed merely as a descriptive one, to signify the pale and indurated conditions of organs which gave them a resemblance to bees-wax; just as the term spongy is used to express the loose and open condition of textures. No distinct pathological meaning was involved in the term; no special waxy degeneration was thought of then any more than a special spongy degeneration is thought of now. The first minute examination of the waxy structure was, we believe, made in 1845[2] by Dr Bennett, who, on subjecting to the microscope a specimen of enlarged liver, of waxy consistence and appearance, found the cells indistinct and destitute of oil globules. He came to the conclusion that the lesion was an advanced stage of the fatty degeneration, in which the oily matter having been absorbed, the empty shrunken cell walls were left behind, and aggregated together, while their nucleus had disappeared. This was all that was known of the minute structure of the waxy liver, and it does not appear that Dr Bennett had described any other waxy organs. In 1848, Dr Gairdner made the important step of recognising a waxy kidney as a distinct form of Bright's disease, although he did not indentify it with Rokitansky's lardaceous kidney, with which he does not seem to have been acquainted. Dr Gairdner's description is remarkable; he considered the waxy lesion in the kidney to consist in "the spontaneous obliteration of the capillaries of the kidney, with subsequent thickening and varicose dilatation of tubuli throughout the organ." This description is most interesting as the first indication (so far as we know) of the affection of the bloodvessels in the waxy disease. It is valuable, also, as separating the waxy from the fatty kidney, with which it has generally been confounded.

Such was the state of knowledge on the subject till 1850. About that date, the normal anatomy of the spleen, long neglected, was investigated by Ecker, Kolliker, Sanders, and others. In 1851–2, the pathological anatomy of the spleen began to be studied in the pathological theatre of the Edinburgh Royal Infirmary. The sago-spleen soon attracted attention, and Dr Sanders (to whom Dr

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[1] In 1853, Wedl, who ascribed the lardaceous spleen and liver to infiltration with colloid matter, could find nothing abnormal in the lardaceous kidney.—*Path. Histology.*

[2] His description was not published till April 1853 in his Clinical Lectures.

Hodgkin had shown his preparations in Guy's Hospital), on careful examination, found it to consist, not of a cancerous deposit, but of a peculiar transparent alteration of the Malpighian bodies. The pathological nature and relations of this transparent degeneration were at this time unknown, and its relation to the waxy induration of organs was not suspected; it was thought to be a condition peculiar to the spleen. In the summer of 1852, Dr Gairdner, then pathologist to the Edinburgh Infirmary, having to leave for the Continent on account of his health, his duties were temporarily undertaken by Dr Sanders. In the autopsy of a patient who died of phthisis pulmonalis, and who had a sago-spleen, Dr Sanders found, on examining the so-called waxy liver, the same transparent material with which he had previously been familiar in the spleen. This at once pointed to an identity in the nature of the lesions, and indicated a new and peculiar kind of morbid change. Dr Sanders communicated these observations to Dr Gairdner on his return, who at once recognised the translucent highly refracting appearance of the diseased structures,—an appearance he had noticed in the liver without further investigating it, and had described as "horny" in his written accounts of the dissections. In the waxy kidney also he remembered to have seen a similar appearance. It was now resolved to investigate this translucent alteration in these different organs,—Sanders taking more especially the spleen, Gairdner, assisted by Drummond and Kirk, the liver and kidney. On searching the literature, Dr Sanders soon identified the so-called waxy conditions of the organs with the lardaceous disease of Rokitansky, and he found also a remarkably accurate description of the sago-spleen, given by Dr Handfield Jones before the Pathological Society of London, in January 1852. Dr Jones noticed the identity of the lesion with Rokitansky's lardaceous disease of the spleen, liver, and kidney. In consequence of finding the lesion so far previously investigated, Dr Sanders contented himself with bringing the subject of the transparent Malpighian bodies before the notice of the Edinburgh Physiological Society in November 1852 (*Monthly Med. Jour.*, Jan. 1853, p. 84), in a short communication in which he referred to Rokitansky and Handfield Jones' researches, describing at the same time a curious specimen of hæmorrhage into the splenic Malpighian bodies, which were the seat of the transparent change. The next result of the researches in Edinburgh was the discovery that in the kidney the transparent or waxy degeneration commenced in the minute arteries and capillaries. This discovery was made by Dr Kirk, and was demonstrated to the Physiological Society at its meeting in June 1853 (*Monthly Med. Jour.*, Sept. 1853, p. 277). This is such an important fact in the history of this investigation, and presented such important bearings on the subject of the waxy degeneration generally, that it deserves special attention. The same fact was either simultaneously or subsequently discovered in Germany; but it is worth recording that the credit of either first, or at least of independently observing it is due to the distinguished physician and naturalist who accompanied Dr Livingstone on the Zambesi expedition, and who is now the British Consul at Zanzibar. After pursuing investigations for more than a year, the results of the Edinburgh inquiries were summed up in the descriptions of the waxy liver, kidneys, and spleen, given by Drs Gairdner and Sanders, in the meeting of the Edinburgh Physiological Society in December 1853 (*Edin. Monthly Med. Jour.*, Feb. 1854, pp. 185–8). At the same time Dr Bennett renounced his opinion that the waxy was an ultimate stage of the fatty degeneration, having satisfied himself that the alteration of the cells was primary, not secondary to a fatty deposit.

Although the Edinburgh observers were not agreed on all points (*l. c.*, Nov. 1854, p. 470, 471), they had recognised and studied a distinct pathological condition at that time generally unknown. In regard to the waxy degeneration in general, the conclusions stated by Dr Sanders are as follows. After describing briefly the very peculiar physical properties and the chemical reactions of the translucent substance, he affirms that "it is the formation of this translucent matter which constitutes the waxy degeneration;" and again, that "the waxy conditions of the liver and kidney depend on the same change as that which takes place in the spleen." The terms "waxy degeneration" and "waxy material" were thus substituted for the terms "transparent condition" and "translucent material," which had been previously used to describe the microscopical appearances of the diseased organs. The chemistry of the subject received also an important contribution in the analyses of the waxy livers by Drummond, at Gairdner's instigation, published in this Journal (May 1854, p. 393). Gairdner had also, previous to 1852, ascertained the high specific gravity of the indurated waxy organs, and the same observer also ascertained that the waxy change might take place in cancerous and other morbid growths. Since that period, the waxy organs have been regularly described and demonstrated in the pathological theatre of the Edinburgh Royal Infirmary.

About the same time, or following immediately after the Edinburgh investigations, and independently of them, the same subject attracted attention in Germany, and some of the German researches are widely known. In 1853, Meckel[1] published the results of what appears to have been a most elaborate chemical investigation of the lardaceous (speckig) substance. The chemical results were somewhat confused and indefinite, but Meckel's paper is memorable as first describing those peculiar colorations with iodine and sulphuric acid which have since materially assisted and promoted inquiry. Meckel seems to have been well acquainted with the lardaceous affection of the liver, spleen, and kidney, and to have detected the lardaceous condition of the minute bloodvessels. He concluded from his experiments that the essential ingredient of the morbid deposit was cholesterin, and he accordingly named the lesion the "cholesterin disease" instead of the lardaceous (speckig). This

[1] Annal. d. Charité Krankenhauses zu Berlin, ii. Heft, 1853.

notion was soon after refuted by Virchow's elaborate chemical experiments, and has never been adopted.

Till 1853, Virchow[1] was uncertain about the nature and doubtful even of the existence of a special lardaceous or waxy degeneration. He had been acquainted with the sago-spleen described by Christensen of Copenhagen, in 1844, and investigated in its microscopic structure by Shrant of Amsterdam in 1851; but he had at first believed the transparent material to be colloid, and the affection allied to cancer. Subsequently he regarded the deposit as abuminous, but without obtaining any definite notion of its pathological nature or relations. In the autumn of 1853, after discovering the cellulose reaction (purple or blue coloration with iodine and sulphuric acid) on the corpora amylacea of the brain, he sought for the same reaction in other tissues, and fortunately found it in the transparent corpuscles of the sago-spleen, which thus again became the starting point for fresh researches. Virchow failed to obtain the reaction in the lardaceous liver, which he, therefore, did not at first recognise to be of the same nature with the sago-spleen. Gradually, however, as his observations extended, he seems to have gone over much the same ground as H. Jones and the Edinburgh observers had previously done, only that he employed the cellulose reaction as the testing character instead of the translucency, etc., which had guided the British observers. He thus gradually recognised that the indurated spleen was another form of the same lesion as that producing the sago-spleen; and the same with the lardaceous liver and kidney. In short, he found that the cellulose reaction could be produced, with various degrees of distinctness, wherever the lardaceous condition existed. Curiously, Virchow preferred the epithet waxy to the term lardaceous (speckig) of the Vienna school, without being at first aware that the Edinburgh observers had used it in that sense. By further investigation, Virchow ascertained the existence of the same lesion in other parts, in certain cartilages, in enlarged lymphatic glands, and Dr Jochmann having detected the cellulose reaction in the mucous membrane of the stomach, Virchow traced it in some cases throughout the whole intestinal tract, where the minute bloodvessels of the mucous membrane are almost exclusively affected. By means of the coloration with iodine or iodine and sulphuric acid, these researches were greatly facilitated, and Virchow's discoveries became widely known throughout the Continent. On chemical grounds Virchow was led to conclude that the peculiar characteristic matter of the waxy infiltration was of a vegetable nature, like cellulose or starch. At one time he spoke of the process as the "verholzung" or lignification of the tissues; but afterwards he proposed the term amyloid, which has since come into very general use, although his theory of the vegetable or starch-like nature of the morbid product has been disproved. In Edinburgh the term waxy has kept its ground.

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[1] Virchow's Archiv, bd. vi. 1854, p. 268 and 416; and bd. viii. 1855, 140 and 364; and Canstatt's Jahresbericht, 1853, i. 22, and ii. 33; 1854, ii. 20.

In England, the work of Johnson (1852) shows that the waxy kidney was then little known, and its nature not accurately understood. In Paget's Classical Lectures on Pathology (1853), no mention is made of a waxy degeneration. Wilks, writing on Bright's disease, in 1853 (Guy's Hosp. Rep.), was unable to recognise completely Rokitansky's lardaceous kidney; he thought it was a fibrous metamorphosis of the kidney, analogous to cirrhosis of the liver. In 1854, Dr Parkes introduced the subject in an analytical review, giving an account of the German and Scotch observations on the lardaceous or waxy degeneration; and Dr Arlidge discussed Meckel's views on the cholesterin disease (Brit. and For. Med. Chir. Rev., Oct.); but in spite of the observations of Handfield Jones, to which we previously alluded, the lardaceous or waxy disease was not practically studied in England till about 1856. In that year Dr Wilks (Guy's Hosp. Rep.), published his "Cases of Lardaceous Disease, and some allied affections," recommencing the study of the morbid process in the hospital, where Hodgkin had first directed attention to it some twenty-four years before, and when his original specimens were retained in the museum,—but where, nevertheless, the subject had curiously enough been, in the interval, neglected and forgotten. Dr Wilks' paper is valuable as a collection of cases illustrating the causation of the affection, and as an independent investigation of the pathological anatomy, containing excellent descriptions of the diseased organs. Dr Wilks makes no allusion to previous observers; he disputes the iodine and sulphuric acid reaction; and he partly mixes up with the lardaceous certain affections which do not belong to the same class. He considers the lardaceous change as one analogous to the cancerous or tuberculous. Dr Harris, in 1861, discussed the amyloid disease in his Cambridge thesis. In 1861–64, Dr Grainger Stewart published in this Journal, and in the Brit. and For. Med.-Chir. Review, certain papers on the pathology of the waxy renal disease, directing attention specially to the increased flow of urine in these cases, which he explained by ascribing to the waxy bloodvessels an increased permeability to fluids. In 1864, Dr Pavy, together with a brief historical sketch of the subject, gave the results of his chemical investigation of the so-called amyloid substance. He confirmed the statements previously made by Schmidt, Friedreich, and Kekule, that the amyloid matter is not, as Virchow supposed, chemically allied to cellulose or starch, but is a nitrogenous body, which Pavy thinks is more nearly allied to chitine than to anything else.

This historical digression has extended to so much greater length than we anticipated, that we must content ourselves with this incomplete sketch, and refer to recent treatises on pathology and on diseases of the kidney,[1] for further information in regard to the waxy affections. We shall only add, that in France and in America the waxy disease was, till lately, unknown; and the authors of these countries have not as yet contributed any remarkable additions to our knowledge, as they will no doubt do when the subject has engaged their attention.

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[1] Virchow, Vogel, Goodfellow, Rosenstein, etc.

Dr Dickinson has introduced certain peculiar views in regard to the nature and origin of the waxy degeneration, which, if confirmed, will throw a new light upon the affection. According to him, the waxy or amyloid substance is nothing else than fibrin deprived of a proportion of its alkalies (dealkalized fibrin). This proposition, which he puts forward very confidently, he founds chiefly on the following grounds:—1*st*, That his chemical analyses have shown, on an average, that waxy livers contain about one quarter less alkaline salts than healthy ones; 2*d*, That the red coloration with iodine, which distinguishes the waxy material, is destroyed by the addition of alkalies, and that fibrin dissolved in very dilute hydrochloric acid (6 parts of acid to 10,000 water), and recovered by evaporation, is transformed into a gelatinous material, which exhibits the same red coloration with iodine as the waxy substance; 3*d*, That the most frequent cause of the waxy degeneration is protracted suppuration in some part of the body. Pus being an albuminous fluid, rich in alkaline matter, its copious discharge deprives the blood of albumen and alkalies. The dealkalized fibrin, now in excess, is alleged to be deposited in the smallest arteries (the parts first affected in waxy organs), because the current of the blood is there sufficiently retarded to allow of coagulation or deposition. It is to indicate this relation of the waxy material as originating from prolonged suppuration, that the author has chosen the name "*depurative*"—a most unfortunate misnomer, as the word does not express what he intends, and is suggestive of depuration or cleansing, which is not at all meant. Pathological names are seldom happy; they are as abnormal as the conditions they indicate.

On a subject where so little has as yet been ascertained, any theory resting on such evidence as we have just stated demands careful and impartial consideration, and will, we trust, lead to further researches in confirmation or correction of it. The chemical analyses made of entire organs are always difficult and uncertain, and the numbers given in the tables vary much in the different examples. But this and the other chemical evidence can only be truly judged of by the repetition of similar experiments. It is known that the red coloration by iodine is destroyed by alkalies; but when, in the author's experiments, he obtained a translucent gelatinous material showing the red staining with iodine, did he find it to present likewise the other physical and chemical properties of the waxy material? Did it show the same resistance to decomposition, for example, which is a peculiar feature of the waxy substance? We find that Dr Dickinson does not admit that the purple or blue reaction with iodine and sulphuric acid ever takes place in waxy structures. This alleged change, which is the true cellulose or so-called amyloid reaction, he says, "appears to be an error of observation, partly arising from the fact, that when sulphuric acid acts upon iodide of potassium—a salt generally present in the test solutions used—a precipitation of iodine takes place which blackens the tissue." But it is rather a strong assumption that the German and Edinburgh observers (*Month. Med. Jour.* 1854, April, p. 381; November, p. 468), who obtained this reaction, could not distinguish between purple and blue colours, and the blackening of a tissue by the precipitation of iodine. That there is a difficulty in obtaining this reaction, and that different tints (purple or blue) occur in different specimens, all have admitted, and it still remains an interesting inquiry why these variations occur. But that is not a reason for denying the existence of the reaction altogether.

With reference to the production of the waxy lesion by long-continued suppuration, or the prolonged discharge of albumen, we may remark of the latter cause, that, as it exists in all chronic cases of albuminuria, waxy disease ought, if the theory were correct, to be invariably present in them. But, that exhausting suppuration is an usual antecedent of waxy lesion is universally admitted, especially in connexion with diseased bone or with tubercular processes. The frequent association with diseased bone led Virchow and Wilks to presume some special connexion, which has not however been made out. In the Edinburgh hospital, where the medical preponderate largely over the surgical autopsies, the relation to advanced tubercular phthisis has been more specially observed. But, on the other hand, as the author admits, waxy disease often occurs when neither copious suppuration nor prolonged albuminuria have preceded it, sometimes, though rarely, when no very definite antecedent morbid condition can be discovered. These exceptions may perhaps be susceptible of explanation. The waxy has hitherto been only known as a secondary or deuteropathic, not as a primary or protopathic affection. What we would contend for, however, is, that the waxy condition should be sought for not only under circumstances where its presence may be suspected—as in chronic suppuration—but also in other diseases. Why not even in acute diseases also, although hitherto it has not been detected in them? No secure generalization can be made till more is known of the antecedent conditions of the disease. If perseveringly sought for, the waxy lesion is even now found to be a very common lesion, either by itself, or complicating other forms, especially of renal disease; at least it is so frequent in Edinburgh and in Germany, that there is little reason to suppose the case to be different elsewhere.

One remark more. We do not understand, on Dr Dickinson's theory, why the fibrin of the blood should not be first and chiefly dealkalized in these cases; yet waxy fibrin is not known in the blood. Why, too, is the lesion confined usually to certain organs? And if the arteries are first affected because the retardation of the circulation allows deposition to take place, why does the waxy material first affect the middle coat of the arteries and not rather line their interior? or why does not the precipitation take place in the capillaries where the current of blood is greatly slower than in the minute arteries?

Such are some of the difficulties which suggest themselves with reference to the author's theory,—the importance of which, however, we fully recognise.

We observe that Dr Dickinson affirms that the waxy or amyloid material becomes converted into fibroid tissue (p. 169), and a similar statement was made by Wilks. This assertion is important, as the waxy material has usually been characterized by its homogeneous appearance and apparent incapacity for organization. We call the attention of observers therefore to this point, which deserves further investigation.

In connexion with his opinion of the fibroid transformation of the waxy material, we find that the author regards the small smooth kidney as the third stage of the waxy degeneration. If by this is meant, that this small waxy kidney had been previously a large waxy kidney, we are not prepared to agree. In the examples of small smooth kidney we have seen, not only was the waxy lesion confined to the arteries and Malpighian bodies, while the other structures were unaffected by it, but usually the concomitant waxy lesions of the liver and spleen were in an early stage, or had not taken place. All this points to an early rather than a late or final stage of the waxy lesion.

But we must now draw our remarks to a close, leaving untouched the clinical history of waxy disease as given by the author, and the instructive chapter on the effects of climate in relation to renal disease. In pointing out some of the more original opinions and observations of the author, we have expressed our opinion of their value, and we have had much pleasure in recognising the justice and correctness of many of his views and the general merits of his work. We recommend it as a valuable contribution to a most important subject.

Stone in the Bladder; with Special Reference to its Prevention, Early Symptoms, and Treatment by Lithotrity. By WALTER J. COULSON, F.R.C.S., Surgeon to St Peter's Hospital for Stone and other Diseases of the Genito-Urinary Organs, and to the Lock Hospital. Pp. 124. London: John Churchill & Sons: 1868.

THE unenviable notoriety attained by St Peter's Hospital for stone must still be freshly remembered. It is the *reductio ad absurdum* of specialism, a division of labour most unnecessary, unprofitable, and insulting; insulting not only to the common sense of the profession, by resting on the theory that to treat stone and stone alone is the guarantee for its being well treated, but still more insulting to the surgeons of the great hospitals, by implying that, for the sake of the patients, any such hospital was needed.

To the students of the great hospitals, such institutions must be peculiarly irritating, for as in theory St Peter's is to attract to itself all cases of stone, and St Mark's all cases of fistula, it is obvious for the student there would remain either ignorance of these important branches of surgery, or weary pilgrimages to the special shrine. Fortunately, as yet the theory has not been reduced to practice. The great schools still have a few stones to crush and cut, and the surgeon of St Peter's is not absolutely overwhelmed with work; so in the meantime we have this book as an earnest of the future, and a proof of the value of special hospitals.

It consists, *first*, of three lectures delivered to the students attending St Peter's Hospital, the stonecutters and crushers of the future, conveying to them in simple language the original observations and ripe experience of their master; and *second*, of a treatise on prevention of stone. Nobly disinterested! to teach the disciples of St Peter's to *prevent* the only disease which, on the special hospital theory, they know how to *cure*.

Judged by the high standard which the special opportunities and high aims of St Peter's Hospital court, we fear this work may disappoint and dishearten. We have failed to find much that is new in it, nor is the absence of novelty made up for by any special grace in the method or arrangement of what is old. The first lecture gives a fair idea of the early symptoms of stone, certainly not above the comprehension of a youthful student, illustrated by a few cases drawn from the author's practice. These cases detail not only the symptoms, but give interesting glances into the social status, and professional qualifications of the patients. The following sentence gives at once a specimen of the author's style, and a glowing picture of the future of St Peter's:—

> "If we are to attain success in this hospital—if we are not to be mere self-seekers, but genuine workers in this domain of surgery—we must not be content to acquire mere operative dexterity, though this is of great importance but we must avail ourselves of all the means at our disposal for improving and sharpening our powers of diagnosis. By doing this, we shall be rewarded by a success in the treatment of these affections unprecedented in this or any country."—P. 16.

The operation of sounding is then described and the gratifying success attained by the author is thus detailed,—

> "I could illustrate this part of my lecture by numerous examples, both in hospital and private practice, where over and over again I have been enabled to detect the presence of calculi, after the ablest surgeons had failed to detect them by the sound."—P. 19.

The second lecture is on the preparatory treatment and the operation itself. Mr W. Coulson prefers M. Civiale's method, but fails in his description thereof even to approach either in clearness or exactness the well-known one given by Sir Henry Thompson in his standard work. One excellent recommendation is given, that the operation should be practised on the dead subject, for which he hopes ere long to give facilities at St Peter's.

The third lecture on after-treatment and complications contains nothing very remarkable or original.

The remainder of the book under the head of "Prevention," includes forty pages of elementary description of the various forms of calculi and the condition of the urine in each, and twenty pages of "Treatment" under the three following heads :—

> "1. To diminish the amount of the normal constituent in the urine, or to remove, so to speak, the diathesis.
>
> "2. To prevent the calculous material from being precipitated.
>
> "3. To keep the urine in a dilute state, by abundant drink, and to flush the urinary system from time to time."—P. 107.

Under these heads the advice given is sensible, if not original. being very much of the quality of an answer to a surgical examination paper by a good senior student.

We are quite certain that five years hence Mr Walter Coulson will agree with us in thinking, that, as a rule, it is a mistake to rush into print with a first course of lectures, and that the honour of being an author is not sufficient recompense for associating one's name with an ephemeral compilation.

The Diseases of the Prostate: their Pathology and Treatment: comprising the Jacksonian Prize Essay for the Year 1860. By Sir HENRY THOMPSON, F.R.C.S.; Surgeon-Extraordinary to H.M. the King of the Belgians; Surgeon and Professor of Clinical Surgery to University College Hospital, etc., etc. Third Edition. London: John Churchill & Sons: 1868.

WE welcome with much pleasure the appearance of this most excellent standard work in its third edition. Characterized, like the other works of its distinguished author, by exactness of observation, thorough system, and full acquaintance with the researches of others, this new edition gives more practical hints, as the product of a more extended experience. These have been added without adding to the size of the work, and yet without missing out anything really essential to its completeness.

On Spinal Weakness and Spinal Curvatures: their early Recognition and Treatment. By W. J. LITTLE, M.D.

DR LITTLE is so well known and experienced an authority on the treatment of deformities, that any work on this subject from his pen deserves the careful attention of the profession. The treatise under consideration is divided into two principal portions, the first of which treats of "Spinal Weakness," the second of "Spinal Curvatures."

Dr Little considers spinal weakness under the following four forms:—Simple uncomplicated spinal weakness; Spinal irritation; Hysterical spinal weakness; Tabes dorsalis. In our opinion, the distinction of the first three of these as special diseases is unnecessary.

We are inclined to class the whole of these as *symptoms* merely of a particular state of the constitution, or of some general affection.

To consider these various conditions as distinct diseases, is, we think, the very way to direct the attention of both surgeon and patient too exclusively to the *spine*, and thus to give rise to that form of quackery which Dr Little in his preface so rightly condemns. Dr Little's observations on these *symptoms*—as we prefer to call them—and their treatment, are most interesting and valuable.

In the second portion of this work, the various forms of spinal curvature are carefully described. The first and second chapters treat of slight and severe "Rotatory or Lateral Curvature." Dr Little's remarks on the symptoms, diagnosis, and treatment of these affections are very valuable; and we sincerely wish that all cases of distortion of the spine were treated on the sound, common-sense, and honest principles advocated by Dr Little.

Chapter III. is "On Curvature of the Spine after Pleurisy." According to Dr Little's experience, this curvature is a simple lateral distortion of the vertebræ without any of the rotatory displacement which occurs in ordinary lateral spinal curvature. Dr Little also remarks, that "A pathognomonic difference between marked ordinary lateral curvature and that which ensues upon lung-compression from adherent and shrunken ribs is, that in the former there exists a prominence of the submammary region on the side opposite to that on which the dorsal bulge is situated, whilst in the latter no such mammary prominence exists."

The remaining chapters are devoted to a description of certain special lateral curvatures, of posterior and anterior curvatures, and of angular distortion of the spine. Dr Little's observations on the treatment of the latter common deformity are most judicious.

The book concludes with a short appendix, containing ten woodcuts, which, though small, are clear and distinct, and admirably illustrate some of the facts to which Dr Little has directed attention. A careful study of Dr Little's book will well repay the professional reader, for we feel sure, that unless a surgeon has a correct knowledge of such principles of treatment as are advocated in its pages, he cannot hope to treat successfully and honestly cases of curvature of the spine.

The Essentials of Bandaging; including the Management of Fractures and Dislocations; with Directions for Using other Surgical Apparatus. With 110 Wood Engravings, pp. 166 foolscap 8vo. By BERKELEY HILL, M.B. Lond., F.R.C.S., Instructor in Bandaging, etc., University College; Assistant-Surgeon, University College Hospital, etc. London: James Walton: 1867.

THIS very neat little book is another added to the list of small manuals on the subject of bandaging and surgical appliances, which have been called forth by the greater attention paid by examining boards to these subjects. It possesses the advantages of brevity and clearness, and will doubtless be of use to the students for whom it is written. To be small, such a work must be selective, and thus a candid reviewer must condone much omission, yet many of the methods of treatment recommended bear the stamp of local, possibly merely of individual adoption. Possibly approved and taught by the author, and even perhaps acceptable to the surgical authorities of University College, many of the methods of treatment described should not be trusted to by a student, even for a pass at other examination boards. The gutta-percha gauntlets, the splints advised for the treatment of fracture of the lower end of the radius, and especially the apparatus figured for fracture of the collar-bone, are examples, among others, of plans of treatment by no means generally adopted.

The application of a starch-bandage is well described, but surely a young student should not be told that "each turn of the roller should be made as tightly as possible," when, in the preceding page, he has been told "that some surgeons apply the starch-bandage immediately after the fracture has occurred."

The chapter on dislocations and club-foot contains nothing either novel or remarkable. The time-honoured diagrams of the position of the pulleys for dislocations of the hip are given in detail; surely they have nearly passed out of use in these days of chloroform and manipulation.

The chapter on miscellanea contains a very curiously arranged bundle of directions on minor surgery. The instructions as to the administration of chloroform are, according to the views held in this school, exceedingly dangerous; and the woodcut is as misleading as it is absurd in appearance. A mild-looking operator, with a large bag-pipe slung over his shoulder, is with one hand holding an apparatus to the nose of an old gentleman, who is *sitting bolt-upright* on a chair, and with the other feeling his pulse! The little book is neatly got up, and profusely illustrated.

The First Step in Chemistry. By ROBERT GALLOWAY, F.C.S., etc. Fourth Edition. London: Churchill: 1868.

First Principles of Modern Chemistry. By U. J. KAY-SHUTTLEWORTH. London: Churchill: 1868.

Chemical Notes for the Lecture-Room. By THOMAS WOOD, Ph.D., F.C.S. Second Edition. London: Longmans: 1868.

NOW that Chemistry is gradually taking its place as one of the subjects taught in schools, the selection of a good elementary treatise or school-book becomes a matter of very great importance; the more so, because the demand, which now unquestionably exists, has produced a large supply. A good school-book of chemistry should be accurate, simple and clear in its statements, should deal more with facts than with theories, and especially with such facts as can be readily proved by easy experiments or by reference to familiar instances. These facts should be arranged so that their relations to one another can be at once seen and understood; the simplest and most easily explained phenomena should be treated of first, and a gradual ascent made to the more difficult examples. Generalizations should be introduced sparingly, and never without a full supply of illustrative facts.

Professor Galloway's "First Step" fulfils these conditions extremely well—it is indeed an admirable school-book, and every chapter proves the author's skill and experience in the difficult art of teaching beginners. One great advantage, which is peculiarly important at the present time when science is being introduced into the curriculum of our schools faster than schoolmasters can be produced who can teach it, is that this book can be used by teachers who have only a very slight knowledge of chemistry. It would be easy to make objections to Professor Galloway's method, but much more difficult to suggest improvements. For the purposes of teaching, it is in all respects superior to the systematic arrangement adopted by most writers of text-books, while it has the peculiarity, which many will no doubt consider a disadvantage, of being altogether unsuitable for *cramming.*

Considerable changes and improvements have been made in this edition, and seven new chapters added, giving an account of the atomicity of elements and radicals and of the systems of classification based on atomicity. In these chapters the new atomic notation is used, and a link is thus supplied connecting this book with the author's "Second Step."

Mr Kay-Shuttleworth's little book is a careful resumé of the chemistry of the non-metallic elements. It is written in a clear and simple style, and gives an accurate and intelligible account of modern chemical theory. Dr Crum Brown's graphic, and Dr Frankland's symbolic notations, are employed throughout in such a way as to fix the attention of the student on the important idea of chemical structure.

The third book on our list is one of those which have been called into existence by the modern system of examinations. There are two ways in which a student may be "prepared for an examination" on chemistry. One, is to study the subject, and make himself acquainted with it; the other, is to buy Dr Wood's book, and "get it by heart." Strange as it may appear, many prefer the second course, which must be nearly as difficult, and certainly far less profitable than the first. That the book is intended to be "crammed" is candidly acknowledged in the preface. The author says, "Many years' experience in teaching has convinced me that the average boy can only be provided with a very limited amount of producible information on any subject for an examination. A small book, therefore, with which he may become so familiar as to be able to refer to it with ease and rapidity, and which he can almost get by heart, is the thing required, and the present edition is offered as such."

That the producible information, with which the author would provide the average boy, is not only very limited in amount, but very inferior in quality, will be seen from the following extracts:—"The chemical term *salt* is applicable to nearly every compound. The compounds of any metal with the metalloids, when spoken of collectively, are commonly called *salts; e.g.*, salts of potassium, salts of iron, gold, etc."—(p. 27),—and this is all that we have tc represent a definition of the term salt! "Gases and vapours are *elastic fluids*, and are mostly invisible; *e.g.*, air and steam.—Liquids are *non-elastic fluids*, and all liquids are visible; *e.g.*, water.—*Elasticity* (from ἐλαύνω, I drive). A property which some bodies possess of springing back to recover an original form on the removal of the force which had compressed or otherwise altered this."—(Pp. 29, 30.) It is scarcely necessary to remark that *both* gases and liquids possess perfect elasticity of *volume*, while neither of them possess the elasticity of *form* alluded to in the definition above.

At p. 91, phosphoric, phosphorous, and hypophosphorous acids are all said to be tribasic, and their formulæ given thus,—$P_2O_5 + 3H_2O = 2(H_3PO_4)$; $P_2O_3 + 3H_2O = 2(H_3PO_3)$; $P_2O + 3H_2O = 2(H_3PO_2)$, whereas it is known to every tyro that phosphorous acid is dibasic, hypophosphorous acid monobasic, and that P_2O has no existence. We might easily multiply such examples, but these may suffice to show how boys are prepared for examination. It is to be hoped that examiners have some means of distinguishing real knowledge, however small in amount, from such "producible information" as this.

Part Third.

MEETINGS OF SOCIETIES.

PROCEEDINGS OF THE EDINBURGH OBSTETRICAL SOCIETY.

SESSION XXVII.—MEETING IV.

8th January 1868.—Dr Keiller, and afterwards Dr Sidey, in the Chair.

Dr M'Callum, Edinburgh, was elected an Ordinary Fellow, and Dr Turner, Keith, a Corresponding Fellow of the Society.

I. *Dr Andrew Inglis* exhibited a fœtus between the third and fourth months, in which there were three malformations, viz.,—a spina bifida, an abdominal tumour, and a deformity of the hand.

Dr Keiller thought it a beautiful specimen of a complex malformation, and was of opinion that the abdominal tumour, if cut into, would be found to contain the liver and intestines.

II. *Dr Keiller* showed a fleshy mass which had been expelled from the uterus, and sent to him by Dr Thomson of Dalkeith. On opening into it no trace of a fœtus could be discovered. The mass was ragged at one end, and he thought it possible that the fœtus might have escaped through the opening.

III. *Dr James Young* showed the scarificator he was in the habit of using, and which, for convenience, he had got fitted into an ordinary penknife.

IV. *Dr Keiller* exhibited a very compact and useful pocket armamentarium, which, although of very small size, contained a scarificator, a vaccinator, a scalpel, and an abscess bistoury.

V. THREE CASES OF HAND PRESENTATION. BY MR FURLEY.

A few days ago I said to a medical gentleman that it was my intention to read a paper to this Society on a case of hand presentation that I had had, and he asked me if there was any peculiarity in the case that I should do so. I answered that I did not know that there was. My object, therefore, in bringing it before you is just to elicit from the members of this Society, by their criticisms, questions, and conversation, whether there be any peculiarity, and to learn, from the observations of others, hints that may be of service to me in conducting any subsequent case that may come under my care. Indeed, I think that the aims of this Society are as much subserved by bringing forward cases of ordinary occurrence as by cases of great rarity, which come only under the notice of one practitioner in a hundred. However, the remark of my friend had partly the effect of inducing me to add two other cases of the same kind; and these three constitute the whole of that class of cases that have happened in my nine years' practice.

The frequency of hand presentations in British practice, according to the statistics collected by Dr Churchill, is 1 in 252½ cases; the proportion of mothers lost, 1 in 9; and of children, nearly one-half. Thus, although such cases are by no means rare, the loss of mothers and children is of very serious importance.

On the morning of the 18th November 1867, a midwife, who says she has been in practice, or, as she phrased it, "on the road" for twenty-two years, called upon me. She said she had a case of foot presentation, and wished me to go and assist her. I willingly complied, saying she need be under no apprehension if it was only a footling case. On arriving at the house and examining the woman, I found that the foot unmistakably "shook hands" with me—that the membranes had ruptured thirteen hours before my examination—that the pains were coming on every five minutes or thereabouts—and that the woman was in great mental distress about it being a cross birth. Taking the midwife aside, I told her she had mistaken the nature of the case—that it was a hand that was presenting, not a foot—and that the child would have to be turned. She asked me if we could do it "oursels." Certainly, I said, if she could give the chloroform. "Hoots, ay; I can haud the cloot to her mouth." But thinking that if that was all her knowledge of the administration of chloroform her assistance would be anything but valuable, I said it would be better to get another doctor. Having obtained the assistance of my friend Dr Cairns, we proceeded to the operation of turning.

Her condition at the time was as follows, it being kept in mind that it was her third confinement:—membranes ruptured; right hand presenting just within the external opening of the vagina; uterus quiescent; fœtal and placental bruits heard on auscultation; bowels cleaned out by a dose of castor oil, and bladder emptied by catheter.

Having put her in a proper position, with the buttocks projecting slightly over the edge of the bed, the chloroform was administered. I then passed my left hand up to the os uteri by side of the presenting arm. There I found the funis hanging loosely through the os. Pushing it up, I gradually succeeded in inserting the hand into the uterus, and, slowly pressing upwards, I came to the left knee. I hooked it by the flexure and gently tried to bring it down, but could not move it; and, using greater force, I tried again, still I could not get it to move. I then pressed up to get hold of the foot. I could reach it, but could not get a proper hold of it. I then tried to get a hold of the other foot, which I readily did. On pulling it down it gradually came lower, but it slipped and receded twice. The third time I succeeded in bringing it down, and got hold of it with a cloth.

Here I thought the greatest difficulty had been overcome. Not so, however, for, on using traction, I could not manage to bring it any further down. I pulled with such force that I thought I should injure the limb, or pull it off, an accident which I believe has happened before now. What to do next, after being thus tantalizingly baffled, was the question. There was nothing for it but to pass my hand, this time by the side of the leg, and try to get hold of the other foot; for I thought that if I got hold of both legs, I should hardly succeed in pulling them both off, without bringing down the body. Passing my hand up, then, I managed to get hold of the second foot, and, after great exertion, I got it as far as the mouth of the womb. By this time the muscles of my arm and hand were considerably cramped, and I had to take both hands to get the second foot as far down as the first. This having been accomplished, it did not require great traction to bring down the body. The passage of the head now became the only difficulty. The face was towards the sacrum, the cord had ceased to beat, and there was no time to lose; but I am sure a force equal to 70 or 80 lbs. was exerted before the head was born. The child was dead. The usual means to restore suspended animation were resorted to, including the "Marshall Hall" method, which, by the way, does not appear to succeed nearly so well in cases of this kind as the alternate immersion in hot and cold water. Having lost the child, then, all our efforts were directed to save the mother. The placenta was *in utero*. I pulled gently at the cord, but the placenta did not come down, although it appeared to be detached. I then put on the binder, for I have found that the application of the binder has a wonderful effect in detaching and almost expelling the placenta. In this case, however, it did not succeed, and on inserting my hand into the uterus, I found that the placenta was quite detached, but that the uterus had contracted firmly on a portion of the membranes, and this was the cause of the retention of the placenta. Taking hold of the membranes near the point where they were grasped, I gradually managed to release them, and then the placenta came away at once. I then inserted my hand again into the uterus to ascertain its condition, but everything being favourable, we left the patient in charge of the veteran midwife. Next day when I called there were symptoms of pneumonia; and, looking upon these as the forerunner, or rather a part of puerperal fever, I treated it by calomel and opium. After a week had elapsed the patient began decidedly to improve, and eventually made a good recovery.

Having described this case more minutely perhaps than necessary, I shall briefly relate the other two.

One evening, about six o'clock, I was requested to go to a poor woman in the West Port to take away the afterbirth. I was told that the child had been born at one o'clock, but that the afterbirth had not come away. On proceeding to the place I found a rather extraordinary state of matters, for, on examination, I discovered that, although the child was born at one o'clock, the afterbirth which I was requested to remove was nothing less than another child. But what was worse, it was a hand presentation. Well, thought I, there can be little difficulty in this case. The uterus has been distended by the passage of a child already,—I can introduce my hand quite readily, and can feel the foot. True, the uterus is contracted on the child, but it will relax when I begin to turn. There is nothing to do, in fact, but, in the language of the writers of Memoranda of Midwifery, "insert the hand into the uterus, seize hold of the opposite foot, and turn the child." Very easily said, but sometimes very difficult to do. And so I found it. Having got hold of the foot, I tried to turn, but turn the child would not. I sent for Dr Cairns, but his success only equalled my own. So we gave it up, and sent for Sir James Simpson. On his arrival the patient was immediately chloroformed, and Sir James, apparently with the greatest ease, turned and extracted the child. Now, why was it so easy to him, and so difficult, nay, impracticable to Dr Cairns and myself. There was only one thing which enabled him to accomplish what we had failed to do, and that was simply, but very emphatically,—*he chloroformed the patient.* That is the secret of his success and our failure. The lesson has not been lost upon me, although I cannot accuse myself in the matter at all, for the operation seemed so easy that it was almost unnecessary to give chloroform. And I cannot help thinking that if Sir James had examined the patient before giving the chloroform, he would have tried to operate at once, as well as I did. The sequel of the case is this:—The patient took peritonitis, and died in a few days, and the twins died one after the other, apparently from want of care.

The third case presents scarcely any peculiarity or difficulty. It was the patient's second confinement. The presentation was detected before the membranes were ruptured, and the child was readily turned, notwithstanding that during the operation the uterine contractions were very powerful. One thing I observed in this case was, that the uterus seemed to contract in the middle, producing the feeling that the hand was in an hour-glass. Both mother and child survived.

Remarks.—I shall confine myself to three. *First*, That the term "contraction of the uterus" does not properly describe the condition. *Second*, That there is no danger, but sometimes advantage, to be derived from introducing the hand into the uterus after delivery. *Third*, That the placenta is frequently retained by the uterus contracting upon a portion of the membranes. But as I shall bring before you, at a future time, papers on these subjects, I shall in the meantime be content if discussion be provoked.

Dr Cochrane did not think it was necessary to introduce the hand except in cases of hæmorrhage, and he always found that he could ascertain by external manipulation the condition of the uterus.

Dr Pattison said he seldom found it necessary to introduce the hand except in cases of hæmorrhage, and agreed with the remarks made by Dr Cochrane. He thought that in many cases in which the placenta did not come away easily, its expulsion might be greatly assisted by atmospheric pressure.

Dr Cairns agreed with the observations already made as to the impropriety of introducing the hand on all occasions. He alluded to a common cause of retention of the placenta,—one which was not mentioned in books,—the result of the membranes, or a portion of the membranes, being firmly grasped by the contracted fundus of the uterus. He had met with this condition in several cases, in which the membranes were so firmly retained that he had almost to peel them off from the womb itself.

Dr James Young concurred with the views expressed by Dr Cairns, and said that just last night he had had two cases in which, while the placenta was quite free, the membranes were retained by the uterus. He did not see the use of applying a binder to aid in the expulsion of the afterbirth, and thought it was much better to grasp the womb externally. He usually found that whenever he had occasion to introduce the hand into the uterus, such interference was followed by febrile symptoms.

Dr Keiller said that there was one point touched on in Mr Furley's paper which was certainly a novelty, viz., the proposal to substitute the term collapsing for contraction of the womb. He did not quite understand what was meant by collapsing of the uterus, for, according to his notion, the womb always contracted after expelling its contents, in a case of natural labour. It might feel soft to the touch, but that was caused by the bundling up of its fibres. Examination of the uterus soon after delivery showed it to be contracted, diminished in size, and greatly increased in thickness. He was therefore not disposed to adopt Mr Furley's views on the subject. In cases such as those detailed in the paper, he thought the best practice was, after getting one foot down, to assist the rotation of the child with the other hand. In twin cases it is not unusual to have to turn the second child, even although there is no abnormality. He remembered a case in the West Port where the second child presented by the shoulder, and turning was resorted to; but he regretted it afterwards, for it might have been delivered by extraction. A little traction in such cases, after the parts have been distended by the passage of the first child. suffices to bring the second child into the world. As to the placenta, Dr Keiller was of opinion that the putting on of a binder was of no use, and the less we touched the placenta the better. He agreed with Dr Pattison in thinking that many cases of so-called adherent placenta were merely cases in which the afterbirth was retained on account of the air not being allowed an entrance. In such cases the fingers should be passed through the body of the placenta, so as to admit the air, and then they could be used as a hook to pull it down. He stated that he was not in the habit of introducing the hand except in cases of hæmorrhage. M'Intosh used to say that every time the hand was introduced into the uterus the woman was in great danger; but Dr Figg had conferred a great boon upon the profession by showing the fallacy of this view. Dr Keiller held that the hand, if clean, is as innocuous as an instrument; but he believes that, at the present time, there is too great a tendency to hasten the third stage of labour.

Dr Ritchie stated that, in cases in which ergot had been used, the uterus was apt to contract upon the placenta, and so cause retention; but steady traction on the cord usually sufficed to bring it away.

Dr Cappie said he never had found it necessary to introduce the hand into the womb even for hæmorrhage.

Mr Furley, in reply, said that his reason for introducing the hand after delivery was, that in that way he got a better idea of the state of the uterus than by external examination. The term contraction, he thought, was not sufficiently expressive, as the uterus was, so to speak, folded in upon itself, crumpled up as it were.

SESSION XXVII.—MEETING V.

22*d January* 1868.—Dr Burn, *President*, in the Chair.

Mr Charles Edward Smith, late surgeon of the Hull whaler "Diana," addressed the Society on—

THE MIDWIFERY AND DISEASES OF THE ESQUIMAUX; ALSO THE EFFECTS OF PROTRACTED EXPOSURE TO COLD AND PRIVATIONS UPON THE HUMAN SYSTEM.

The following is a condensed report of the address:—

An Esquimaux woman, when in labour, is confined in a small hut carefully provided for the occasion, the carcase of some animal, generally a dog, is enclosed with her, and she is left to overcome her difficulties alone and unaided. On no account will they permit the attendance of a second person. I noticed several of these private maternity hospitals, each containing a woman and newly-born infant; and in one of unusually small dimensions I discovered a bitch with a litter of puppies. I was extremely anxious to observe the phenomena of labour amongst these children of nature; but my services as an accoucheur were firmly declined. I could not learn that Esquimaux women ever died in childbirth, or experienced much difficulty in their confinements,—indeed the remarkably broad and deep pelvis would indicate facility in labour. Their breasts, also, are unusually developed, though not to the extent observable amongst the Hottentots and Bushmen tribes of Southern Africa.

As a race the Esquimaux are robust and well developed in proportion to their height, and remarkably healthy in spite of their filthy habits and total neglect of the ordinary rules of health. The children are vigorous, but not numerous, although many of the men have two wives, and conjugal fidelity is altogether ignored. The married women are distinguished by a variety of devices tattooed upon their faces and shoulders. I only noticed one aged man in the Pond's Bay Settlement of Esquimaux, which may be accounted for by their custom of deserting, or even putting to death, such of their number as are incapacitated by accident, disease, or the infirmities of age from assisting in the hunt, and thus becoming a burden to the community. Children that are deformed or of sickly habit are similarly disposed of; whilst the extremities of cold and hunger, and the accidents incidental to their adventurous and uncertain mode of life, are continually thinning their numbers.

The diseases most prevalent amongst this singular people are pleuro-pneumonia, conjunctivitis, staphyloma, hæmorrhages, and syphilis. The latter, introduced amongst them by European sailors, and extended by the promiscuous intercourse that prevails between the sexes, seems to have been fatal to whole communities. Although subsisting solely upon animal food, I never saw or heard of a case of scurvy amongst the Esquimaux; whilst the rapidity with which they recover from the effects of severe wounds and injuries indicates that their general systems, and more especially the blood, must be in an uncommonly healthy condition.

The "Diana" was unfortunately beset in lat. 71° 30′ N., upon 26th August 1866, and compelled to winter in the country with only two months' provisions on board, with very little fuel, and totally unprovided with the extra clothing, antiscorbutics, and other requisites necessary for the preservation of life and health during the rigours of an Arctic winter. Upon 3d September the ship's company of 50 souls went upon short allowance,—3 lb. bread, $3\frac{1}{2}$ lb. salt meat, and $\frac{1}{2}$ lb. flour per man per week, the meat being afterwards reduced to 2 lb. per week, including bone. The cold was intense, and aggravated by the want of firing, the staves of a single cask having to provide two days' fuel, and cook food for all hands. The berths were lined with solid ice of a thickness of two inches, caused by the congelation of the moisture in the breath of the sleeping wretches, whilst, as if to add to their miseries, the ship was so severely crushed upon the 3d of December that she was only kept afloat for four months by constant toiling at the pumps both day and night. All on board suffered dreadfully from cold, hunger, exposure, and privations. The captain, an old man of sixty-four, was the first to succumb, dying of general dropsy and uræmia the day following Christmas. One of the crew died from protracted exposure to intense cold when in a weakened and exhausted state. Scurvy made its appearance in January: we were totally unprovided with preserved meats, vegetables, lime-juice, and other antiscorbutics, so that the disease made rapid progress. The only treatment that could be adopted under such hopeless circumstances was to endeavour to alleviate the various symptoms as they occurred, and to encourage the men to exert themselves, take active exercise, and, above all, keep up a stout heart, and repel the gloom and despondency incidental to this insidious disease. Men of strumous constitutions were the first to be affected with scurvy,—amongst the Shetlanders it was especially fatal. Men of medium size, dark complexions, active habits and cheerful temperament, enjoyed the greatest immunity, although, by the time the ship arrived in Shetland (2d April), every man on board was either dead, dying, or more or less seriously affected with scurvy, the decks piled with corpses, the berths crowded with disabled and dying men, the few that were still able to be on deck, and work and pump the ship, presenting such a spectacle of famine, misery, and disease, as sent tears of pity rolling down rough cheeks little accustomed to such display of emotion.

The bi-tartarate of potash seemed to have little or no influence upon scurvy but there was not sufficient of this so-called remedy on board to give it a fair trial. A very small quantity of bouilli-soup given to our first cases had a most decided and beneficial effect. The deaths of the scurvy patients occurred from general dropsy, exhaustion, from sudden shock (such as a fall) followed by fatal syncope, dysentery, etc.; whilst the blood was so completely disorganized that all superficial wounds and abrasions became ulcerations. Hæmorrhages from the mouth, nose, stomach, lungs, bowel, kidneys, were universal amongst us, with mortifications from frost-bite, etc., etc. We lost four men from scurvy after arriving in Shetland (making a total of thirteen deaths); but the survivors of that poor ship's company recovered rapidly upon change of diet. I found a diet of milk and raw eggs of the greatest service with our worst cases, many of whom were unable to swallow a more solid nourishment. Fresh animal and vegetable food, especially potatoes, with small doses of the preparations of iron, rapidly completed their recovery.

It may be worthy of remark that, during the winter, two of the ship's dogs exhibited decided symptoms of hydrophobia, and were destroyed; whilst a canary and common linnet survived the intense cold of the fireless cabin, and were brought home alive.

On the motion of Dr Pattison, the President, in the name of the Society, thanked Mr Smith for his most interesting remarks.

DUNDEE MEDICAL SOCIETY.

At the monthly meeting of the Dundee Medical Society, held on Wednesday, 5th February—Dr Crockatt in the chair—Dr Park, Broughty Ferry, read some notes of two cases of placenta prævia which he had recently met with.

Dr J. W. Miller read the following notes of a case of general emphysema, which had occurred in hospital.

Jane C., æt. 9, was admitted to the Hospital on 17th January, about 11 A.M. The first glance at her gave the impression that it was a case of extreme general anasarca; but on touching the surface the distention of the cellular tissue was found to be due to emphysema, which existed to an extreme degree over the whole trunk, neck, and face. The whole surface was tender to the touch. Respiration was considerably embarrassed, and slightly accelerated, being 28 per minute. The lips, however, were of good colour. She was unable to swallow. Auscultation was not very satisfactory, owing partly to the pain occasioned by the pressure of the stethoscope, but chiefly to the crackling of the air in the cellular tissue. The respiratory murmur, however, seemed natural, and equal in both lungs; and the cardiac sounds healthy. The pulse was 96, of good strength. She was quite conscious.

Her history of her illness was not very clear; but after some trouble it was made out that she had been in her usual good health that morning, previous to going to the mill at 6 o'clock A.M., and that, after being there about half an hour, she had begun to swell; the swelling had rapidly increased, and she had been carried home. The first symptom observed by her was pain about the throat or neck. We could not succeed in discovering any cause for the supervention of the emphysema for some time; but at last she told us that she had nearly choked on a piece of crust before going to work; and she afterwards stated that she did not get relief until the crust was ejected after a prolonged and violent fit of coughing. In this violent fit of coughing we must find the cause of the emphysema, its probable mode of origin being that, during the violent expiratory efforts, some of the pulmonary air-cells were ruptured, the air then finding its way by the cellular tissue in the lung to that of the mediastinum, and thence to the subcutaneous cellular tissue of the trunk and neck.

At 4·30 P.M., the emphysema was still increasing. The respiration was more embarrassed, being 38 per minute; pulse 132. There was great pain over the surface, much increased by pressure. A short incision was made by Dr Anderson over each pectoral muscle, and on left side of chest posteriorly, from which incisions bubbles of air continued for some time to well out. At 8 P.M., the swelling had considerably diminished, especially on the face, and she was suffering less pain. Pulse 132. She still refused to swallow. Next day at the hour of visit (about 11·30 A.M.), she was much better. The pulse was 120.

The respiration was easy. She swallowed readily. The swelling was still diminishing. She said she was unable to lie on her right side. At 8 P.M., she moaned a good deal, and was troubled by a short tickling cough, caused, she said, by pain in her throat.

From this time she gradually recovered. On the 22d, the emphysematous crackling was still present over right lower jaw, along great cervical vessels of both sides, along the course of the vessels of left arm, and all over the trunk; but there was now little or no swelling visible to the eye.

Part Fourth.

PERISCOPE.

SURGERY.

ACTION OF CRYSTALLIZED SULPHATE OF SODA ON STAINS OF THE CORNEA.

BY M. D. DE LUCA OF NAPLES.

UNDER ordinary means I have found during my long practice the impossibility of getting rid of many stains and opacities of the cornea, produced in various ways, often even the result of remedies applied to the eyes. I have also observed that laudanum and the tinctures, as well as astringent solutions (such as tannin), produced by their action on the eyes stains which persist under all ordinary treatment. It is known, indeed, that these alcoholic and astringent substances coagulate the albumen of the tissues, which, in this manner, lose their natural transparency, so that I have in my practice forbidden the use of all substances which can in any manner affect the transparency of the tissues of the eye.

After many unsuccessful attempts, I thought that the crystallized sulphate of soda, which has the power of maintaining the fibrine of the blood in solution, might act favourably upon the eyes, and cause the complete or partial disappearance of stains of the cornea.

In my first experiments I used a watery solution of sulphate of soda (saturated when cold), and applied several times a day in drops to the globe of the eye. After several days of treatment the patient improved, and the opacities became smaller, but it was easily seen that the action should be more prolonged to obtain really satisfactory results. At length I thought of using the sulphate of soda in a state of crystallization, and in the form of a fine powder. Placing the patient in a horizontal posture, I put a pinch of this powder on the ball of the eye, leaving it to melt in the liquid which it formed and produced in the eye. Excellent results were obtained; the stains began to disappear after some days of this treatment, and patients who could see nothing before the treatment began, were able not only to distinguish light from darkness, but even to perceive in a manner more or less distinct movements executed before them. Under this treatment the patients' experience an agreeable sensation of coolness after the application of the powder, when it begins to dissolve in the tears. It is well known that crystallized sulphate of soda lowers the temperature of water in which it is dissolved.

In a word, sulphate of soda, either in solution, or better, in a fine powder, causes total or partial opacities of the cornea to disappear; this has been demonstrated partly by the experiments made on several individuals in the Hospital for Incurables at Naples, under my care, partly in my own practice. The results of future experiments shall be submitted to the Academy, but I trust that other experimenters will make attempts in the same direction, so as to add new facts to those now mentioned.—*Gazette des Hopitaux*, No. 69, 1867.

EXCISION OF THE SPLEEN.

From a lecture by M. Kœberlé of Strasburg, the following brief abstract of his own case and of other previous cases[1] is extracted:—Mme. M. æt. 42, of strong constitution, mother of two healthy children, was suffering from an enormous tumour of the spleen; it measured 28 inches from above downwards and was very hard. The patient had become thin and feeble; her blood of a reddish-brown colour coagulated feebly, and resembled broken-down spleen texture. The other organs were apparently healthy; the appetite was voracious. After remedies had failed, the patient herself being very anxious for operative interference, Prof. Schutzenberger consented to M. Kœberlé's proposal to excise the spleen.

Operation.—An incision in the middle line occupying the middle third between the umbilicus and the xiphoid, enabled the surgeon to introduce his hand and determine the character of the adhesions between the diaphragm and the tumour. As these did not seem to contra-indicate operation, the incision was prolonged upwards and downwards to a total length of about 20 inches. The enormous vessels entering the spleen were then tied in six or seven portions, and divided between double ligatures to avoid hæmorrhage. At the upper portion of the fissure, the vessels were so short as to be tied with great difficulty, as they passed between the stomach and the spleen. The adhesions of the spleen to the diaphragm were carefully separated, however, not without difficulty, and the organ was removed. It weighed about 13 lbs., and two quarts of blood flowed out of it—a very large quantity of blood was lost during the operation, as it did not coagulate, and the adhesions bled from every pore (*en nappe*). It was impossible to tie the bleeding points, both from their depth and their number. A sponge dipped in alchohol was held against the bleeding points without avail. To restore the abdominal viscera to position that they might compress the bleeding surface, and to stitch up the wound, was all that remained to be done. The patient never regained consciousness, the breathing became more and more embarrassed, and she soon expired. The operation lasted an hour and a half; about 6½ lbs. of blood was lost.

Extirpation of the spleen, even when no morbid adhesions exist, is an operation of great danger, from the enormous size of the vessels and the peculiar condition of the blood.

Zaccarelli, in 1549, operated on a woman aged 24, for a spleen hypertrophied and painful. The patient was cured in 24 days.

In 1678, Mathiæ appears to have excised the entire spleen in a man aged 23, who had received a cut with a knife in the left side, through which the spleen protruded—cure in 23 days.

Fantini records the case of a woman, aged 30, in whom an abscess formed at the umbilicus, which being opened, the spleen protruded, and was removed with success.

Quittenbaum, in 1836, operated on a girl aged 22. She died six hours after the operation with all the symptoms of death from hæmorrhage. The spleen weighed upwards of 5 lbs.

Kuchler, in 1855, extirpated a spleen weighing 3 lbs. in a man aged 36 years. Seven double ligatures were applied to the vessels. The operation did not present any special difficulties. The patient died two hours after the operation, from the effects of the loss of nearly a pound of blood from a small branch of the splenic artery which had not been tied.

Spencer Wells, in 1865, removed a spleen weighing 5 lb. 12 oz. from a woman aged 34. The patient died on the 7th day of purulent infection and septicæmia.

Pean, in 1867, removed a spleen with success from a girl aged 20, of which last case a brief account is added.—*Gazette des Hôpitaux*, No. 137, 1867.

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[1] M. Kœberlé has omitted the cases of this operation recorded by Mr Bryant of Guy's Hospital.

SUCCESSFUL EXTIRPATION OF THE SPLEEN.

THE patient, a girl of 20, was exceedingly weak. She had suffered for two years from a tumour of the abdomen, supposed to be an ovarian cyst. The operation seemed fully indicated; the cyst was nodulated as is usual in multilocular cysts; seemed to be connected with the left ovary. There seemed no likelihood of its being connected with either kidneys or mesentery. It could hardly be even supposed to be of the spleen, as it occupied a position in the hypogastrium resembling a pregnant womb, and round its upper edge there was "sonorous percussion." Puncture, which, however, was not practised, would have given no information, as the fluid in the cyst resembled that found in ovarian cysts.

The operator made his incision in the linea alba, between the umbilicus and pubes, punctured the cyst through an adherent mesentery, and evacuated three *litres* of a yellowish brown viscid liquid. The hand introduced into the cavity of the pelvis, found that the tumour was independent of ovaries, kidneys, and mesentery. It being impossible to discover its site, the surgeon enlarged his incision to three inches past the umbilicus, and then ascertained that the tumour was developed in the tissue of a hypertrophied spleen. Rather than abandon the patient to certain death, the surgeon determined to excise the whole tumour. This was done, the vessels being secured, some by ligatures, some by the actual cautery; those in the gastro-splenic omentum being secured by four metallic ligatures, placed as far as possible from the stomach and pancreas. The ligatures were cut short and left in the abdomen. No severe symptom interfered with the cure, which was rapid, since which the patient's health has been excellent.

M. Pean draws the following conclusions from this case:—1. It shows the difficulty of diagnosing ovarian tumours. 2. The advantage of making up the mind to give the patient a chance, even in unfavourable circumstances. 3. It widens the province of surgery. (?) 4. It proves the possibility of a successful splenotomy. 5. It affords to physiologists a most interesting case to study; and, in this view, it is remarkable that the absence of the spleen has had no notable effect on the health of the patient.—*Gazette Médicale de Paris*, No. 51, 1867.

ELEPHANTIASIS CURED BY DIGITAL COMPRESSION.

DR VANZETTI of Padua reports a case, of which the following is a brief abstract:—A girl, æt. 21, stout, and of a lymphatic constitution, had elephantiasis of the right leg, the symptoms of which had gradually been getting worse for seven years. The results obtained by Carnochan emboldened M. Vanzetti to tie the femoral, but before doing so, he thought of trying what digital compression might effect. To avoid any error in the effects of digital compression being ascribed to simple rest, the patient was kept in bed for twenty days, with a bandage on the limb, without any noticeable improvement. At length, on March 13, digital compression was maintained for nine hours on the 14th, 15th, and 16th, for twelve hours each day; and on the 17th, for two hours only, as she began to complain of pain, and the part pressed was red. On examining the limb on the 18th, it had diminished more than an inch in circumference; it was softer, and little white wrinkles, like those on the abdomen of a woman recently delivered, were seen all over it. Though the treatment had to be suspended for a month on account of a feverish attack, on its being again used the limb again improved, and the limb gradually diminished; and when last seen, three years after leaving the hospital, the affected limb was found to be the smaller of the two. It is not mentioned whether obliteration of the vessel had taken place.—*Gazette des Hôpitaux*, No. 144, 1867.

ELEPHANTIASIS OF THE VULVA.

M. BOURGUET (of Aix) records an operation he performed for the removal of an enormous elephantiasis of the vulva, in a girl æt. 18. The operation seems to have been a difficult one, the tumour involving not only the whole vulva from the groin to far into the gluteal region, but also sending prolongations inwards between the rectum and vagina. One of these was attached to the bladder, and, accordingly, after removal of the tumour, it was found the bladder was extensively wounded. The patient died of peritonitis on the third day. No post-mortem examination was permitted. The tumour consisted chiefly of fibrous tissue, containing in its meshes thickened serum, and a few cysts. Even after the escape of the contents of the cysts, and also of much of the serum, it weighed 17½ pounds.—*Gazette des Hôpitaux*, No. 140, 1867.

TREATMENT OF WOUNDS.

The following brief extracts from a leading article in the *Gazette des Hôpitaux*, from the pen of Dr Victor Revillout, will be read with interest at this time, when so much is being done with the view of facilitating the union of wounds, and avoiding the dangers of suppuration. He contrasts the principles and methods of Guerin, Maisonneuve, and Lister. "Guerin wishes to prevent, as far as possible, the contact of air, and thus to place open wounds in similar conditions to subcutaneous ones. Oxygen is, in his eyes, the principal cause of suppurative inflammation; and by excluding it, he hopes more easily to obtain immediate union."—[He endeavours to obtain this end by fixing over a stump a nightcap of india-rubber, which communicates by a tube with an air-pump; the air being thus exhausted from the bag, it is retained in position by the weight of the atmosphere, excludes air from the wound, and maintains an equable compression.]

"Maisonneuve, on the other hand, is not afraid of oxygen, but *he* is afraid of dead fluids. His conclusions are:—1. That the so numerous and varied febrile accidents which complicate most wounds, and constitute the principal danger of surgical operations, are always the result of *a poisoning*. 2. That liquids exuding from the surface of wounds die when in contact with the air; that they then putrefy and become dangerous poisons. 3. That, could we prevent the liquids from putrefying on the surface of the wounds, then the greatest surgical operations, even amputations of limbs, could be performed without danger."

In M. Guerin's apparatus, Maisonneuve believes that he has discovered a simple and practical means of fulfilling his indication. By exhausting the air, it removes the fluids effused on the wound. The surfaces having been bathed in alcohol, are brought together with strips of plaster, which do not prevent the escape of serum, etc.; then the whole is covered with a bandage soaked in some aromatic and anti-putrid fluid, such as tincture of arnica, and then the caoutchouc bag is put on.

Lister, like M. Maisonneuve, claims the honour of having been the first to discover a principle. He does not pretend to have been the first to use phenic acid (carbolic acid), but to have been the first to use it with method and with a clear design. Mr Lister does not trouble himself about the "oxydation of wounds," like M. Guerin, nor into the "death of confined fluids, like M. Maisonneuve. His fundamental theory is that of germs—that is to say, he, following the opinion of M. Pasteur, attributes to microscopic organic germs floating in the air all the fermentations and decompositions of contained fluids. To destroy these he uses carbolic acid; he endeavours to render all his wounds "sub-carbolic wounds." This is another kind of occlusion, directed not against oxygen, but against the organic dust of the atmosphere.—*Gazette des Hôpitaux*, No. 141, December 1867.

REPORT ON MIDWIFERY AND DISEASES OF WOMEN.

By G. Stevenson Smith, Esq.

ECLAMPSIA: OR PUERPERAL CONVULSIONS. BY D. WARREN BRICHELL, M.D., PROFESSOR OF OBSTETRICS, NEW ORLEANS.

In a lengthened and most interesting lecture, Professor Brichell discourses on this important disease. There are two prominent characteristics, he says, which distinguish eclampsia from hysteria, chorea, tetanus, catalepsy, and the minor convulsive and spasmodic conditions we meet with in practice. These are (*first*) intellectual annihilation; and (*second*) arrest or diminution of hæmatosis. The convulsive affection, however, to which eclampsia is most closely allied, in a phenomenal point of view, is epilepsy; but the points of difference between them are these :—*first*, It is the exception to find epilepsy not characterized by what is called the "aura" or warning, whereas in eclampsia there is no premonition ; *second*, Epileptics are strictly convulsed, and these convulsions are marked by antagonistic action of flexors and extensors ; but the eclamptic patient becomes rigid, being bent backward or to one side, and her limbs are fixed in extension. Then, again, epilepsy occurs to both sexes, in all ages; while eclampsia is peculiar to the pregnant or parturient female. Again, epilepsy is distinctly disposed to become chronic and to last through years, recurring at intervals, and rarely kills speedily ; whereas eclampsia is acute—less than twenty-four hours generally deciding the issue, and a few days being the full term of duration. After pointing out the distinction between eclampsia and apoplexy, Professor Brichell, in answer to the question, What then is eclampsia? briefly answers, It is *tonic* muscular action—spasm—*with coma.* —*Southern Journal of the Medical Sciences.*

TWINS WEIGHING SEVENTEEN POUNDS.—POST-PARTUM HÆMORRHAGE FROM ATONY OF OVER-DISTENDED UTERUS — RECOVERY. BY DR HOLSTON, GANESVILLE, OHIO.

DR HOLSTON, at 6 A.M. on the morning of 31st January 1867, was called to a lady in labour, and on examination found the os fully dilated, soft, and no hæmorrhage. Vertex presenting under right pubis. The pains being feeble, ergot was administered, and subsequently, to allay mental agitation, chloroform to partial anæsthesia was given. About half-past 6 P.M. a fine big boy was born, whose weight, as was afterwards ascertained, amounted to nine pounds. The membranes of the second child protruded, and were broken,—the ergot given in increased quantity, and the flaccid abdomen of the mother supported by the hands of the nurse. The second child presented the vertex under the left pubis, and was born alive and hearty a quarter of an hour after the first. It weighed eight pounds. Now came the chief difficulty. She had taken one ounce of the tincture of ergot, and the abdomen had been constantly sustained; but the birth of the second child was followed by a terrible gush of blood, deluging bed and floor, and leaving the patient almost lifeless. Dr Holston introduced the left hand into the uterus, and placed the right on the surface of the abdomen, making strong friction and pressure. The umbilical cord had been carefully tied with and severed between two ligatures. The placenta (there was but one with two cords) was partially detached. He removed it entirely, immediately re-introducing the hand, as the hæmorrhage, though slightly checked, was still flowing at a fearful rate. Twenty grains of powdered ergot, with a table-spoonful of tincture, and five grains of sugar of lead, were poured down the throat of the almost lifeless patient. The hands inside and outside the uterus seemed to be almost in contact, so thin had the uterine and abdominal walls become from the enormous distention (seventeen pounds of children, besides water and placenta)! The uterus felt like a wet bag. The Doctor could now control the hæmorrhage by pressure on the aorta through the back of the womb, but there was not the slightest effort at contraction. He now got a hard snow-ball, the size of a small orange, and introduced it bodily into the womb. This had the desired effect; the patient heaved a deep sigh, shuddered, and the uterus slowly contracted on the hand. A bandage and a thick compress were firmly applied, and the patient rallied slowly. Opiates and alcoholic stimuli could not be borne, but iced milk was given freely, and by 10 P.M. she was considered safe. Mother and twins did well.—*Philadelphia Med. and Sur. Reporter.*

EXTIRPATION OF THE UTERUS BY MISTAKE FOR OVARIUM TUMOUR.

BY DR KRAKOWIZER.

DR KRAKOWIZER presented to the New York Pathological Society the body of a uterus, in the walls of which was embedded a large fibroma, taken from a woman 48 years of age. Dr K., when first called in, found a tumour about the size of a two-year old child's head, occupying the lower part of the abdomen.

It was smooth, elastic, painless on pressure, and moveable from side to side. On making a vaginal examination, it was found that the mass bore down behind the symphisis pubis, and had crushed the uterus backwards. The os was felt on the posterior part of the tumour. A uterine sound entered with great facility to a depth of two inches and a half behind the tumour. When the sound was grasped by one hand and the tumour held with the other, it seemed to move independently; Dr K. therefore concluded that he had a simple ovarian tumour to deal with, and that the uterus was normal and unconnected with it. The woman, on the condition of things being explained to her, expressed a wish for a radical operation. Dr Kammerer was then called in, and corroborated the former opinion, except that he thought that the tumour originated from the left side, while Dr Krakowizer thought it came from the right. Chloroform was given, and an incision was made midway between the umbilicus and the symphisis, to the extent of four inches, and the tumour was presented. At the previous examinations, never having felt any fluctuation, it was supposed that the walls of the cyst were very thick. It was found that the omentum was adherent in several spots on the surface of the tumour. On bringing the hand between the promontory and the tumour, the mass was found to be intimately connected with the cervix uteri. A trocar had previously been plunged into the mass, and as no fluid escaped, and the end of the trocar was firmly held, the operator became aware that he had to deal with a solid tumour. The left ovary could be felt between the promontory of the sacrum and the posterior aspect of the tumour, and he could also ascertain that the mass was a continuation of the cervix uteri. The question then arose as to the propriety of desisting from the operation ; but it was decided to go on. Both Fallopian tubes were ligated, and then the tumour was lifted out of the abdominal cavity. The chain of the écraseur was then passed round the neck of the tumour, and was tightened very gradually. Great hæmorrhage took place after the parts were cut through, and the stump was seized with forceps, and both uterine arteries secured ; but the veins, as well as several sinuses in the cervix, continued to pour forth blood, and were with difficulty secured. For security, a silver wire was passed round the end of the stump, and the ends brought out of the wound. The operation took two hours and a half. The wound was closed with fine silver wire sutures. After midnight peritonitis set in, and extended rapidly, and at four next morning the patient died.—*Medical Record*, Sept. 1867.

REDUCTION OF A COMPLETELY INVERTED UTERUS OF FOUR YEARS' STANDING.

BY DR WORSTER, NEW YORK.

After alluding to the rarity of inversion of the uterus, and the fact that many accoucheurs long in extensive practice have never met with a single case, Dr Worster refers to a patient under the care of Drs Emmet and Thomas of New York, in whom the inverted womb was with great difficulty returned after having been down for a period of twelve weeks. In this case a peculiar kind of manipulation was resorted to, which was also attended with success in the case about to be related. Mrs S. J. S., aged 27, of a nervous temperament, was married at 21. Became pregnant soon afterwards, and in due time was naturally delivered, but was seized with profuse hæmorrhage, which induced syncope, and nine days afterwards complete inversion of the uterus took place. It was not discovered for a week, and was then mistaken for a polypus. Hæmorrhage, more or less, had taken place during the period, *nearly four years* previous to her being placed under Dr Worster's care. Believing the return of the organ to its natural position to be impossible, she had resolved to submit to amputation of the displaced womb, and came to New York for that purpose. It was resolved, however, to try reduction by the hand, aided by the anæsthetic influence of chloroform. Before doing so, however, several superficial ulcerations and abrasions on the everted interior surface of the uterus were healed. The operation was then performed in the following manner. At 11 A.M., on 3d November, Dr Worster, assisted by his son, who gave the chloroform, introduced his left hand, well greased, into the vagina, and grasped the fundus uteri with the fingers, endeavouring as much as possible to lessen its lateral diameter. Then grasping it with the thumb and fingers, strong pressure upwards was made in the proper axis, wedging it between the sides of the os and neck, which soon began to descend and surround the inverted fundus. At the same time strong counter-pressure with the thumb and fingers of the right hand was made through the abdominal parietes from above and behind the pubis downwards into the centre of the depressed fundus and cervix uteri, which soon began to yield. The second finger of the right hand most successfully operating as a wedge, dilated the cervix, until the finger, in the centre of its circular ring, met the thumb of the left hand within the vagina; using the thumb at times to reinvert the cornu after the manner of Nœggerath, and occasionally resorting to the method with which Dr Sims, in a more recent case, had been successful in a few minutes. This manipulation was continued for thirty minutes, when it was desisted from. The patient had lost much blood, and was much exhausted by the long continuance of an amount of pressure which only those who have had a similar experience could suppose the uterus capable of enduring. The tenderness of the uterus and abdomen subsided in a few days, but the manipulation was not renewed till the 8th, when the operation was resumed. The patient lying upon her back, the limbs drawn up, and under the full influence of chloroform, Dr Worster again seized the protruded fundus with the two fingers of the left hand and thumb thrust into the right cornu, and, by long and strong compression, so diminished its size, that he could carry it up much farther than before. The bulk of the uterus within the embrace of the fingers had retained its position, and was much diminished, and could now be easily pushed beyond the grasp of the surrounding cervical ring. He also succeeded in more depressing the cervix through the abdominal walls, and effecting a wider divergence of the encircling cervical border on all sides, a space of about two inches in diameter. The pressure was continued in both directions this time for one hour and five minutes. The parts were secured against further protrusion by Barnes' dilator in the vagina. At 10 P.M. the operation was resumed, but was interrupted by hæmorrhage coming on, caused by the thumb passing through the cornu into the Fallopian tube. At this time the os was so far dilated that two fingers of the right hand could be passed downwards from behind the pubis, and through it, until they met the thumb of the left hand, carrying up the cornu from below. A menstrual period passed, and on four different days manipulation was again had recourse to. On 11th January 1867, the patient was placed in the usual position, and full anæsthesia induced. With assistance, a pressure from below upwards of about twenty pounds was maintained per vaginam, and a corresponding counter-pressure from above downwards through the abdominal cavity upon the encircling edges of the cervix. These efforts were combined and continued for *three hours*, until the resistance of both cervix and uterus was overcome, and a triumphant success resulted. The case, for the length of duration and obstinacy of the inversion, and the severity of the effort needed for the reduction, is perhaps without a parallel in obstetrical history. Patient went home almost quite well ten days after the operation.—*The American Journal of the Medical Sciences*, October 1867.

Part Fifth.

MEDICAL NEWS.

DEATH OF SIR DAVID BREWSTER.

This distinguished philosopher, the venerable Principal of the University of Edinburgh, died on 11th February, at his seat, Allerly House, near Melrose, at the age of eighty-seven.

THE LATE DR BURT, F.R.C.P.E., F.R.S.E.

Dr John Graham Macdonald Burt died at his house in Edinburgh, on the forenoon of the 9th of February. He was seized on the night of the 2d February with erysipelas in the head, of which he had previously had several severe attacks. On the 6th it seemed to have yielded, and desquamation was taking place; on the 7th a red spot appeared on the back of the hand. This was the forerunner of diffuse inflammation of the cellular tissue, which rapidly extended, accompanied by failure of the circulation and delirium.

Dr Burt was in all respects an Edinburgh physician. His grandfather and father were respectable family practitioners in Edinburgh, and enjoyed a large share of public confidence. He was educated at the High School and Edinburgh University; and while travelling on the Continent he took his degree of M.D. at the University of Giessen in 1826. Three years afterwards, on his return to this country, he obtained by examination the licence of the Royal College of Surgeons of this city. In 1843 he joined the Royal College of Physicians, and was soon made a Councillor, and rapidly rose in the estimation of the College, until, in 1863, he was elected President. He assumed this office at a time when the College had been largely divided by discussions on the Medical Act, and, by his honesty of purpose and conciliatory manners, was very instrumental in restoring and establishing the peace and harmony which now prevail.

We cannot better show the estimation in which he was held in that body, than by quoting a minute prepared, we understand, at the request of the Council, by his friend Dr Alexander Wood, and which was unanimously adopted at a meeting of the College held on the 18th February:—

"Since the College last met, only fourteen days ago, the Fellows have been called on to follow to the grave the remains of one of their office-bearers. The College is constrained, under these circumstances, to record its sense of the loss which it has sustained by the death of Dr John Graham Macdonald Burt, a Trustee and Councillor of the College, and who was so recently the occupant of the Presidential Chair.

"Sagacious in counsel, shrewd in affairs, ready in debate, decided in opinion, emphatic and sententious in the expression of his views, warmly attached to the College, and grudging no labour or trouble to serve its interests, his removal from amongst us is a loss deeply to be deplored. The suddenness of his death increases the severity of the blow; while those who know the assiduous care and constant attention which he bestowed on every detail of the erection and adornment of the New Hall, will regret that he was not spared to witness the entire completion of a building which owes so much to his tasteful superintendence.

"The loss which the whole College deplores will be more deeply felt by those who were brought into more immediate contact with him in the management of its affairs, and who were thus in a position to appreciate his many social qualities, and especially the strength and enduring character of his friendship, and the warmth of his kindly and loving heart.

"The qualities which made Dr Burt respected and beloved by all who were associated with him in public life, must have endeared him in no common degree to his immediate relatives and his family circle. To his bereaved widow and children the College would beg respectfully to tender an expression of deep sympathy, and do hereby direct that a copy of this minute be forwarded to

Mrs Burt, that she may be assured how many appreciated his many excellencies, and unite with her in deploring his loss."

Dr Burt was also a Councillor of the Royal Society of Edinburgh; and, in taking the chair at the meeting after his death, Dr Lyon Playfair, C.B., the Vice-President, alluded to the removal of his colleague in these feeling terms:—

"Dr Burt, a member of our Council, was well known as a public-spirited citizen, who has long usefully devoted himself to the development of our institutions, and who, by his genial disposition and honesty of character, endeared himself to all those who enjoyed his acquaintance."

By his mother Dr Burt was associated with an old Scottish family not unknown in the military annals of our country, and from her side of the house he seems to have imbibed that taste for arms which led him to hold the office of Surgeon to the Mid-Lothian Yeomanry corps; Surgeon to the Royal Company of Archers, Her Majesty's Body Guard for Scotland; and Surgeon to the Edinburgh Artillery Volunteers. He was also Surgeon to his late Majesty in Scotland. He early succeeded his father as Consulting Physician to the North British Insurance Office,—an appointment which he held to the day of his death, and in which he proved himself not only the adviser but the friend of the Directors of that prosperous Institution.

Dr Burt's eminent social qualities led his company to be much sought after, and deprived him of that leisure which might have made him a contributor to the literature of the profession. Early in life, however, he showed that love of art and good taste for which he was afterwards conspicuous, by the publication of some excellent drawings of Surgical Anatomy. He also contributed one or two papers to this Journal.

The same cause, to a certain extent, prevented Dr Burt from keeping himself abreast of the rapid progress of Medical Science, but the foundations of that noble study had been securely laid in his student days. As a practitioner, his sound practical sense, ready recognition of disease, and thorough knowledge of human nature, made his advice ever sound and reliable. Where severe disease or acute suffering were to be combated, there were none more attentive or more tender than he was. He perhaps had not enough patience for the minor ills and discomforts to which flesh is heir, and which often occasion more real distress than serious diseases; his honest and true nature shrunk from the petty arts by which many have crept into favour. He never pretended an interest which he did not feel, nor simulated an attention which the exigencies of the case did not demand.

Amidst the many dissensions which divided the medical practitioners in Edinburgh, Dr Burt always preserved his own position, and stood well with and was trusted by all parties; and he never ceased to deplore what in practice he failed to acknowledge,—the social separations which public differences of opinion in many cases involved. His friendship, once given, was never causelessly withdrawn, and no man ever lived who more willingly and cordially served a friend.

Scarcely a fortnight before his death, he was gratified at receiving the elegant volume by Sir Wm. Fergusson entitled "Lectures on the Progress of Anatomy and Surgery," and dedicated to him in the following just terms:—"This volume is inscribed to Dr JOHN G. M. BURT, F.R.S.E., late President of the Royal College of Physicians of Edinburgh, etc., etc., in testimony of his professional eminence and social worth, and in affectionate remembrance of an uninterrupted friendship of more than forty years, by his early schoolfellow, the Author."

Nor was Dr Burt unmindful of his duties as a citizen. Early in life he was a High Constable of Edinburgh, and for many years, and until that body was merged in the Town Council, an active Commissioner of Police. For a long period he represented the College of Physicians at the Board of the Royal Infirmary, an office from which he only retired in January last.

He took a warm interest in municipal affairs, and had considerable influence in his Ward. No one who witnessed the scene will easily forget the coolness, courage, and determination with which he forced an unwilling audience to hear him when the representation of his Ward was very keenly contested in 1866.

He was interred in the Dean Cemetery on the 14th of February, and although it was the wish of his relatives that the funeral should be as quietly conducted as possible, yet, despite the tempestuous character of the day, few private citizens have ever been followed to the grave by a larger number of friends anxious to pay the last tribute of respect to their memory.

THE LATE M. SERRES, AND DR JOHN DAVY, F.R.S.

MEDICAL science has lost two of her most distinguished cultivators in the persons of M. Serres, eminent both as a physician and a comparative anatomist, who died in Paris on 24th January, at the age of eighty; and of Dr John Davy, brother of Sir Humphry, who was a Graduate in Medicine of Edinburgh in 1814, and whose physiological researches are widely known. Dr Davy served as an Army Surgeon, and attained the rank of Inspector-General of Army Hospitals. He died at Ambleside on 24th January, in his seventy-eighth year.

GOODSIR FELLOWSHIP—SUBSCRIPTION LIST *continued.*

Dr James Miller,	£2	2	0
Dr A. G. Miller, Charlotte Square,	2	2	0
Dr J. Gerhard Reid, Cape of Good Hope,	1	1	0
Dr James Sabben, Stoke Newington,	1	1	0
Edinburgh University Club, London, per Dr Halley, *Treasurer*,	10	10	0
Dr Lorimer, Ticehurst, Sussex,	1	1	0
Dr Heslop, Birmingham,	1	1	0

PUBLICATIONS RECEIVED.

Annual Report of the Surgeon-General of the United States Army, 1867.
Frederic Bateman, M.D.,—On Aphasia. London, 1868.
Alexander Bruce, M.S.,—An Epitome of the Venereal Diseases. London, 1868.
Dr E. H. Clarke,—Observations on the Nature and Treatment of Polypus of the Ear. Boston, 1867.
W. J. Coulson,—On Stone in the Bladder. London, 1868.
W. T. Gairdner, M.D.,—On Certain Moral Aspects of Money-getting. Glas., 1868.
Guy's Hospital Reports, vol. 13. London, 1868.
Health of the Navy,—Report for 1866-67.
J. A. B. Horton, M.D.,—On the Guinea Worm. London, 1868.
Klob's Pathological Anatomy of the Female Sexual Organs. Translated by Drs Joseph Kammerer and B. F. Dawson. New York, 1868.
Pennsylvania Hospital Reports, vol. 1. Philadelphia, 1868.
W. Sharp, M.D.,—Organopathy. London, 1867.
W. Sharp, M.D.,—On the Physiological Action of Medicines. London, 1866.
Sir J. Y. Simpson, Bart., M.D.,—Proposal to Stamp out Small-pox. Edin., 1868.
H. R. Storer, M.D., — Pocketing the Pedicle. Boston, 1868.
Sir Henry Thompson,—On the Diseases of the Prostate. London, 1868.
Thomas Wood, Ph.D.,—Chemical Notes. London, 1868.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On Primary Diffuse Cutaneous Cancer*, by Dr VALD. RASMUSSEN. Translated from the *Hospitals-Tidende*, Copenhagen, January 1, 1868, by WILLIAM DANIEL MOORE, M.D. Dub. et Cantab., M.R.I.A., L.K.Q.C.P.I.; Honorary Fellow of the Swedish Society of Physicians, of the Norwegian Medical Society, and of the Royal Medical Society of Copenhagen; Secretary for Sweden, Norway, and Denmark to the Epidemiological Society of London.

WHILE cancroid (epithelial cancer) very frequently occurs in the skin, and always primarily, carcinoma in the skin and subcutaneous connective tissue is, on the contrary, comparatively rare, if we except the cases in which cancer is propagated from subjacent organs, or becomes developed in the vicinity of cancerous tumours, especially in the breast. It is most frequently secondary,[1] although this too is rare; and I remember only to have seen it once among the tolerably large number of dissections which I have either performed myself or have seen made by others,—in this instance, with extensive carcinoma in the internal organs there were very numerous tubercles, some as large as a pigeon's egg, in the subcutaneous connective tissue, scattered over the whole body, but without ulceration of the skin. As a primary affection, cutaneous cancer is certainly among the greatest rarities,[2] although Rokitansky states that he has frequently seen multiple tubercles primarily developed.[3] Lebert quotes twenty cases of primary cutaneous cancer, eleven of which occurred in the face (six in the under, two in the upper lip, and three in the alæ nasi), four on the penis, three on the labia majora, one in the epigastrium, and one on the shin. There is, however, scarcely any doubt that many, and perhaps the majority of these cases belong to cancroid, as we nowadays understand it. This is evident, partly from the locality stated, which is precisely the favourite seat of cancroid, partly from the description itself, giving the mixed forms of cancroid towards the surface, carcinoma in the depth, the finely filamentary stroma containing elastic filaments[1] (remains of the subcutaneous connective tissue). We ought, at all events, not unconditionally to set down these cases of Lebert's as true carcinomas, as Förster does; for Lebert, as is well known, based the diagnosis of carcinoma solely on the presence of specific (heteromorphous) elements, cancer cells, while cancroid, on the other hand, was, in his opinion, only an hypertrophy of the epithelium (homœomorphous formation).

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[1] A rare case of medullary melano-carcinoma in the dura mater cerebri, with secondary depositions in the soft membranes of the brain and spinal cord, in the lateral ventricles, the lungs, the heart, the spleen, the pancreas, the kidneys, the supra-renal capsules, the mesentery, and with scattered tubercles —some as large as nuts—in the subcutaneous connective tissue, is described by Professor Malmsten and Dr Blix in the *Hygiea*, vol. xx., Stockholm, September 1867, p. 398.

[2] Dr Ritter, in the Archiv für klinische Chirurgie, 5te bd., 1 h., 1863, p. 338, communicates a case of congenital carcinoma in the skin which is certainly unique. The tumour, the size of a pigeon's egg, proceeded from the point of the nose; it was extirpated, but the child died when three weeks old.

[3] Handbuch d. pathol. Anat., 2te Th., p. 74.

All the forms of cutaneous cancer mentioned by writers are described as circumscribed tumours, which, after the lapse of a shorter or longer period, constantly pass into ulceration. From these they except only the ivory-like cutaneous cancer, *carcine eburnée*, described by Alibert, which is looked upon by Rokitansky, Förster, Rindfleisch,[2]—though by the first only as probably,—as infiltrated medullary carcinoma, or as a schirrus of the skin and subcutaneous connective tissue. Still it is evident from the description that none of the authors mentioned have themselves observed this form of cutaneous cancer. I have in another place[3] shown that, according to Alibert's description, it would be more reasonable to look upon it as sclerodermia, or rather as a form of elephantiasis.

If we except the ivory-like cutaneous cancer, and the form of cancer of the skin proceeding usually from the breast, and described by Velpeau as *Squirrhe en cuirasse*, to which I shall hereafter revert, cases of diffuse, non-ulcerating primary cutaneous cancer are, so far as I am aware, not mentioned by writers, and the following example is the more deserving of record, as, in a diagnostic point of view also, it is of interest:—

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[1] Traité des Maladies cancéreuses, Paris, 1851, p. 602.

[2] Lehrbuch der pathol. Gewebelehre, 2te Lieferung, Leipzig, 1867, p. 257. Rindfleisch identifies Alibert's ivory cutaneous cancer and "cancer en cuirasse (Cruveilhier)" as a hard mammary cancer. "Squirrhe en cuirasse" is a designation which, as is well known, proceeds from Velpeau, and is not recognised by Cruveilhier, who divides schirrous cancer into an hypertrophic and an atrophic form: the hard atrophic form is spoken of only with reference to the breast; in its most intense degree the breast resembles that of a man, and then not only the gland, but also the subcutaneous tissue and the skin are attacked (Traité d'Anat. pathol. gén., t. v. p. 205). Then Alibert says nothing (Clinique de l'Hôp. St. Louis, Paris, 1833, p. 201) about his ivory-like cutaneous cancer always proceeding from the breast. As I shall hereafter have an opportunity of stating, there is another point of resemblance between *squirrhe en cuirasse* and *carcine eburnée*, as they both probably do not belong to cancer, but to sclerodermia.

[3] Hospitals-tidende, 1867, No. 24. Edinburgh Medical Journal, September and October 1867.

Carcinoma sclerosum subcutan. et cutaneum pectoris; Carcinoma mammæ, pleuræ, hepatis, costæ, gland. axillar. Pleuritis exsudat. sin.

Ane Margrethe Andersen, æt. 69, widow of a gravedigger, was admitted, on the 4th January 1867, into the second division of the Municipal Hospital. She was born of healthy parents, and no similar affection has occurred in her family. She has always enjoyed good health, and the only serious illness she ever laboured under was an attack of typhoid fever in her 30th year. Menstruation was always normal, and ceased in her 49th year. She has had two healthy children, who are still living.

Her present illness dates from the middle of last October, when, without any known cause, and particularly without cold,—to which she was in general but little exposed,—or any pressure, a swelling of the skin over the outer half of the great pectoral muscle set in, accompanied with moderately stinging pains and a strong feeling of tension in the skin, which latter assumed a light bluish-red colour. A medical man who was called in prescribed poultices and the use of an ointment; but her state became worse. A number of firm, reddish tubercles, about the size of hemp-seeds, situated in the skin itself, now appeared upon the part first attacked, and in the course of three weeks the affection gradually spread down over the shoulder and the whole arm. No febrile symptoms ushered in the disease or were subsequently present, and the appetite was very good, as was the general health; but she felt languid, and had occasionally night-perspiration. She now applied at the hospital.

On her admission she was found to be, for her age, strong and in good condition. Her complexion was rather sallow. The entire of the left shoulder and arm was greatly swollen, down to the metacarpo-phalangeal articulation. The skin on the shoulder and upper arm was of a deep red colour, and was tense, shining, and inclined to peel off. Below the elbow the colour passed gradually into a more brownish red, and on the hand was natural. Pressure gave the sensation of a firm (pasty) infiltration, which only on stronger pressure,—which, moreover, gave the patient some pain,—left a slight pitting: the skin could not be puckered or lifted into a fold. Down towards the back of the hand, and especially upon it, the swelling became softer, so that a distinct and persistent pitting was easily caused by the pressure of the finger. The sensibility was not diminished,—on the contrary, there was slight hyperæsthesia. The patient complained of a painful feeling in the arm, resembling the prick of a needle, and of a slight burning sensation in the skin. The motion of the shoulder-blade was somewhat limited; at the elbow-joint she could bend the arm to about a right angle with the upper arm: on the contrary, complete extension (the arm is held in a slightly flexed position) was almost impossible. From the shoulder the infiltration extended upwards on the side of the neck, and downwards towards the middle of the sternum, and down over the left mamma, and round to the left axillary region. In these places the skin was almost of the natural colour, with the exception of a part of about the size of the palm of the hand, and to the left of the manubrium sterni, and thence extending across over the whole lateral surface of the neck in a belt about an inch in breadth, between two natural furrows, where the colour is of a deep bluish red, and the hardness considerable, almost like that of wood; there is here, at the same time, a perceptible depression, both in relation to the surrounding infiltrated parts, and to the healthy right side. The skin is altogether immovable, as if adherent to the subjacent parts. In the part indicated, the tubercles already mentioned are met with; those, on section, are of the size of a pea; they lie in the skin itself, and rise over it with an arched smooth surface and intense bluish-red colour. Similar but smaller and paler tubercles are found scattered over the upper half of the left breast. In the axilla is a highly tubercular swelling of the lymphatic glands.

The left breast is remarkably swollen and tense, especially when compared with the right atrophic and flaccid mamma,—the upper half is tolerably soft, but rather swollen; the lower, on the contrary, is hard, is likewise swollen, rather uneven, not susceptible of the pressure of the finger. The skin can neither there nor in any of the other affected parts be plaited, but it is easily wrinkled, and, to a slight extent, movable over the proper and more deeply situated hardness. Beneath the breast, and in the lateral region, the infiltration is lost uniformly in the healthy skin. Nowhere are distinct tubercles met with at the boundaries of the swelling.

Physical examination exhibits nothing abnormal, with the exception of the modifications on percussion presented by the affected parts of the chest. The liver projects about one inch below the margin of the ribs. In other respects, the patient says she feels right well: she has some thirst, a very good appetite; the tongue is but little furred; the bowels are regular. Pulse 108. The urine contains no albumen.

On the 12th February, the infiltration extended along the left side down to the inguinal region. On the 23d there was some difficulty in swallowing, and some dyspnœa. Her voice had become rather hoarse. On the 6th of March, there was some œdematous swelling of the left cheek. On the 9th she had repeated attacks of vomiting, and she died at 10 o'clock in the evening of the same day.

The body was examined in the forenoon of the 11th.—Cadaveric rigidity. The body in good condition. The arm much swollen;

of doughy consistence. On the left side of the chest, corresponding to the great pectoral muscle, and thence extending in a slender belt across the neck, the skin and the subjacent tissue are depressed, and as hard as wood. This part is, at the same time, studded with tubercles of the size of peas, of a whitish or light-reddish colour, and smooth arched surface. The hardness extends also down over the left breast, but diminishing, so that the skin on its upper part is easily movable, this mobility again diminishing on its inferior part, which is hard and swollen. On cutting into the first-mentioned hard part, both the skin and the subjacent tissue, down to the ribs, are found changed into a lardaceous mass, in which, in the outer layer, corresponding to the subcutaneous connective tissue, yellow remains of fatty acini are found scattered, and in the inner layer pale œdematous remains of muscular tissue. Where the infiltration does not reach quite to the very cutis, smaller scattered tubercles, varying from the size of a pea to that of a nut, internally becoming larger and coalescing, are met with. The same is repeated inwardly towards the ribs; in the middle the infiltration has a more whitish fibrous appearance, and is very firm. A grayish yellow fluid is expressible from the infiltrated parts, most abundantly internally and externally. In the upper half of the breast the skin is found to be scarcely altered, while, on the other hand, a reddish, firm, juicy tissue surrounds the several fatty acini as well as the acini of the gland; in the lower half this tissue is much more abundantly developed, and there are found at the same time in the connective tissue, under the gland and in the muscles, similar lardaceous tubercles. The lardaceous infiltration extends from the place first mentioned somewhat out over the anterior part of the shoulder; and in the axilla the glands are found to form similar firm lardaceous masses: through these the axillary vessels run, and, as well as the subclavian, lie in an exceedingly firm and lardaceous tissue. The axillary vein contains throughout fluid blood, but its calibre is somewhat diminished. On cutting into the arm and the rest of the shoulder, a large quantity of clear reddish fluid escapes. The skin itself and the subcutaneous tissue are readily loosened, together with the fascia communis, from the subjacent muscles. On the surface of section the skin appears perfectly natural, except that, as well as the subcutaneous connective tissue, where the fat is abundant, it is strongly œdematous.

In the left pleural sac are about two quarts of yellowish, slightly turbid fluid, containing numerous fibrinous flocculi. The lung is pushed upwards and inwards against the spinal column.

The heart is pushed over to the right, no part of it being found to the left of the margin of the sternum. The lung is strongly compressed, flat. The pleura is milky, opaque, and has, especially inwards towards the hilum, whitish yellow tubercles as large as peas, homogeneous on the surface of section, with a scanty yellowish juice. The pulmonary tissue itself is completely permeable, is tough, dry, and void of air. On the costal pleura similar, but not numerous tubercles are met with. The right lung is somewhat œdematous. The bronchial glands on the left side are swollen and coloured. In the pleura itself, on the right side, no tubercles are found. On the other hand, on the fifth rib, close to the angle, is a prominent tubercle, rather larger than a walnut, exhibiting on section a yellowish or slightly reddish yellow infiltration of the bony mass, which is brittle, and contains a yellowish thick juice. The heart is rather flabby, contains a large amount of fluid blood and a small quantity of fibrin; the muscular structure of the heart is pale, opaque, with some yellowish striæ. The spleen is somewhat enlarged and firm. The liver is of the ordinary size. On the surface, a couple of tubercles almost as large as walnuts, and several smaller tubercles, project; similar tubercles, though usually only of the size of peas, are met with in the parenchyma. The larger ones have a lardaceous appearance, somewhat yellowish in the centre; they are tolerably firm, and contain only a scanty amount of juice. The kidneys are rather cyanotic; their cortical substance is slightly atrophic; otherwise, these organs are unchanged. The intestinal canal is healthy. The bladder is almost empty; its mucous membrane is normal. In the fundus uteri is a fibrous body, of about the size of an egg, partly calcified. In other respects, the uterus and vagina, as well as the remaining pelvic organs, are healthy. No change in the spinal column.

The microscopical investigation confirmed the macroscopical observation. In the infiltrated parts of the skin the lardaceous mass was found to consist of alveoli filled with numerous irregularly formed flat cells, with one or two nuclei. In the tubercles situated both outwards towards the epidermis and inwards towards the ribs, the trabeculæ which formed the stroma were thin, the alveoli were large, containing numerous and fresh cells, while the middle, firmer, almost fibrous parts, exhibited the alveoli much limited in extent by great development of the trabeculæ of the stroma, so that in some places there were found only inconsiderable heaps of fat-granules, as remains of former alveoli. No active participation of the elements of the epidermis in the cancerous formation was discoverable. In the places where the infiltration extended out to the epidermis, the papillæ were found rather compressed, and the epidermis itself somewhat attenuated. The tubercles which projected on the surface of the skin consisted only of dense connective tissue. The other deposits had the usual structure of medullary carcinoma.

It will certainly be allowed, that this case presented no slight difficulty in a diagnostic point of view. The patient was admitted by the visitor, Dr Hirschsprung, under the diagnosis of cancer, which diagnosis also the medical men attached to the division of the hospital were most inclined to adopt. I myself, who, during the patient's stay in hospital, had abundant opportunity of seeing her, contended, with others of the physicians of the hospital, for another view, which I shall in the sequel support, not to excuse or cover an error, but because I believe that it is instructive, not only for the observer himself, but also for those at a greater distance, to see how two very different diseases may clinically, though only apparently, closely simulate each other. We, in fact, looked upon the disease as sclerodermia, and we were perhaps led to that conclusion chiefly by the great resemblance this case presented to one of sclerodermia, which we shortly before had the opportunity of watching, and which has been described by the writer in a more elaborate essay upon that disease.

We had before us a patient, who, for her age, 69 years, must be considered very strong and in good condition. Three months ago a swelling of the skin began to be developed over the great pectoral muscle on the right side, with lancinating pains, a feeling of great tension, and a light bluish red colour of the skin. According to her own statement, a similar, though much more considerable swelling, had in the course of three weeks, and immediately before her admission into hospital, formed over the whole shoulder and arm, with erysipelatous redness of the skin. When we ourselves got an opportunity of examining the patient, we found the part of the chest first attacked perceptibly sunk in, almost as hard as wood, immovable, and as if adherent to the subjacent parts. We found the whole shoulder and arm much swollen, more than double as thick as the other arm, with a pricking, burning sensation in the skin over the greater part of the arm, which was of a brownish-red colour, and almost incapable of receiving the impression of the finger. We then looked upon the affection as sclerodermia, which on the chest had commenced to pass over, or had entirely passed over into sclerosis, while on the arm it was still in the first stage (that of infiltration), and here even seemed quite to have the character of elephantiasis, and where the initial erysipelas had passed into a chronic state, instead of, as usual, disappearing, as, however, occasionally occurs in sclerodermia (Grisolles, Villemin's case).

The post-mortem examination brought us, as has been seen, information which showed that the disease was something quite different from what we had supposed. It at once occurs to endeavour to discover the differences which our case presented, compared with those hitherto known to belong to sclerodermia. The first peculiarity in it were the severe, flying, lancinating pains, which continued in the already sclerotic parts of the chest. These do not occur in sclerodermia, when sclerosis has set in; we then find neither spontaneous pains nor tenderness on pressure, but only a disagreeable feeling of tightness, due to the great pressure caused by the sclerotic connective tissue. Another important diagnostic element was this, that the skin on the breast, where the affection was undoubtedly more recent than above, could be slightly wrinkled or moved, which indicated that the infiltration had not its seat in the skin itself as in sclerodermia, in which the skin is the starting point of the peculiar infiltration, and therefore cannot be wrinkled or lifted into folds. Less importance must be attributed to the highly swollen and hard axillary glands in the concrete case. It is indeed true, that swollen lymphatic glands, according to the cases hitherto recorded, are only exceptionally met with in sclerodermia; but we had a state of considerable irritation of the skin of the arm, as the most likely origin of the swellings, as there was neither clinical nor anatomical sign of thrombosis of the vein. It was on the whole this peculiar state of the arm, which, at least in my opinion, made the supposition of the existence of a sclerodermia extremely probable, as it exhibited a striking resemblance to the highest degree of sclerodermia or rather of elephantiasis. The essential point to be decided was, whether the œdema had taken place in the first instance, upon which the erysipelas had supervened, or whether the reverse had been the case,—in other words, whether we had before us a case of anasarca erysipelatosum, or a form of elephantiasis. The patient's own statement seemed to be in favour of the latter view, as she thought that the œdema and erysipelas had occurred simultaneously, though she was not quite certain upon the point. Such an anasarca erysipelatosum is certainly extremely easily recognised, as we so frequently see it, especially on the lower extremities in general dropsy, where acute erysipelas supervenes upon œdema of long standing. If, on the contrary, erysipelas has, as in our case, occurred about contemporaneously with the œdema, and if it has existed long, the skin acquires a brownish-red colour, a peculiar firmness, does not take the impression of the finger, cannot be wrinkled or plaited, and thus acquires quite the same characters as in a high degree of sclerodermia in the stage of infiltration. Just as easy as it was to see the true state of the case after dissection, was it difficult at the bedside to determine the mutual connexion between the glandular swellings, the œdema, and the erysipelas. On the whole, it was the last, which had occurred quite accidentally, and was quite unessential to the disease, which led us from the right track. It may further be mentioned, that the patient succumbed more rapidly than one would expect in a sclerodermia of comparatively inconsiderable extent.

It can therefore not be denied, that there were several and essential differences between the group of symptoms, which we find in sclerodermia, and that which we observed in our patient, and we were of course not blind to this fact. It must, however, be remembered that acute cases of sclerodermia resembling elephantiasis are still very little known, and when we, as I have stated, believed we recognised sclerodermia in its two characteristic stages, these differences must in our eyes have had less importance. When I add that primary cutaneous cancer of the chest with secondary development in the breast was foreign to the experience both of others and of myself, our incorrect diagnosis will perhaps be looked upon in a less unfavourable light. I believe, at all events, that it has been attended with some positive advantage, as by it we have gained characteristic marks between the two diseases, sclerotic cutaneous cancer and sclerodermia, which may in future ensure us a correct diagnosis. And this is in practice of great importance, as cancer is absolutely fatal, while sclerodermia is only exceptionally so. With respect to treatment, too, the diagnosis will be important, as, if we are sure that we have a sclerotic cutaneous cancer before us, we will abstain from the often severe treatment recommended in sclerodermia.

But may our case then not come under the form of cancer which Velpeau has described as *Squirrhe en cuirasse?* Although this question must, for the present, be answered in the negative, I will take the opportunity of speaking of this variety of cancer, partly because it is either mentioned imperfectly and incorrectly by writers, or is wholly overlooked by them, especially by most pathological anatomists, partly because I entertain a doubt how far all that Velpeau refers to this designation ought, on the whole, to be looked on as cancer.

Velpeau[1] designates as *Squirrhe en cuirasse ou tégumentaire*, or, as *Squirrhe ligneux en plaques ou diffus*, a form of mammary cancer to which he first directed attention in 1838. Its favourite seat is the skin, and it occurs most frequently as a complication of an already existing cancer, and only in a smaller number of cases does it confine itself, from beginning to end, exclusively to the skin. It commences in or around the breast, either on a large surface, or in several smaller spots, which however quickly coalesce: the skin in such places becoming hard, wrinkled, thickened, dotted with light reddish or grayish points, and acquiring the appearance of hard leather. The affection rapidly spreads, similar spots, sometimes rather elevated, sometimes rather depressed, occurring in the periphery, and it may thus extend over the whole chest, which becomes, as it were, encased in a solid cuirass. The disease in general proceeds unobserved, until it has attained a certain degree; but then burning sensations and flying lancinating pains occur in the affected parts, followed by sleeplessness, anxiety, and disgust to food. Subsequently the respiration becomes difficult, the movements of the chest are impeded, and the thorax seems to be surrounded with a coat of mail, which closes more and more, threatening to suffocate the patient, and may at last become so hard and immovable that respiration is rendered impossible.

In an anatomical point of view, Velpeau gives but scanty information. He says only, that we can by an incision easily satisfy ourselves that the affection has its seat exclusively in the skin, that this may become of double the normal thickness, and that it is as firm as tanned leather. He gives neither reports of post-mortem examinations, nor of microscopical examinations of the skin.

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[1] Maladies du Sein, Paris, 1854, p. 426.

It is well known that in the neighbourhood of a hard mammary cancer a diffuse sclerotic infiltration of the skin, or, more correctly, of the subcutaneous connective tissue, is sometimes developed, and this often to a great extent. Such a form of cancer of the breast it is which is generally suggested by the expression *Squirrhe en cuirasse*, and which is spoken of in the different manuals as well as in Pitha and Billroth's.[1] That our case cannot come under such a form of mammary cancer follows, both from the clinical observation and the anatomical investigation, in which we found on the chest cancer in its oldest, characteristic development (its so-called cicatricial tissue), while the tubercles in the breast were evidently recent, abounding in cells, and of luxuriant growth.

It is, however, scarcely this form for which Velpeau has reserved the name *Squirrhe en cuirasse*, but rather that, where the infiltration is confined exclusively to the skin and may occupy almost the whole body, and where the mamma does not seem properly to be the starting point, but is drawn into the process only on account of its accidental position. This form, which Velpeau himself designates as rare, is subsequently described by scarcely any writer; and it is what I consider cannot be referred to cancer, but is rather a form of sclerodermia.

Velpeau communicates only one accurately recorded case, observed in his own clinique; I shall briefly quote it here. A seamstress, aged 36, born of healthy parents, got, after passing safely through her confinement, about a year and a half before, a hard, not painful, tumour over the left breast, according to her statement produced by the pressure of a corset. The induration extended around the whole gland, then beyond it, and down over the whole left side to the eighth rib. Four months ago the right breast was attacked in a similar manner, as well as the skin over the sternum. On admission into hospital, on 20th January, the breasts were larger than the normal size, the left being somewhat larger than the right, somewhat flattened, and as it were adherent to the subjacent tissue; the nipple was large, cauliflower-like, especially on the left side. The skin had an erysipelatous tint, especially on the right side, and the redness, which disappeared on the pressure of the finger, was, on the whole, stronger on the right side. The normal mobility of the integuments was gone. The breasts were firm, shining like marble; all the affected parts on pressure felt hard as wood. The patient's general health was good. The affection advanced still further during her stay in the hospital round to the back, an erysipelatous redness and swelling of the skin preceding it. On the 2d February, the left nipple was loosened by an areolar ulceration, with the discharge of an ichorous fluid. The redness and infiltration constantly advanced, both on the neck and down over the abdomen. Difficulty of breathing set in, with a feeling of constriction, and with burning pains in the back, left side, and arm. Notwithstanding her state, the patient desired to be dismissed on the 16th February.

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[1] Handb. der algem. u. speciellen Chirurgie, 3te Bd. 2te Abth. p. 100. Here tubercles as large as beans in the skin are mentioned; Velpeau speaks only of *plaques*. It is not easy to see from Billroth's description whether the latter is based upon his own observation, or only on a rather imperfect reproduction from Velpeau. The diffuse infiltration, which specially interests us, is not mentioned.

Velpeau adds, that he entertains no doubt that this unfortunate patient, in less than a year after leaving the hospital, would feel herself included as in a breastplate of iron, and would by it be, so to speak, suffocated, and that without the presence of ulceration, or dissection necessarily exhibiting cancerous depositions in the internal organs.

It seems to me that this disease, of which the case quoted may serve as an example, in the strongly marked irritative, erysipelatous state of the skin, with subsequent retraction, and in the good state of the general health, in spite of the long duration of the disease, strongly resembles sclerodermia, although the sclerosis had not yet attained to a considerable degree; and that this resemblance becomes still more striking when we compare the clinical course of the disease with the anatomical changes indicated by Velpeau himself: the degree of thickness and firmness of the cutis, its leathery appearance, this striking character, mentioned by all investigators of sclerodermia, the absence of ulceration, or, at all events, as in sclerodermia, only slight and superficial ulceration, the absence of metastases and of cachexy. When I add, that no one has yet demonstrated anatomically such a diffuse infiltration of the skin itself, and exclusively of it, and that this moreover exhibits a contrast to the form (the tubercular form) under which cancer, at least in a certain stage of its development, always manifests itself, and which we also found in our case, I do not think it wholly unjustifiable to entertain some doubt whether this *squirrhe en cuirasse* ought, on the whole, to be looked upon as cancer. To me it appears highly probable, that *squirrhe en cuirasse* and *carcine éburnée* are one and the same disease, and that both are forms of sclerodermia. On account, however, of the imperfect description given, no stringent proof of this can be brought forward, but still less can this be done in favour of cancer; it appears to me therefore incorrect to refer these two diseases to cutaneous cancer, as has hitherto been traditionally done, at least so long as we have not more definite anatomical investigations than those we are as yet in possession of.

ARTICLE II.—*Struma Exophthalmica*[1] (*Vascular Bronchocele and Exophthalmos*). By Professor VIRCHOW. Translated, with Notes and Observations, by J. WARBURTON BEGBIE, M.D.

[IN a recently published part of the important work of Virchow on "Tumours" (Die Krankhaften Geschwulste),[2] there occur some statements and reflections in regard to a peculiar form of disease which, both in this country and on the Continent, has of late years attracted very considerable attention. The opinion, as to the nature of so complex a disorder as "Vascular Bronchocele and Exophthalmos," entertained by the eminent professor of Berlin cannot fail to prove of interest to the members of the profession in general. I have therefore thought it worth while to offer a translation of the author's observations, and to this have added some remarks suggested by their perusal. The latter, for the sake of clearness, and in order to avoid all risk of confusing the reader, are thrown entirely by themselves at the close of the translation. The footnotes and references are those of the author unless otherwise distinguished.]

Finally, that is an exceedingly remarkable connexion of goitre with affection of the heart (Herzreizung, literally heart irritation), and large staring eyes (Glotzaugen). So far as is yet known, Flajani[3] was the first to notice the coincidence of goitre with lasting palpitation of the heart. He mentions three cases of this nature, all of which occurred in men, two of the three being youths. In all of these cases, chiefly by means of external treatment of the bronchocele, a cure was obtained. Of the condition of the eyes he says nothing, but visible enlargement and varicosity of the veins over the thyroid gland were noticed. Parry[4] appears to have been the earliest to observe the third symptom—prominence of the eyes. Next to him, I find two descriptions with post-mortem examinations of accurately investigated cases by Adelmann,[5] in which considerable goitres appeared with enlargement of the heart. During life there existed continued violent palpitation in the region of the heart, great dyspnœa, pain in the abdomen; and in one of the cases it is mentioned that, in addition to these symptoms, "the staring look of the large eyes caused a very remarkable aspect." These facts, notwithstanding, remained nearly unknown till the new experience of Pauli,[1] Von Basedow,[2] and Graves,[3] was published. Although the first communication of Graves appeared in 1835,[4] that of Von Basedow has, notwithstanding, the advantage; the history of the disease having been, in the first place, completely given by him, and, through his knowledge, much information regarding it supplied. By Von Basedow was originated the designations of exophthalmos, of the staring eye (Glatzauges) as the most striking feature, and of the cachexia exophthalmica, large staring eye cachexia (Glatzaugenkachexie), which has more recently become so universally known; and it is from this circumstance that, according to the proposal of G. Hirsch,[5] it has been customary to style the whole assemblage of complex symptoms "Morbus Basedowii." Trousseau,[6] on the other hand, maintains that the disease should be termed "Graves' disease." I hold that this is not correct; for Graves regarded both the palpitation of the heart and the goitre as essentially symptoms of hysteria, while the condition of the eyes is only incidentally mentioned by him; moreover, he is not the first who observed the complex symptoms. By recent writers, prominence has been assigned at one time to one, at another time to another of three principal symptoms; and, according to it, the choice of a designation for the disease has been made. Of late, the name "Struma exophthalmica" (goitre exophthalmique) has become very generally disseminated; but Lebert,[7] having reference to the affection of the heart, suggests for the disease the appellation of "Tachycardia Strumosa."[8]

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[1] The word "Struma," as used by Virchow in the text, and employed by various German writers, means simply Bronchocele or Goitre—the enlargement of the thyroid gland from whatever cause—and has no relation to that morbid condition or cachexia known as the strumous, or Scrofula. (Translator.)

[2] Zweiundzwanzigste Vorlesung, iii. band, i. halfte, s. 73.

[3] Guiseppe Flajani, Collezione d'osservazioni e riflessioni di Chirurgia, Roma, 1802, t. iii. p. 270.

[4] Caleb Hillier Parry, Collections from the unpublished Medical Writings, London, 1825, vol. ii. p. 3, quoted by Stokes. "Die Krankheiten des Herzens u der Aorta." Uebers, Von Lindwurm, Würzb. 1855, s. 232.

[5] Adelmann, Jahrbücher der philosophisch. medicinischen Gesellschaft zu Wurzburg, 1828, bd. i. ii., s. 104–108.

The condition of the Thyroid gland is, during life, subject to variety. As a general rule, the swelling is not so great as that of the ordinary goitre, still a very marked enlargement is discovered. The most conspicuous feature of the swelling is an increased development of the vessels, with which, not unfrequently, a diastolic beat and "souffle" (ein diastoliches Klopfen und Rauschen) are perceived, so as to be directly styled "Struma aneurysmatica,"[9] or

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[1] Pauli, Heidelberger Medic. Annalen, 1837, s. 218.

[2] Von Basedow, Casper's Wochenschrift, 1840, No. 13, s. 198.

[3] Robert James Graves, Klinische Beobachtungen, Deutsch von Bressler, Leipz., 1843, s. 409.

[4] Stokes, a. a. O. S. 234.

[5] G. Hirsch, Klinische Fragmente, Königsberg, 1858, heft ii. s. 224.

[6] Trousseau, Gazette Hebdomadaire de Médecine, 1862, No. 30, p. 472.

[7] Lebert, Die Krankheiten der Schildrüse, s. 307.

[8] "Tachycardia" (Ταχυς, quick, Καρδιά, the heart), in reference to the rapid and excited action of the organ. The exact term suggested by Lebert is "tachycardia exophthalmica strumosa." See his "Grundzüge der Artzlichen Praxis," 1867, s. 230. (Translator.)

[9] Henoch, Casper's Wochenschrift, 1848, No. 40, s. 629. Romberg und Henoch, Klinische Wahrnehmungen und Beobachtungen, Berlin, 1851, s. 191. (Of this interesting paper, an abridged rendering by the translator will be

"Bronchocele vasculosa."[1] Sudden appearance of swelling, and its rapid subsidence, have been associated. The results of anatomical inquiries are not agreed upon.[2]

In the only case investigated by me, and of which Traube and Von Recklinghausen have given an account, the gland moderately increased in size, exhibited simply excessive plastic without any gelatinous, nodular, or cystic formation. The lobes of the thyroid reached very distinctly forwards, the interstitial structure was abundant, and the veins only were generally enlarged. Very similar was its condition in the case given by Reith, also in one by Trousseau, which Peter has described, except that, in the last, there is no mention made of the enlargement of the veins. Smith found a very considerable augmentation, especially of the right lobe of the gland, while the arteries were much enlarged and strangely tortuous. Markham remarked the gland as being large and firm, at the same time (in a woman of 26 years of age) he found an enlarged and persistent thymus gland. In an instance recorded by Hirsch, the thyroid gland was big, hard, and externally covered by enlarged vessels. Heusinger describes the thyroid as double the natural size and uniformly hypertrophied, but without the presence of any abnormal formation. Very similar was its condition, according to the observation of James Begbie. In the case of Schleich, related by Laqueur, Runge found a large gelatinous goitre. Naumann describes the thyroid as very large, its structure uniformly red, and presenting hæmorrhagic spots, the arteries greatly developed. Von Basedow discovered the gland enormously enlarged with hydatid and varicose degenerations, and Marsh (Sir Henry) saw the thyroid irregularly lobed, containing cysts, which were occupied by a clear fluid, and the jugular veins very greatly distended. Analagous to this was the case recorded by Banks. Lastly, Praël found a ponderous goitre which stretched downwards into the cavity of the chest, its right lobe embracing

found in the "Edinburgh Medical and Surgical Journal," April, 1854.) Bullar, Medico-Chirurgical Transactions, 1861, vol. xliv. p. 37.

[1] Laycock, Edinburgh Medical Journal, 1863, p. 1. J. Warburton Begbie, ditto, September 1863, p. 211.

[2] Marsh, Dublin Journal of Medical Science, 1842, vol. xx. p. 471. Von Basedow, Casper's Wochenschrift, 1848, No. 49, s. 775. Heusinger (in Braunschweig), Casper's Wochenschrift, 1851, No. 4, s. 53. Naumann, Deutsche Klinik, 1853, No. 24. s. 269. Smith, bei Stokes, a. a. O., s. 239. Banks, Dublin Hospital Gazette, 1855 (quoted by W. Moore, Dublin Quarterly Journal, 1865, November, p. 347). James Begbie, Edinburgh Medical and Surgical Journal, 1855. Case-book, p. 33. F. Praël, sen., Archiv f. Ophthalmologie, 1857, bd. iii. 2, s. 199. Markham, Transactions of the Pathological Society of London, 1858, vol. ix. p. 163. Hirsch, a. a. O., s. 224. L. Laqueur, De Morbo Basedowii nonnulla, adjecta singulari observatione, Dissertatio inauguralis, Berol., 1860, p. 12. Traube und Von Recklinghausen, Deutsche Klinik, 1863, No. 29, s. 286. Trousseau et Peter, Gaz. Hebdom., 1864, No. 12, p. 181. Archibald Reith, Medical Times and Gazette, November 1865, p. 521.

the trachea, and having passed into a state of cartilaginous degeneration.[1]

It appears from the foregoing comparison of observations, that it is not a determinate variety of goitre, or a fixed enlargement of the same, or definite course, which settles the appearances. Indeed, in many cases the alteration of tissue is so trifling, that we may ask, as Graves did, whether it is in reality a bronchocele, or merely a swelling (intumescentia) of the gland which exists. From this consideration, there arises a direct refutation of the opinion entertained by some, that the cause of the exophthalmos is the pressure of the thyroidal tumour on the vessels of the neck.[2] Further, it shows, that at first a simple swelling of the gland exists from which a true bronchocele is formed, and that the goitre runs its usual course from a very moderate, chiefly plastic formation, or advances to a fibrous induration of nodular form. The same series of changes, however, occurs with sufficient frequency in the ordinary goitre, without the appearance of the other symptoms, and accordingly the alteration of the thyroid gland is to be regarded as a secondary phenomenon. That the persistent enlargement of the vessels, and especially of the veins, plays a decided part, may already be conjectured from clinical details. It seems to depend less upon the condition of the arteries; at least in all cases in which these were remarkably changed, there existed also considerable disease in other parts of the vascular system.

In nearly all the cases the Heart is greatly enlarged, for the most part dilated, even where the valves are healthy; the left ventricle being chiefly affected. The aorta and great vessels were, in most instances, but by no means in all, atheromatous. Clinical investigation demonstrates that the hypertrophy of the heart belongs to an advanced stage of the disease; accelerated motion (100 beats and upwards in the minute) is the ordinary phenomenon.

The earliest entertained notion in regard to the Eyes was that a hydrophthalmos existed; this is now on all sides abandoned. Naumann alone has found a trifling enlargement of the eyeball. The essential change lies in the fatty tissue of the orbit, which is sometimes hypertrophied, but is for the most part expanded by a hyperæmic swelling, capable of being overcome during life by pressure, and readily disappearing after death.[3] Reith alone, besides greatly distended veins, found a small quantity of partially coagulated blood effused over the eyeball.[1] If to this be added a fatty degeneration of the muscles of the eye, as Von Recklinghausen detected, we are enabled to understand how so considerable a prominence of the eyeballs occurs, that in fact the eyelids can no longer be closed,[2] and that in the uncovered portion of the eye inflammation may be induced, which, in turn, may lead to a complete destruction of the cornea and wasting of the eyeball.[3] As a rule, the prominence of the eyes is on both sides and also symmetrical; still it does happen that the protrusion is either earliest seen, or, at all events, is more marked in one eye than the other.[4]

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[1] Besides the names already mentioned in the text, that of "Cardiagmus strumosus" (Καϱδιωγμὸς, a Hippocratic term, synonymous with Cardialgia) has been applied to this disease by Hirsch. (Translator.)

[2] Piorry, Gazette Hebdomadaire, 1862, No. 30, p. 477. A. Cros, Gazette Hebdom., No. 35, p. 548. Nunneley, Medico-Chirurgical Transactions, vol. xlviii. p. 32.

[3] Dechambre (Gazette Hebdomadaire, 1862, p. 482) quotes an interesting parallel observation by Decès (Thèse inaugural sur l'Aneurysme cirsoïde, 1857), where a transitory exophthalmos appeared in a woman who suffered from alternating arterial dilatations in different parts of the body.

For the present it must be left undecided, whether the majority of observers have found Enlargement of the Spleen to be an essential or merely an accidental result. At all events, we cannot on *à priori* grounds lightly estimate the disturbance of the digestive function, more particularly the vomiting and tendency to diarrhœa, which are often observed. There always remain the three intimate symptoms or triad: affection of the Heart, the Thyroid gland and Eyes (Orbital-polsters, orbit cushions), as the regular, although, in relation to each other, not constant phenomena, and it may be asked what the explanation of this combination is. That the lesion of the thyroid gland is not to be considered as the centre or mainspring of this complex disorder, I have already made apparent. Individual observers indicate that the bronchocele may be altogether absent.[5] Still less can we regard the affection of the fatty tissue in the orbit as of principal importance, particularly as the protrusion of the eyes is sometimes wanting, or else it only becomes apparent at a later stage.[6] Neither can it be held that the hypertrophy of the heart is itself to be looked upon as the point of departure. On the one hand, hypertrophy is not always present; and on the other, considerable hypertrophy of the heart often exists without the staring eyes and without the goitre. The anatomical changes of all these elements cannot then be regarded as diagnostic.

We come therefore to the question of the functional disturbances. Here I must specially call attention to the fact, that there exists a peculiar combination of affections of the thyroid gland and heart. This combination, which was formerly mentioned (vol. i. p. 114), is the so-called Iodism, or the goitre cachexia (Kropfcachexie). Here we observe, with the disappearance of the goitre as the consequence of a slight iodism, a most remarkable acceleration of the pulse, not unfrequently the production of annoying palpitation. Only the exophthalmos is wanting; instead of it there is another prominent symptom rarely present in the "struma exophthalmica,"[1] to wit, the association of rapid and great emaciation with voracious appetite [mit Bulimie].[2] The point now mentioned, at all times worthy of notice, is so much the more so, from the circumstance that in a case of Oliffe's,[3] the moderate exhibition of iodine in "struma exophthalmica" produced the worst effects. Trousseau[4] himself has made similar observations, and on this account has not hesitated, in the discussion on iodism in the French Academy of Medicine, to regard as cases of "struma exophthalmica" instances which, by Rilliet, had been described under the name of iodism. Rilliet[5] has, on the contrary, in the most decided manner claimed as examples of iodism recorded cases which had been described under the name of "struma exophthalmica." Extended observations are required in order to clear up this dispute. Neither bronchocele nor iodine produce the phenomena of the "cachexia exophthalmica," or of the "cachexia iodica;" in both cases there must, in addition, be the presence of something peculiar. In reference to this, we must go back to an original predisposition; and I may mention that Bednar[6] has repeatedly found in newly born children the co-existence of an enlargement of the thyroid gland and hypertrophy of the heart. Yet these facts, supposing them to possess a general importance, which is unlikely, do not exclude from the inquiry the existence of a further cause.

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[1] A. Reith, l. c., p. 521.

[2] Graves, a. a. O., s. 411. Stokes, a. a. O., s. 231.

[3] Casper's Wochenschrift, 1840, No. 14. s. 221. Von Gräfe, Archiv f. Ophthalmologie, 1857, bd. iii. 2, s. 282. Teissier, Gazette Méd. de Lyon, 1863, No. 1, 2.

[4] Von Basedow, a. a. O., 1848, No. 49, s. 772. Henoch und Romberg, Klinische Wahrnehmungen, s. 182. Reith, l. c., p. 521. Praël, a. a. O., s. 206, 207.

[5] Praël, a. a. O., s. 209.

[6] Henoch und Romberg, Klinische Wahrnehmungen, s. 179, 180.

In the acceptation of the humoral pathology, it is concluded that there is always a blood derangement when several organs are together affected, without there appearing to be a simple dependence of the disease on one or other of these. Von Basedow[7] has forthwith extended this view to the establishment of an independent dyscrasia, which he has expressed as a hidden scrofula. At a later period, he has pointed out this dyscrasia as being similar to the chlorotic.[8] This opinion has been subsequently embraced by many other observers,[9] and Anæmia has become the theoretical foundation of the complex symptoms, while Mackenzie has gone so far as to indicate the condition of the eyes as being neither more nor less than "exophthalmia anæmica." In favour of this view, there is not merely the frequent occurrence of pulsations, palpitations, and murmurs in the vascular system, as in chlorotic patients,—not merely the circumstance that the majority of cases of staring eyes with goitre (Glotzaugen-Kropf) are observed in women,[1] and that on several occasions pregnancy and child-bearing have exerted a remarkably favourable influence on the removal of the malady,[2] but also very specially the experience which we possess in relation to the satisfactory operation of an invigorating treatment.

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[1] Trousseau, Union Méd., 1860, tome 8, p. 437–456.

[2] Emaciation and bulimia do sometimes co-exist in cases of "struma exophthalmica."—(*Translator.*)

[3] Trousseau, Union Méd., 1860, tome 8, p. 513.

[4] Trousseau, Gazette Hebdom., 1860, Avril, p. 219–267.

[5] Rilliet, Mémoire sur l'Iodisme constitutionnel, Paris, 1860, p. 83.

[6] Bednar, a. a. O., s. 79.

[7] Von Basedow, a. a. O. 1840, s. 225.

[8] Von Basedow, in the same place, 1848, s. 772.

[9] L. Gros, Gaz. Med., 1857, p. 232. Hervieux, Union Méd., 1857, No. 117, p. 477. Beau, Gaz. Hebdom., 1862, No. 34, p. 539. Fischer, Arch. Génér., 1859, Dec., p. 671. Begbie, Edin. Med. Jour., 1863, Sept., p. 201. Praël, a. a. O., s. 210.

It is, however, undoubted that the Anæmia, granting its existence, cannot directly produce such an effect. At the very least, we must assume that, through the disordered blood, an injurious influence on the nerves takes place. In returning, however, to the nerves, the question arises, whether anæmia is required in order to produce such a condition of the nervous system. Different observers[3] have been content to look upon it as a feeble state of the nervous system (einen Schwächezustand des Nervensystems). Graves and Brück[4] considered it hysterical. Stokes[5] limited himself to pointing out that the essence of the disease consisted in a functional disturbance of the heart, upon which organic change is apt to follow. More recently a further advance has been made, and attention has been directed to the nerves of the heart, and especially to the Sympathetic,[6] with perhaps also participation of the spinal cord.[7] Köben, the first to offer this conjecture, supposed that the sympathetic was compressed and irritated by the bronchocele; since then the bronchocele has justly been regarded as pertaining to the Neurosis. In support of this view, some not unimportant facts in pathological anatomy have been advanced. Peter[8] found the lowest cervical ganglion enlarged and greatly reddened, its interstitial tissue increased, the nerve fibres diminished. Somewhat similar is the account given by Moore[9] of the inquiry conducted by Cruise and M'Donnel. Reith describes the middle and lower cervical ganglion on both sides, especially the left, as enlarged, hard, and firm; and, when viewed under the microscope, filled with a granular material resembling a lymph gland in the first stage of tuberculosis. The trunk of the sympathetic itself, as well as its branches proceeding to the "arteria thyroidea inferior" and "arteria vertebralis," were enlarged. He held these changes to be tubercular. Directly opposed is the observation of Von Recklinghausen, in so far as the cord and ganglia of the sympathetic were small, as if atrophied, but without histological changes. All now stated was undoubtedly insufficient to explain the real nature of this interesting affection, especially as the appearances of the "struma exophthalmica"—if we appeal to the familiar physiological experiments of Cl. Bernard—correspond in part to the paralysis, in part merely to the irritation of the sympathetic; while, again, there appears to be no true connexion, as some of these constant phenomena in the pupil have not been noticed.[1] Only in isolated cases has enlargement of the pupil been observed. Stromeyer[2] compares the "exophthalmia strumosa" with the temporary incomplete prominence of the globe, which he had noticed in connexion with habitual spasm of the head (Krampfe des Kopfnickers). When this cramp occurs either from maintaining the erect posture, or from mental emotion, he seeks the foundation of the staring eyes in spasm of the oblique muscles of the eye, and the levator muscles of the eyelids. Demme[3] has also frequently observed with the ordinary goitre partial changes in the pupils, especially "Mydriasis," and a notable elevation of the upper lids. He gives as an anatomical condition at the same time (besides serous swelling and interstitial connective tissue in the recurrent nerve) marked reddening and serous swelling of the sympathetic. These statements, however, suffice just as little as the older descriptions of different changes in the vagus connected with the goitrous condition. Certainly the direction of the inquiry turns, even as in the question of the connexion between disease of the supra-renal capsules, bronze-skin,[4] and other cases, more and more to the nerves themselves; while there still exists too little material for enabling us to arrive at a decision.[5] After all, the question resolves itself not so much into an examination of the cases in their later stages, which can be cleared up by autopsies, but into an investigation of the earliest determining cause.

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[1] Trousseau, Union Médicale, 1860, t. viii. s. 437. Charcot, Gaz. Med., 1856, Sept., p. 584. Gaz. Hebdom., 1862, Sept., p. 564. Corlieu, Gaz. des Hôpitaux, 1863, p. 125.

[2] Von Basedow (Casper's Wochenschrift, 1848, s. 774) mentions the following remarkable facts—that the mammæ of a man were found greatly enlarged, the left being hard, congested, and painful, yielding colostrum.

[3] Handfield Jones, Med. Times and Gazette, Dec. 1860, p. 541. Fletcher, British Med. Journal, 1863, May. (Hyperneurie.)

[4] Graves, a. a. O., s. 410. A. Th. Brück, Casper's Wochenschrift, 1840, No. 28 (Buphthalmus hystericus), 1848, No. 18, p. 275.

[5] Stokes, a. a. O., s. 244.

[6] Köben, De Exophthalmo ac Struma cum Cordis affectione; Diss. inaug., Berol., 1855. Von Gräfe, a. a. O., s. 280. Trousseau, Union Méd., 1860, t. viii. p. 487. Aran, Gaz. Hebdom., No. 49, p. 796. Reith, l. c., p. 522.

[7] Laycock, Edin. Med. Jour., 1863, Feb., p. 681; July, p. 1.

[8] Peter, l. c. p. 182.

[9] Moore, Dublin Quar. Jour., 1865, Nov., p. 348.

At least, there can no longer be any hesitation in acknowledging the intimate nervous dependence of the complex symptoms as the only probable view. With justice has reference been made to the existence of great derangement of the general nervous system, to the loss of sleep, the often noticed epigastric pulsation, the sensation of heat,[1] and, lastly, to a macular eruption occurring on the head after a slight mechanical irritation.[2] In what particular portion of the nervous system the original seat of the disturbance, and what the disturbance itself is, that must first be more accurately established; and the inquiry must be made also, whence, from what source (whether from the blood) the disturbance has been developed. At all events, it is a step in advance to become acquainted with these complex symptoms, and the jeers of M. Piorry are unable to deter us from acknowledging the entity of the disease.[3] In the history of goitre, it forms an episode as remarkable as it is important; for although this variety of bronchocele seems of itself to have little importance, yet it constitutes a part of a grave malady, and one not unfrequently fatal, although, in other circumstances, readily curable.

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[1] Henoch und Romberg, Klin. Wahrnehmungen, s. 182. Reith, l. c., p. 251.

[2] Stromeyer, Handbuch der Chirurgie, ii. 2, s. 389.

[3] H. Demme, Würzb. Med. Zeitschrift, bd. iii., s. 269, 273, 297.

[4] *Bronzing of the skin* occurs in connexion with "struma exophthalmica." It exists in a marked degree on the face of a patient (a man) of Dr Begbie's presently under observation.—(*Translator.*)

[5] Only very recently there died, in my division, a man who had long suffered from very violent palpitation of the heart, with great dyspnœa. His eyes, without being precisely exophthalmic, had an unusual glare (glanz), and gave the impression of being increased in size. A few months previously, Herr Von Gräfe, on account of a commencing glaucoma, had performed iridectomy. Near the close he was affected by dropsy, with very diminished secretion of urine, which was albuminous and rich in uric acid; also with obstinate and violent pain, associated with bloody diarrhœa, great restlessness and fever, with other symptoms. On post-mortem examination I found hypertrophy of the heart, with very extensive myocarditis, a goitre, and very considerable enlargement and interstitial thickening of the sympathetic in the neck, especially of the uppermost and lowest ganglion.

As to the ætiology, there remains little more to be said. According to the observations hitherto advanced, it is by no means in goitre-districts of country that this variety has frequently occurred. Still more does the "struma exophthalmica" plainly constitute one of the most important species of sporadic goitre. Females in a greatly preponderating degree suffer,[4] more so in the early period of life, especially about puberty, and in childbed. Uterine derangements act by no means always as exciting causes; and while serious diseases, such as typhus, and colds particularly affecting the throat, exert an influence, chlorosis is chiefly to blame. Since the latter disease, according to my understanding, is one of early life—is even a disease of development[5]—we are led to assume the existence of an original predisposition. Romberg and Juncken[6] observed the disease in two sisters.[7] We are still further removed from understanding the cause of the disease in men. Exhausting labour, great and long continued depression of the mind, and weakening diseases, have at times preceded it. According to the comparison instituted by Von Gräfe,[8] the disease appears at a later period, on the average, in men, but, at the same time, is more serious. It is worthy of remark that, not unfrequently, the commencement of the malady has been noticed to be quite sudden, for example, after a fright or hard labour.

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[1] Von Basedow, Casper's Wochenschrift, 1840, No. 13, s. 202; No. 14, s. 220. Teissier, Gaz. Méd. de Lyon, 1862, No. 29; 1863, Nos. 1, 2. Trousseau, Gaz. Méd., 1864, No. 12, p. 180. Warburton Begbie, Edin. Med. Jour., 1863, Sept., p. 216.

[2] Trousseau, Gaz. Méd., 1864, No. 12, p. 180.

[3] Piorry, Gaz. Hebdom., 1862, p. 477.

[4] It is not without interest that Rorie (Edin. Med. Jour., 1863, Feb., p. 696) frequently found prominence of the eyeballs in persons of weak intellect, and this particularly in women (35 per cent.); inequality of the pupils, also, he noticed not unfrequently. Foderé remarks, concerning Cretins:—"Aux uns les yeux sont enfoncés dans la tête, aux autres ils sont très en dehors. En général leur régard est fixe et égaré, et il y a toujours un air d'étonnement."

[5] Virchow, Cellular pathologie (3d edition), s. 211.

[6] Henoch, Casper's Wochenschrift, 1848, No. 40, s. 627.

[7] Dr Begbie informs me that, quite recently, he has observed the disease in two sisters, both married. In both the malady had assumed its unequivocal characters.—(*Translator.*)

[8] Von Gräfe, a. a. O., s. 292.

Death results with an increase of the appearances, sometimes very quickly, accompanied by great uneasiness and disturbance of the brain, mostly in a gradual manner, with decay of nutrition and strength, which is hastened by urgent diarrhœa, sometimes dysenteric in character, and mucous catarrh of the lungs. At another time, on the other hand, chiefly in recent cases, a complete cure results; the goitre, however, it is true, not always entirely disappearing. Sometimes the preparations of iron have effected this, sometimes digitalis,—it is seldom that iodine is useful. The best consequences have succeeded the employment of cold-water treatment, sea-bathing, and an invigorating diet.[1]

Observations by Translator.—It is necessary, in the first place, to correct an error into which Virchow has fallen when offering the interesting historical summary regarding "Struma Exophthalmica," with which his observations commence. Parry, he remarks, appears to have been the earliest to observe the prominence of the eyes. The fact is, however, that Parry noticed the enlargement of the thyroid gland in connexion with disease of the heart, but in the whole course of his statement regarding that connexion there is only a single, and that evidently casual, reference to the condition of the eyes. Unquestionably the earliest observer in our own country of the peculiar affection of the eyes, believed by him, at the time, to be a real enlargement, was Dr Stokes, and next in order to him was the late Sir Henry Marsh, of Dublin. Antecedent to 1835, the date of his first publication on the subject, Dr Graves had incidentally had his attention directed to the coincidence of cardiac disease and enlargement of the thyroid gland. In 1839, Dr Begbie had evidently, in a manner altogether independent, noted the association of the three peculiar symptoms. During the succeeding ten years other instances of the kind occurred to him in practice, and, in 1849, he published an account of them. In doing so, Dr Begbie was the earliest in this country to assert the entity of the disorder, to advance a theory of its cause, namely, its dependence on anæmia, to indicate its amenability to treatment, and capability of perfect cure, and, lastly, to suggest a plan of treatment, the success of which has, happily in many instances since that time, been most satisfactorily proved. Five years subsequently to the incidental observation by Dr Graves, of enlargement of the thyroid gland with disease of the heart, a German physician, Von Basedow, published a paper, in which terms now sufficiently familiar in connexion with the disease, were for the first time employed. For example, "Exophthalmos," "Cachexia exophthalmica," and, in the German, "Glotzaugen" (staring eyes), "Glotzaugenkachexie" (staring-eye or goggle-eye cachexia). In these phrases it will be noticed that no particular reference is made to the condition of the thyroid gland, which, in connexion with heart enlargement, had already attracted the attention of Parry and others; but Von Basedow was familiar with the bronchocele as well, and accordingly, in consideration of the correctness of his observation so far, there can be no objection, as has been proposed by Hirsch, and followed by German physicians generally—although scarcely receiving the sanction of Virchow's high authority—to designate the disease "Basedow's disease" ("Basedow'sche Krankheit," "Morbus Basedowii"). The late distinguished and lamented physician of the "Hôtel Dieu," whose unbounded admiration for the character and writings of Dr Graves is as well known as it is highly appreciated by all readers of the "Clinical Lectures," has styled the disease, "Graves' disease" ("Maladie de Graves"), and whether rightly or wrongly so—Virchow, as we have seen, thinks wrongly—there can be no doubt that under this name, as originally employed by Trousseau, it will long be familiarly known and described. Were it not that I entirely agree with the late Dr Todd, of London, in regarding it as no compliment to the great names of our profession "to attach them to any of the numerous ills which flesh is heir to,"[1] I should feel disposed to suggest the appellation of "Stokes' disease." Such, too, is a sufficient reason for not encouraging the use of that name which my filial respect had otherwise most cordially approved, to wit, "Begbie's disease," as already proposed by more than one writer. There is little to be said in favour of the other nomenclature adverted to in the translation, the "Tachycardia strumosa," or "Tachycardia exophthalmica strumosa," of Lebert, and the "Cardiagmus strumosus" of Hirsch and Von Dusch; and it must be confessed that a really good and serviceable title for the disease is still a desideratum.

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[1] I may take the opportunity of directing attention to a brief but very interesting account of the disease by a physician of Heidelberg, Dr Theodor Von Dusch, in his recently published volume, entitled "Lehrbuch der Herzkrankheiten," Leipzig, 1868.

Dr Von Dusch's observations on the "Basedow'sche Krankheit" are illustrated by a woodcut representing an example of double exophthalmos, but without goitre, in a man of thirty-two years. The portrait, in the first instance, was photographed from nature.—(*Translator.*)

Like other observers, it is worthy of note, that Virchow indicates no special form of disease as incident to the thyroid gland in this complex disorder. The most marked or characteristic feature is its increased vascularity, and the peculiar pulsation, or pulsatory thrill, which is, at all events, distinguishable over it. Hence the application to this variety of goitre of the terms "vascular bronchocele" and "struma aneurysmatica." While increase of gland structure, and various kinds of degeneration, as, for example, the fibrous and cystic, have been detected in such goitres, there is no doubt whatever that the augmented activity and chronic enlargement of the vessels, particularly the veins in them, is of chief importance.

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[1] In referring to facial palsy as "Bell's paralysis," in Clinical Lectures on Diseases of the Nervous System, Lecture 4.

The central organ of the circulation invariably and, in point of time primarily, suffers. At first, however, and usually for a lengthened period, the heart is only functionally disturbed. Of this disturbance its greatly accelerated action is the prime, as it is the unmistakable, indication. After a time, structural change ensues, and that is, for the most part, of the nature of dilatation, or hypertrophy with dilatation. The left ventricle is the chamber chiefly affected. Valvular disease of the heart is certainly rare ; when it exists, the imperfection is secondary, never primary. In other words, the increased size of the mitral and tricuspid orifices, discovered on the post-mortem examination of some cases of the disease, bore an intimate relationship to the greatly augmented capacity of both ventricles. This, the earlier result, was evidently the cause on which the insufficiency of the auriculo-ventricular valves depended.[1]

In the great majority of cases, a disturbed action of the heart, generally spoken of as palpitation, is the *first* symptom to attract attention. If, however, a very careful inquiry into the previous history of such patients be made, it will be found that for some time before the distressing action of the heart was noticed—possibly at a considerable period antecedent to the acknowledged existence of any departure from health—there had occurred a diarrhœa or lientery, a menorrhagia or leucorrhœa, an epistaxis, rather a frequently recurring loss of blood from the nose, or a hæmorrhage from piles ; and although, in many cases, none of these may have been sufficient to produce the more manifest indications of anæmia, it will, I firmly believe, be further found that impoverishment of blood—readily enough recognisable—exists.

Virchow, like all recent observers of this disease, rejects the notion of the prominence of the eyes being due to "hydrophthalmos." He looks upon a change in the fatty tissue of the orbit—a view originally maintained by Heusinger—as being the essential morbid condition upon which the very strange aspect of the eyes depends. An hypertrophy of the fatty tissue, or its congestion, are the states more particularly indicated. Enlargement of the spleen is referred to by Virchow as having been noticed by some observers, but he truly remarks that the degree of importance to be attached to its occurrence has not been as yet accurately determined.

There are then, it may be stated, *four* essential symptoms in this most interesting malady — namely, 1. Disturbance of the Heart's action, prone to terminate in structural change ; 2. Enlargement—vascular in its nature—of the Thyroid gland ; 3. Prominence, with peculiar expression, of the Eyes; and, 4. Remarkable visible pulsation and vibratory thrill throughout the whole Arterial system. Of this "tetrad" of symptoms, the first and fourth are always present; the second and third may each be absent. Without cardiac and vascular derangement, the disease has no existence. An exquisite illustration of the malady, however, presumes the presence of all the features now mentioned.

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[1] See, for example, the case of J. K., as recorded by Dr Begbie, in "Contributions to Practical Medicine," pages 143, 148.

In discussing the essential pathology of "struma exophthalmica," Virchow has justly observed that anæmia, granting its existence, cannot directly produce the results which are witnessed. While the "primum mobile" is, however, seated in the blood, it is abundantly evident that an injurious influence is largely exerted on the nerves. A decided advance in our knowledge on this point has recently been made, for Peter, Reith, and Von Recklinghausen have each discovered a distinct lesion of the sympathetic nerve and its ganglia, while Virchow himself so far confirms their interesting and probably important observations as to have found in a case not exactly of the "struma exophthalmica," but bearing a certain resemblance to it, a lesion of the same nervous trunk.

Further observations are, however, required in order to determine, with any amount of accuracy, what value is to be attached to these morbid appearances in relation to the intimate pathology of that strange disease with which they have been found connected.[1]

It cannot be too distinctly stated, nor too carefully borne in remembrance, that, in this disease, *iodine* is an unsuitable remedy—its administration, so valuable in ordinary goitre, is in the "struma exophthalmica" not only useless, but injurious. From *iron*, *digitalis*, and *belladonna*[2] discriminately employed, and from the steady perseverance with an invigorating plan of treatment, the best effects have been found to follow.

ARTICLE III.—*On the Therapeutical Action of Medicines in Dilated Conditions of the Bloodvessels—Aconite.* By ARCHIBALD REITH, M.D., Physician to the Royal Infirmary of Aberdeen.

IN a former communication I endeavoured to point out the analogy between the incipient steps of the inflammatory process and those of medicinal agents, with the practical conclusions suggested thereby. I stated that, if the double property of contracting and dilating the bloodvessels—in other words, of stimulating and paralyzing the sympathetic—be characteristic of medicines in general, in morbid conditions, where dilatation was a prominent symptom, the medicines, acting physiologically on the affected part, would be the proper remedies; and, if given in sufficiently small doses, would, by contracting the dilated vessels, tend to alleviate or cure the disease.

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[1] I have observed a decided feebleness in the lower extremities, almost amounting to paraplegia, in two or three aggravated cases of the disease. This also points to the implication of the nervous system. In two of these instances, the patients walked with considerable difficulty, and had acquired a peculiar rotatory movement in progression.

[2] On Vascular Bronchocele and Exophthalmos, by the translator; Edinburgh Medical Journal, 1863, page 217.

I stated also that, in accordance with the experiments of physiologists, the vaso-motor system, when paralyzed, was more susceptible of stimuli than in the natural condition; and that, in consequence, only very small doses of medicines having an affinity for the affected organ were required to contract the unduly dilated vessels.

I now proceed to bring the theory to the test of experience. In doing so, I shall confine myself entirely to my own observations, which, though at present limited, are sufficient to warrant me in laying them before the profession. I could not hope, in a reasonable time, to collect such a mass of facts as would carry instant conviction to every sceptical mind. My observations, therefore, though satisfactory to myself, may not be so to others; and the utmost I expect, meantime, is that the ice being broken, my experience will be taken as a groundwork of further investigation, and either controverted or established.

One of the first medicines on which I experimented was *Aconite*, to the therapeutical application of which, in accordance with the foregoing theory, I shall devote this paper. I do not intend to touch upon all the remedial uses of this drug, but merely to show its great value in one morbid condition of the system, namely, the febrile state. Before doing so, however, it will be proper to notice some of the more prominent characteristics of aconite, as recognised by the latest authorities who have made it the subject of their observations.

In my last paper I stated that medicines, viewed in relation to their effect on the bloodvessels, might be divided into two classes—the first consisting of those which merely induce contraction without subsequent dilatation, the pure astringents, acting chemically on the tissues; and the second, much the larger class, comprising those whose primary action of contraction (stimulus of sympathetic) is followed, sooner or later, by a corresponding dilatation of the vessels (paralysis of sympathetic). Passing by the former for the present, we find that the second class, those possessing the afore-mentioned double property, may be subdivided into two sections:—1. The medicines whose primary stimulating action on the sympathetic (contracted vessels) is prolonged and decided, and whose influence must be partially or wholly removed before re-action takes place (dilated vessels and paralysis of sympathetic); 2. The medicines whose primary action of spasm is momentary, scarcely, if at all, perceptible, though intense, and is followed, during the continuance of their influence, by very manifest and decided dilatation of the bloodvessels (paralysis of sympathetic). Cold is an example or type of the one class, heat of the other. Perhaps the greater number of drugs belong to the latter class, manifesting considerable variety in the intensity and duration of their temporary stimulant action on the sympathetic, but resembling each other in the shortness and comparative imperceptibility of the stimulus. Aconite, on the other hand, is an example of the former class of medicines. The analogy between its poisonous action on the system and the effects of cold are very striking. The prolonged contraction of bloodvessels with the numbness characteristic of cold, when applied to the body, have their counterpart in the phenomena exhibited by aconite. Hence these two agents have hitherto occupied a place in the Materia Medica different from most other remedies. I stated formerly that what were known as the physiological or toxic properties of medicines were in reality their secondary effects, being a reaction from a previous, often invisible, or, at least, unobserved state of spasm of the bloodvessels. In experimenting with drugs, observers had overlooked this primary condition, and had wholly concentrated their investigations upon the secondary condition of dilatation. The majority of agents belonging to the second class, mentioned a little ago, their primary action is comparatively of short duration, and it is not surprising that it should have escaped attention. With aconite and cold the case is reversed. The primary effect of these agents, that of contracting the bloodvessels and exciting the sympathetic, is so decided and prolonged that it has constituted their main feature in descriptions of their properties. Thus the recognised physiological properties of cold and aconite are due to their primary action, those of other remedies to their secondary. It is easy to see what confusion this has introduced into therapeutics. The double action of medicines has been entirely disregarded; in some the primary, in others, such as aconite, the secondary phenomena have been overlooked. Both these actions, however, must be taken into account before we can form a correct estimate of the therapeutical value or application of remedial agents.

The chief brunt of aconite falls on the nerves of the heart. When taken internally, although other organs of the body come under its influence, its main action is upon the centre of circulation. If we could suppose Dr Richardson's ether spray directed on the heart, it would give a fair idea of the effect of aconite upon that organ. Its mode of operation seems to be to cause excitement of the cardiac plexus with consequent contraction of the bloodvessels supplying the tissue of the heart. There is a diminution of blood in, and a decrease of the vital properties of, the organ. Its pulsations become less frequent, and may fall so low as forty per minute. The surface of the body becomes cold; there are rigors, shiverings, cramps, denoting defective circulation in the system. The general condition of the patient in this state resembles the collapse of cholera. In fact, it is a true collapse, induced by a poison, as much as is that of cholera. The sympathetic system is in a state of excitement, and the bloodvessels in a state of contraction. If a moderate dose of the poison be taken, the symptoms induced are those of the chilly stage of a fever, a less advanced degree of collapse. These, then, are the recognised physiological effects of aconite on the circulation. The attention of observers seems to have been arrested at this stage. Any further phenomena, characteristic of the medicine, appear to have been totally overlooked, or, at least, considered unworthy of special notice. Whereas the truth is, that the subsequent manifestations of aconite on the circulation are equally important with, if not more so than, those just mentioned. In all cases of collapse, unless the influence be strong enough to extinguish life in that stage, there is of necessity a reaction to the opposite condition. So much is this recognised, that it is the practice in India to abstain from all interference with cases of cholera during the stage of collapse, and to wait for the reaction which is certain to occur in the majority of the patients. The chilly stage, also, of a fever is succeeded by the febrile stage. In like manner, the primary symptoms of aconite just detailed are followed by a corresponding reaction. In proportion to the stimulus exerted upon the cardiac nerves, more or less paralysis ensues. The bloodvessels of the heart are preternaturally dilated, more blood is sent to its tissue, and there is a consequent increase in its vital properties. The pulsations are increased in frequency and strength, the circulation is more active, and there is consequently a febrile condition present. Had our nomenclature been consistent, this aconite-fever should have been regarded as the physiological action of the drug, and brought it under the same category with other medicines whose secondary symptoms have been considered their proper physiological manifestation. If the duplicate phenomena, however, be admitted, the nomenclature is of little importance. Now, these febrile symptoms exhibited by aconite, though acknowledged by the profession, are nevertheless practically ignored. They are never mentioned as affording any indication of its therapeutical application. All our ideas of aconite have hitherto been confined to its primary action as much as they have been to the secondary action of other medicines. But, in possessing a double property, its use in disease is rendered more intelligible and accurate.

It is unnecessary, I think, to enter more minutely into the influence of aconite on the circulatory system. Its action is so well known, that any detailed account would be superfluous. As I intend to confine my remarks to its remedial uses in the febrile state, it is equally unnecessary for me to enter upon its effects on other parts of the economy.

The secondary action of aconite being attended with dilatation of the cardiac bloodvessels and quickened circulation, it follows that, in accordance with the views formerly expressed, it should prove more or less remedial in conditions of the system where such dilatation existed—the dose, of course, being small enough to induce only the primary effects. The febrile state is that for which aconite is specially indicated. And herein is exemplified the advantage of recognising the double properties of medicines. Aconite, viewed solely as a depressor of the heart's action, that is to say, a stimulant of the cardiac plexus, has been strongly recommended by some physicians in various kinds of febrile disorder. But overlooking the secondary action of the drug, and guided only by its primary, they have prescribed it on the principle of the larger the dose the more powerful the effect. The experience of its use in this way is very diverse. Sometimes it has succeeded; but it has as often failed, and in many cases dangerous symptoms have occurred. Sir James Simpson says, "One medicine has of late been supposed to produce this sedative effect on the heart, without exerting any other appreciable therapeutic action. I allude to aconite, a strong tincture of which has been employed by various European surgeons in surgical fever; but it is difficult to regulate it in its effects, and in many persons it produces an almost dangerous and depressing effect, even in very small doses."[1] Aconite has therefore not come into general use as a febrifuge. But when the principles are applied, which I endeavoured to expound in my last paper, we obtain the true conception of its therapeutical application. The two following points may be noticed:—

1. In the first place, the secondary action of aconite being analogous to the febrile state, we infer that it will prove remedial in cases of simple uncomplicated fever only.

2. In the second place, the effect desired being to produce moderate contraction of the cardiac bloodvessels such as will not be followed by undue reaction, the dose of aconite must be correspondingly small; and on account of the extreme susceptibility of the paralyzed sympathetic to stimuli, care must be taken to regulate the dose accordingly.

The first proposition excludes aconite almost entirely from the domain of specific fevers (except, perhaps, rheumatic fever, of which I shall speak presently). I have used it extensively in typhus and enteric fevers, without the least advantage. Sometimes I found the pulse to come down a little, but it invariably rose afterwards. In the majority of instances no impression whatever was produced on it. It is therefore vain to expect any good from the employment of aconite in such cases. I should think, also, that in other specific fevers, such as measles, scarlatina, smallpox, or erysipelas, the same negative results would be obtained; but I have not tried it in these cases. Mr Liston spoke highly of it in the last-named disease, but his example does not seem to have been followed. Belladonna is so beneficial that I have not resorted to aconite when treating erysipelas. My experience of the latter medicine in the two continued fevers I have mentioned is decidedly unfavourable, and confirms the impression I had formed from its secondary action, that it would be useless in fevers depending on the presence of a specific poison in the blood. Then, again, even in inflammatory fever, for which it has been highly extolled, and with reason, no impression will be produced on the symptoms *if there be extensive local lesion.* In such a case, the medicine acting physiologically on the affected part is the remedy properly indicated, whether it be belladonna for meningitis, antimony for pneumonia, or cantharides for nephritis. Aconite may and does prove very serviceable in conjunction with these, but by itself it will probably fail. Its use must therefore be limited to simple fever without local complication.

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[1] Medical Times and Gazette, 1859, vol. i. p. 516.

Next, as to the dose. There can be do doubt that the conflicting testimony regarding aconite is in great measure due to the quantity prescribed. Forgetting the febrile reaction which takes place, the tendency among observers has been to use gradually increasing doses. Finding injurious effects produced, they went backwards; only, however, to a certain extent. In some cases even small doses were found capable of acting injuriously. The medicine was consequently abandoned altogether. It never seems to have struck any one that, by still further diminishing the dose, more accurate results would have been obtained. On the contrary, the idea has been taken up, as with many other medicines, that unless benefit were derived from the doses of the Pharmacopœia, it was contra-indicated. The old-fashioned phrase of "not agreeing with the patient," became the usual house of refuge. Now, I beg to call attention to the following fundamental axiom of therapeutics, which has never, so far as I am aware, been stated before. It is this:—*Medicines, whose recognised physiological action is in reality their secondary manifestation, when indicated by the pathology as proper remedies for a particular disease, are unsuitable for, and injurious in that disease, when given so as to develop the physiological symptoms.* We have a notorious example of this in quinine, which "disagrees" with so many people. The disagreement is merely the development of its secondary or physiological action. If it be the proper remedy for the patient's ailment, the dose ought to be reduced. If one grain disagree, less and less ought to be given till the proper quantity be found. Many a case of this kind have I met with, and many a case of dyspepsia have I cured with that very quinine which the patient fancied he could not take. What is called idiosyncrasy is, in nine cases out of ten, merely greater susceptibility to the action of medicines; and if the dose were regulated in accordance with the foregoing principles, these peculiar abnormities would be less frequent. In the case of aconite, the physiological action being primary, we are merely called upon to deal with an excess of that action as manifested in the experiences of those who have used it. They find that, in febrile cases, even five minims of our tincture, or three minims of Fleming's, will often cause alarming symptoms. Such a dose would have little effect on a healthy subject. But in conformity with that morbid susceptibility which I have repeatedly insisted on, in the febrile condition it becomes excessive. Why, then, not diminish it? This is the very point where experimenters have failed. It seems singular that when they found five minims to depress the heart too much, they did not try four, three, two, or even one. If they had done so, the value of aconite would have been by this time universally acknowledged.

The preparation I used in my investigations was the common tincture of the British Pharmacopœia. Keeping in view the theory on which I was experimenting, I gave much less doses than the minimum of the prescribed standard. I found then, that to answer all the purposes for which aconite was indicated, it was not necessary to give more than one drop, nor less than a quarter of a drop. On three occasions I found even this latter quantity to act too powerfully. It may be satisfactory to give these cases, the symptoms being alike in each.

A gentleman, aged 30, who had a feverish catarrh, took, according to my prescription, one quarter of a drop of tinct. of aconite, in water. In a few minutes faintness came on, the pulse became very feeble, and there was a feeling of loss of power in the left arm. Brandy was administered, and recovery gradually took place. This patient is liable to feverish colds, but he never takes more than one-eighth of a drop of aconite, which he finds invariably to act as a specific.

A lady, aged 28, in similar circumstances, took a quarter of a drop of the tincture. Soon afterwards her pulse fell very low, and this symptom was attended with powerlessness in the left arm.

A lady, aged 31, suffering from febrile catarrh, took tinct. aconiti gtt. $\frac{1}{4}$. In a few minutes similar symptoms came on as in the other two cases. On two succeeding occasions I prescribed the same quantity of aconite, with the same results.

With these exceptions, then, the remedial dose of aconite I have found to lie between the quarter and the full drop. It may be given in water simply every half-hour, every hour, or every two hours, according to circumstances—always, of course, upon the empty stomach. This quantity may, at first sight, appear absurdly small, and to approximate the homœopathic infinitesimals. It is not, however, a great reduction from the minimum dose of the Pharmacopœia (5 minims), which is confessedly too energetic in many cases. There is a wide difference also between gtt. $\frac{1}{4}$ as a minimum dose, and the thousandth or millionth part of a drop given by Hahnemann, in accordance with his theory of potentization. I do not know the strength of the homœopathic preparations of aconite, nor the relation they bear to our tincture; but it is plain enough, from homœopathic publications, that the more intelligent and enlightened members of Hahnemann's school are giving up the theory of their founder, and prescribing doses more consistent with reason, although still on the principle of *similia similibus curantur*. The spread of Homœopathy is due to the powerful effect of small doses of such medicines as aconite. Separate it from infinitesimals, and there is little fault to find; but an illogical public reasons in this way:—"Aconite cut short my cold, *ergo*, Homœopathy is right." The more intelligent method is certainly to reason, as a gentleman did who, having been cured of a feverish cold in a few hours by some aconite given to him by a homœopathic friend, said to me:—"It convinced me that aconite was a very powerful medicine, but it did not convince me that Homœopathy was true." Now, if the middle position were adopted, which I have indicated in these papers, the present homœopathic system would not have a leg to stand upon. The doses I recommend are what come short of reaction, whether it be 20 grs. of bromide of potassium, or one-fourth of a drop of tincture of aconite. The connexion between this and infinitesimalism is certainly very slight.[1]

I come now briefly to indicate the applications of aconite as a remedy for febrile disorders, in so far as I have made it the subject of personal observation.

I. *Catarrhal Fever.*—We possess in aconite a medicine which quite supersedes the purgatives and antimonials with which patients are too often drenched, *usque ad nauseam.* By purgatives the dilatation of the cardiac bloodvessels, or paralysis of the cardiac plexus, is sought to be relieved, by creating dilatation of the abdominal bloodvessels, or paralysis of the solar plexus. With antimony it is the same; acting by preference on the stomach and lungs before the heart, it lowers the pulse only at the expense of the first-named organ, and after a large dose. Aconite, on the other hand, acting directly on the heart, controls its force prior to any influence exerted on other organs, and therefore, in quantity insufficient to operate elsewhere, lowers the pulse without the weakening effect of purgatives, or the nausea and unpleasant consequences of antimony. It is scarcely possible to speak of it too highly as a remedy for febrile catarrh, in the doses recommended above. For this it acts like a specific. It will not cure the attendant coryza, or tonsillitis, or bronchitis; but a few oft-repeated doses will, almost to a certainty, bring down the pulse, rendering it softer and less frequent. Its operation is usually attended with more or less perspiration, and followed by the removal of the oppression and restlessness under which the patient laboured. In order to save time, I have frequently administered the medicine myself, and have, before leaving the room, witnessed the good effects of a single dose. It is quite common for me to hear parties express themselves as greatly relieved a few minutes afterwards. I have frequently had occasion to use it myself, and it has always within ten minutes removed the distressing feelings. A second supply was never required. In the majority of cases, three or four doses are sufficient. Indeed, if within twelve hours no improvement takes place, it is to be inferred that there is a specific poison at work; and hence aconite forms a diagnostic element. In not a few instances I have predicted the outset of typhus or enteric fever from the failure of aconite to bring down the pulse.

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[1] I find that many of the enlightened and intelligent homœopathic practitioners are of the same mind. They are prepared to abandon Hahnemann's system, and to occupy a middle position such as I have indicated, did they receive encouragement from our side. Would it be too great a concession to give them that encouragement? We are fully aware that Homœopathy contains an element of truth: shall we continue to reject that element merely because Hahnemann buried it in so much rubbish? I cannot believe so; and I think that the recognition of the double action of drugs might serve as a basis on which the two opposing schools could ultimately be reconciled—a consummation devoutly to be wished.

It would be impossible, within the limits of a short paper, to give even an abstract of the numerous catarrhal cases treated in this way and cured. Their number is legion. I shall therefore give only a few as examples of the whole.[1] They do not differ materially from each other, and it would be an unnecessary repetition to detail every one.

CASE 1.—A. G., female, aged 50, had suffered from general restlessness and feverishness for several days. When I saw her, the pulse was 120, the skin hot, and the tongue furred. She complained very much of "pain in her bones." I gave her at once gtt. $\frac{1}{4}$ of tincture of aconite in water. In five minutes, before I left the room, she felt very much relieved, and expressed her astonishment at the rapid effect. Before morning (it was 8 P.M. when she first took the aconite), she was quite well, and able to be up.

CASE 2.—H. M., female, aged 13, was seized with rigors and general coldness on a Sunday afternoon, followed in the evening by great feverishness, headache, and oppression. I prescribed gtt. $\frac{1}{4}$ of tinct. aconiti every two hours, to be suspended if she felt better after one or two doses. She did not require to take it more than twice, and before morning she was well.

CASE 3.—A. B., male, aged 60, had smart coryza, with great restlessness and general oppression. He had been in a "burning fever" the whole of the previous night. He was ordered to take gtt. $\frac{1}{4}$ of tinct. aconiti during the night, at short intervals. A few doses relieved the symptoms, but without perspiration, and in the morning he was well.

CASE 4.—I was called at midnight to see a child, aged 2½ years, said to be attacked with croup. I found him in a strong fever, with flushed face, swelled tonsils, and great dyspnœa. The croupal symptoms had lasted for more than an hour, but the child had been feverish and unwell for several days. On the first appearance of the stridor and hoarseness the parents had very properly put the child into a warm bath, and applied a poultice to the chest. I ordered mustard to the throat, and gtt. $\frac{1}{8}$ of tinc. aconiti every hour. I was so satisfied that this was more than laryngismus stridulus, that I told the parents to call me at 7 A.M. if the child was not better. I had some misgivings about not giving the usual antimonials, and determined that, should the aconite fail, as little time should be lost as possible. I was not sent for; and when I called in the morning I found the child quite well, free of stridor, and hoarseness, and fever. He was, in fact, amusing himself with his toys. I was told that the alarming symptoms had subsided about 4 A.M. Query, Would antimony or any other medicine cut short threatened croup so effectually as aconite did in this case?

Aconite is also very useful in the chill which precedes the fever.

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[1] I intend to publish separately a full list of all my cases, but wish to compare them with the experience of others. In the text, therefore, I only give examples.

I have met with several cases of this kind. One, a young man, sat shivering over his fire for an hour before he took the medicine. In twenty minutes the shivering ceased, as also the malaise which accompanied it. I infer that the aconite prevented the usual reaction which would probably have taken place.

Notwithstanding what I have stated, aconite does sometimes fail, even in catarrhal fever. Success, however, is the rule; want of it the exception. The reason of the exception I have not been able to make out; but it ought to be regarded, perhaps, as a confirmation.

II. *Rheumatic Fever.*—Aconite has been recommended by many for acute rheumatism, but very conflicting accounts are given regarding it. By some it is extolled highly; by others as much depreciated. Lombard of Geneva, who used it extensively, gives the following conclusions:—

1. The alcoholic extract of aconitum napellus is endowed with a specific action against acute articular rheumatism.
2. It speedily dispels the pains and the swelling, and dissipates the effusions of synovia in the articulations affected by acute rheumatism.
3. The medicine does not act as a derivative on the intestinal canal or on the skin.
4. Given in large doses, it produces a strong stimulus on the brain, and appears to modify the circulation.
5. The alcoholic extract of aconite contains the active principles of the substance, at least as regards its anti-rheumatic properties.[1]

Fleming and Neligan in our own country have followed Lombard in their appreciation of aconite in rheumatic fever. All these observers, however, used gradually increasing doses; and this is probably the reason why, as I explained a little ago, conflicting results have been obtained, preventing the general adoption of their practice. My experience of aconite in rheumatism is very favourable. The small doses are unattended with the risks of larger ones, and produce all their good effects. I always trust to aconite for the reduction of the pulse, and it seldom fails, although it does not operate so speedily or so effectually as in catarrhal fever. For there is undoubtedly a poison in the system, and its presence there must necessarily interfere with the free play of aconite on the circulation. But the rheumatic poison does not resist aconite so completely as other febrile miasmata, and consequently the remedial effect of the medicine is far from being antagonized. While the symptoms in catarrhal fever ought to be relieved within a few hours, twenty-four at the most, I am satisfied if, in rheumatic fever, the pulse come down in two or three days from the time of administering the aconite. The utmost I expect from this is, not the instant cure of the disease, as some have hoped, but the protection of the heart from complication, and, by the quietness of the circulation, giving full advantage to the excretory organs to eliminate the morbific element. This is all that aconite will do, and it is a great deal. It prevents us being driven by the entreaties of the patient to that most mischievous drug, opium, and so saves the sufferer weeks of misery. It shortens the duration of the fever, if it does not arrest it. It is an invaluable auxiliary to the alkaline and blister treatment, and by itself alone will often save the necessity of resorting to either. The following cases will serve as illustrations:—

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[1] Trousseau and Pidoux, Traité de Thérapeutique, 7th ed., tom. ii. p. 119.

Case 1.—J. F., æt. 21, male, affected with acute rheumatism. Pulse 120, very strong and full; knees very much swollen, especially the left; ankles painful and red. Ordered gtt. ¼ of tinct. aconiti every two hours. Next day the pulse had fallen to 80, and the swelling of the knees had almost entirely disappeared. This patient's illness lasted a week. He had been ill three days before I saw him; he was relieved of the more severe symptoms in twenty-four hours, and in two days more he was comparatively well. Cases of this kind are frequently met with, where aconite alone suffices to effect a cure.

Case 2.—E. A., female, aged 28, a servant, was admitted into the Infirmary on the 2d January. She had been ill for a week with rheumatic fever. When seen, all the joints of the limbs were swollen and painful, the two wrists and the right knee being most affected. Pulse 120, full and strong. No cardiac complication. She was ordered gtt. ¼ of tinct. aconiti every two hours. The pulse fell at the end of two days, and her general distress was much alleviated, but the joints, especially the wrists, continued painful. The blister treatment was then adopted for the wrists and right knee with complete relief, and she rapidly recovered. The pulse never rose after it had been reduced. This case is a good illustration of the failure of aconite by itself to cure the disease, but that it is very beneficial in conjunction with other remedies. Perhaps if it had been prescribed at the outset of the fever, it might have been successful alone.

The next case is an example of the complete inefficacy of aconite in severe local complications.

Case 3.—A. R., female, æt. 18, admitted to the Infirmary on 9th January, suffering from rheumatic fever of several days' duration. She had formerly had the same disease about two years ago, and was then under the care of Dr Smith. The heart was affected at that time. On admission, the knees and ankles were found most affected, and there was a double murmur over the aortic valve, and apparently a to-and-fro sound over the apex. A few days afterwards, pericarditis was fully developed, and ran its usual course. From the first she took gtt. ¼ of tinct. aconiti every two hours, without the smallest impression on the pulse, and it was not till after smart blistering that it began to fall. This is the only case of the kind I have met with for some years, as very few of the patients suffering from rheumatic fever are attacked with cardiac complications under the improved treatment now in vogue. It was not a case where aconite might be expected to do good, and larger doses than what I gave might have proved too depressing.

In mild cases, then, of rheumatism, aconite will in most cases lower the pulse and very materially shorten the disease, without further medicinal agency. In more severe cases, while it will prove serviceable in controlling the heart's action, it will be necessary to give it in conjunction with the full alkaline or blister treatment. In carditis, it will probably prove ineffectual.

III. *Inflammatory Fever.*—Aconite has been highly recommended for the fever symptomatic of inflammation. As in rheumatic fever, however, it has been pushed to an extreme, and disappointment has consequently been experienced. So far as I can at present judge from my investigations, it has a certain range of application, but, as I said before, it has no place when there is extensive local lesion. The medicine then indicated is the one acting physiologically on the affected part, such as ipecacuan and antimony in bronchitis and pneumonia. The true place of aconite is at the onset of local inflammation or the congestive stage. I can truly say from experience that it is then most beneficial. It will often prevent the mischief from fully developing itself, producing all the effects formerly said to be derived from bleeding, without the loss of blood. I speak chiefly of its influence in pneumonia, where I have tried it most frequently, the opportunities I have have had of testing it in other inflammations being as yet too few to enable me to speak positively as to its value. The following cases will serve as specimens of the behaviour of aconite in pneumonia, etc.

Case 1, *Pneumonia.*—A. B., aged 35, an inmate of the Infirmary, suffering from bronchitis, with copious muco-purulent expectoration. A few days after admission, feverish symptoms came on with pains in the right side: pulse 120, full, slight dulness over the right base, with crepitant râle; rusty sputa. Ordered gtt. $\frac{1}{4}$ of tinct. aconiti every two hours. Next day the pulse had fallen to 80, the sputa had become natural, and the crepitant râle was replaced by fine bubbling. This is a case not very frequently met with, where pneumonia is found in its first stage. The action of aconite was very decided; and the improvement obviously cannot be set down to the usual hygienic influence of the hospital on a patient newly admitted, for the pneumonic symptoms supervened a week after he had been placed in a comfortable bed. Cases of this kind are met with mostly in private practice, and the disease detected at once. Aconite, then, in this stage takes the place of the lancet.

But the more common cases are such as the following:—

Case 2, *Pneumonia.*—C. D., aged 40, admitted to the Infirmary affected with pneumonia. Dulness over base of right lung, with tubular breathing; rusty sputa; pulse 120, full. He took, according to order, gtt. ¼ of tinct. aconiti every two hours. Next day the pulse was 80; and there was copious perspiration over the body. In three days the tubular breathing was replaced by moist crepitus, and the sputa were purely bronchitic. Cases advanced to the stage of hepatization are not so successfully treated by aconite alone, as in the following instances.

Case 3, *Pneumonia.*—B. M., female, aged 18, affected with pleuro-pneumonia. I was not called to see her till twelve hours after the onset of the symptoms, when hepatization had taken place in the right lung. Pulse 120; sputa of the prune juice character; breathing very much oppressed. She took aconite in drop doses so long as the pulse remained rapid, but it never made the smallest impression on it. It was at first given alone, and then in conjunction with antimony. The pulse at the end of a week stood one evening at 140, and the symptoms looked unfavourable. Next day, however, the pulse had fallen, and by degrees the patient recovered. Her recovery I believe to have been purely natural, and in nowise retarded or accelerated by the medicines employed.

Case 4, *Pneumonia.*—J. M., female, aged 22, affected with pneumonia of the right lung. The symptoms were the same as in the above case, and were entirely uninfluenced by aconite or antimony. Delirium supervened, and death occurred on the sixth day.

These cases well illustrate my experience of aconite as a remedy in pneumonic fever. I have found it, as a rule, useful only in the very earliest stage.

Case 5, *Pelvic Inflammation.*—J. A., female, aged 35, of a strong hysterical temperament, admitted into the Infirmary 9th December 1866. Since the birth of her last child, about fourteen years previously, she had been the subject of chronic enlargement of the uterus, and has undergone an immense variety of treatment. She became so familiar with the names and properties of medicines, and their effects upon herself, that I could seldom deceive her. On a former occasion she had been affected with pelvic inflammation, not, however, ending in abscess. On admission, the resident physician diagnosed the case to be one of peritonitis; there was high fever, severe pain at the lower part of the right side of the belly, and tenderness on pressure over the whole abdomen; tongue dry and brown. When I saw her, I found considerable fulness in the right hypogastrium, and also *per vaginam*, I detected a swelling at the upper part of the right side, without hardness or fluctuation. The symptoms had come on in consequence of sudden suppression of the menses, caused by over-exertion during washing. Poultices and fomentations were ordered to the belly, and drop doses of the tinct. aconite every two hours. In twenty-four hours, the fever had subsided so far that the patient, with her usual curiosity, desired to know the name of the medicine which had done her so much good; and a few days after, when fever had been rekindled, she begged for the same thing again, which was given, with the same effect. The local effusion caused a good deal of trouble, but eventually it completely disappeared.

I have seen one or two similar cases, though not so severe.

I have also tried aconite in other inflammations, but without much apparent benefit. Perhaps if the cases had been seen early enough, it might have succeeded, but it is seldom that medical advice is sought at the first commencement of illness. In mild cases of tonsillitis, coryza, gum-boil, etc., aconite has acted against the fever like a charm, but in more severe inflammations, such as nephritis or pneumonia, it has generally failed. Further observation may disclose its true position in these cases.

IV. *Nervous Shock.*—The influence of aconite on the reaction from nervous shock is equal to, if not more marked than its effect upon catarrhal fever. Numerous cases have come under my notice, of which the following are examples:—

Case 1, *Concussion of Brain.*—W. G., æt. 4, a boy, was knocked down by a spring-cart, the wheel coming into violent contact with his head. He was completely stunned, and brought home insensible. I saw him in the evening. He was then quite unconscious, but in a state of high fever. I prescribed nothing then, but gave a few general directions for his care during the night. In the morning he was no better, if anything worse; the fever was intense, the skin hot and burning, and he was lying in a state of partial insensibility, from which he could with difficulty be roused. I gave him gtt. $\frac{1}{8}$ of tinct. aconiti, to be repeated at the end of an hour if necessary. The first dose acted like a charm: in five minutes there was a perceptible difference, and in two hours he was so much better that his mother thought it unnecessary to continue the medicine. He had taken but two doses. In the evening of that day I found him well.

Case 2, *Concussion of Chest.*—R. B., æt. 8, a boy, received a smart blow on the chest from a stone. When seen three or four days afterwards, he was in a high fever, and complained of pain in the part where he was struck. He did not tell of the accident till a few days after I saw him, when he confessed to his nurse that a boy had thrown a stone at him. I apprehended at first some specific fever, but, as soon as I was told of this, I gave him gtt. $\frac{1}{4}$ of tinct. aconiti. In a few hours all fever had disappeared and he was quite well.

Case 3.—Mrs T., æt. 30, confined of her first child. The labour was severe, but I terminated it within twelve hours by the forceps, as the outlet presented an obstacle to natural delivery. The shock was severe, and I dreaded reaction, but having taken gtt. $\frac{1}{4}$ of tinct. aconiti every two hours, she rallied well, had no fever, and made an excellent recovery.

Case 4.—Mrs G., æt. 22, confined of her first child. It was a footling presentation, and, as there were no pains of any value, delivery was effected artificially. A few hours afterwards, she was in a state of great collapse, feeble, almost imperceptible pulse, pinched features, and coldness of the surface. The case looked very bad indeed. She was ordered gtt. $\frac{1}{4}$ of tinct. aconiti every two hours. Next morning she was perfectly well: reaction had taken place, but only to the natural state, and she made a good recovery without any further bad symptom.

In the shock after severe labours, I always prescribe aconite as above, and can testify to its great value, not only as a febrifuge, but as an antagonist to the usual febrile reaction which then takes place.

I intended to have entered on the consideration of Digitalis as a contrast to Aconite, but must leave it, as well as the question of the antagonism of medicines, to a future occasion.

ARTICLE IV.—*Case of true Polypus of the Left Auricle of the Heart.* By Dr DOUGLAS, one of the Vice-Presidents of the Medico-Chirurgical Society of Edinburgh.

(*Communicated to the Medico-Chirurgical Society.*)

MORBID growths in the interior of the heart, whether sarcomatous or carcinomatous, are very rare, and almost nothing is known of their clinical history. The best clinical record I remember to have seen of any case is given by Andral in his Anatomie Pathologique;[1] but in his narrative there is nothing that would decide the nature or the seat of the lesion.

In the case I have now to record, perhaps some progress is made in the knowledge of the symptoms—specially of the physical signs which may be looked for in such a state of the heart. Moreover, between this case and that of Andral, as well as one depicted by M. Cruveilhier,[2] and one referred to by Dr Bright,[3] there are certain resemblances, and some contrasts, attention to which may help to advance our knowledge of the disease.

For the anatomical description of the case, I am mainly indebted to Dr Grainger Stewart, who kindly made the dissection for me, and favours me with the account I give of the relations and minute structure of the tumour.

On the 28th of December last, I was requested by Sir James Y. Simpson to visit the patient, a gentlemen, aged 35. He was of large frame and development, and of extraordinary physical power. He had been more than ordinarily addicted to habits of active life in the country; he lived in easy circumstances, was temperate, but ate largely, especially when actively engaged. It was remarked by his friends that within a year he had made flesh considerably, with great abdominal fulness, considering his age.

At my first visit, I found him supported in bed, with the

[1] Edit. 1829, vol. ii. p. 328.

[2] Anat. Path., liv. xxix. pp. 2 and 3.

[3] Med.-Chir. Trans., vol. xxii.

shoulders raised. His breathing was laborious and panting, and he had an incessant, most harassing dry cough. He made no complaint of pain, only of the excessive breathlessness and increasing weakness. His surface presented a uniform faint straw-colour, and his expression was anxious and dejected, though there appeared to linger the aspect of ruddy health which he had formerly enjoyed. The pulse varied in frequency—about 120 in the minute—and it was small and soft, but regular. Urine acid, diminished in quantity, and high coloured.

He first experienced the symptoms of failing health a few months previously; and in the autumn he went to the Highlands, in the belief that his usual hard work, grouse-shooting, would put him right. For this, however, he found himself altogether unfit; and on one occasion an attack of hæmoptysis occurred which did not excite alarm, nor was he disabled more than by his previously felt weakness. The symptoms appear to have grown upon him from that time, but his condition did not seriously distress him till the 25th of November, when he consulted his medical adviser in the country for the first time.

His chest was well formed, and there was general fulness of flesh, with prominence of the abdomen.

The impulse of the heart wanted distinctness, had no heaving or force of action; but it had, especially to the ear, an apparent diffusedness and strength, or irritability, which contrasted with the weak and small pulse at the wrist. The exact situation of the apex cordis could not be well defined. The space, dull on percussion, was scarcely extended—no dulness existed on the upper part of the sternum, nor to its right side. The action of the heart was regular.

The sounds of the heart were not well pronounced—the *first* had lost its fulness; the *second* had acquired a degree of sharpness, so that the two were assimilated in their "clang." They had a greater loudness than the state of the impulse led me to expect; they were free of murmur.

There was nothing in the state of percussion or of the sounds of respiration to indicate any lesion of the lungs. There was progressive increase of distress during the first week of January, with increasing cough and dyspnœa, excessive sleeplessness, alarming and singularly vivid dreams, overpowering feebleness, nausea, inability to take food, and occasional paroxysms of excruciating, tearing, substernal pain. He suffered great distress on lying down, and dreaded the nights: the little sleep he had was obtained in an arm-chair, sitting and leaning forwards. His pulse rose to 150, and was rarely under 120 in the minute—always small, but never irregular. His tongue was at first clean: bowels acted, but not freely; urine decreased to 12 or 20 oz., and became excessively loaded with lithates.

The treatment at this time consisted of an aperient pill at bed-time, containing rhubarb, aloes, and taraxacum; frequent doses of an alkaline solution; and the decoction of scoparium.

Between the 8th and 12th January, a very encouraging abatement of his distress occurred, with increased flow of urine, which was now free of sediment. He was able to rest more nearly in the horizontal posture—though subject still to painful starting, especially on the approach of sleep, with dyspnœa and agitation; and indeed no real progress was made towards recovery. On the 15th, he complained that his rest had been much disturbed; and, for the first time, he winced and complained of pain on pressure in the right hypochondrium. The bowels, which hitherto had acted easily, now became torpid: an appreciable increase of the yellow colour of the surface set in, and nausea, which had from the first existed to the extent of loathing food, increased, with irritability of the stomach, and occasional fits of retching. At the same time, the local signs of hepatic derangement continued.

On the 18th, he was visited by Dr Christison and Dr Begbie, in consultation with Sir James Simpson and myself. He was languid and exhausted,—much distressed with dyspnœa, requiring to be raised in bed, and with cough, though it was less incessant and harassing than previously—still he always described it as unsatisfying, and at no time was there any expectoration. The circulation was very feeble and rapid—pulse about 140 in the minute. Tenderness, with fulness, and extended dull percussion in the right hypochondrium, were now manifest; and the urine was excessively loaded with lithates, dark-coloured, and scanty—15 to 20 ozs. daily.

Mild mercurial purges were administered at short intervals, and their action promoted by small doses of phosphate of soda. Frequent semi-fluid motions resulted, dark, and intensely bilious. A very remarkable effect arose from the attempt to relieve the hepatic symptoms by hot fomentations, which invariably excited in a few minutes violent fits of retching; similar attacks were subsequently excited by ordinary manipulation in examination of the belly. Nourishment was taken with great difficulty, and stimulants were demanded incessantly. Brandy and the still wines he soon loathed, but champagne was taken gratefully. He had great comfort in the use of ice, which usually averted impending fits of retching. At this stage of his illness there occurred, and continued to the end, a marble-like coldness of the surface—especially of the hands—though he complained much of the sense of intense heat. The circulation declined in strength daily, and I noticed a variation in the cardiac sounds such as I have not before met with. With the declining strength of the circulation, the assimilation of the heart's sounds became more and more marked, making it difficult to distinguish the first from the second. A slight reduplication of the second sound had been developed, and, at the same time, it underwent a marked intensification of its clang, acquiring the *hollow amphoric character*. This state of the sounds endured, in its highest degree, for about three days, when the assimilation in the clang of the sounds diminished, the first re-acquiring, in a measure, its natural fulness. With this change, a great improvement occurred in the frequency of the pulse, and, at the same time, diuresis set in—the quantity of urine increasing to 80 oz., under 20-grain doses of the acetate of potash. Any improvement, however, which occurred was transient. Œdema of both legs had been slightly present for a day or two; and, on the fifth day before death, both feet became rapidly œdematous and much distended. Anasarca extended to the thighs and lumbar regions; prostration increased with coldness of the surface; he had much distress from dyspnœa, and occasional fits of retching, at times accompanied by semi-convulsive attacks, leaving him in a state of approaching syncope, and excessive confusion. He died, after one of these attacks, on the 28th January.

Sectio Cadaveris.—Fifty hours after death. Decomposition had commenced. The bulk of the body and the volume of flesh were great. The only organs examined were the lungs and heart, and the liver and kidneys.

The lungs were in a normal condition. The heart was flabby, bulky, and very slightly dilated. Its cavities contained well coagulated blood. The right side presented no special abnormality.

On opening the left ventricle, the rounded nodulated extremity of a tumour, to be described presently, was seen projecting through the mitral orifice. On opening the left auricle, a tumour was found growing from its posterior wall of such bulk as seemed nearly to fill the cavity of the auricle, and hanging downwards, its point projecting into the left ventricle. The tumour was 4½ inches long, 2½ broad, and 1¼ deep at its deepest part. Its superficial and dependent part was coated with some layers of coagulated fibrin, and it presented nodules of a translucent appearance; but its base was organically connected with the auricular wall, and was dense in structure. On the outer side of the auricle, opposite the point where the tumour had its attachment, there were small outgrowths of a structure identical with that of the tumour itself. There was no coagulum in the auricular appendage, nor between the bands of the columnæ carneæ. The pulmonary veins were open; and the valves of the heart were healthy. The aorta was slightly dilated in its ascending portion and just above, it presented an insignificant narrowing, with a small cicatrix at the part.

Microscopic Examination of the Tumour.—The tissue, at its base, was rich in cells, many of them free, many of them resembling connective tissue bodies with long processes projecting from them. Many had prominent nuclei, but none presented the characters of typical cancer cells. In some parts the cells contained fat granules, and the intercellular structure, which was mainly fibrous, was studded with similar elements. The muscular fibre of the auricular wall was altered, containing elements similar to those found in the mass of the tumour.

The liver was enlarged, reaching slightly below the margin of the ribs. It was intensely congested, and its cells contained fat granules and colouring matter of the bile. The gall-bladder and ducts were much distended with a dark coloured bile.

The kidneys were healthy.

This case is of sufficient importance, in every point of view, to justify its record without remark or commentary; but an extraordinary interest attaches to many questions relating to its nature, its progress, and its symptoms, and a full review of these would be altogether beyond the limits of such a report as this. In the commentary I offer, I restrict myself to certain points in the diagnosis having a direct practical reference.

I pass from the discussion of the malignant or non-malignant nature of the disease with one remark, noting what was very observable, viz., the soft, translucent, and colloid aspect of the tumour, from which I infer that its growth was very rapid, especially latterly, accounting for the rapid course of the symptoms, which scarcely lasted over three months, and involving important questions in connexion with the fact that, in the beginning of August, he passed an insurance examination. In all probability the alterations observed in the state of the circulation during the last fortnight, as well as the rapid decline of his circulation, arose from the rapid growth and changes of the tumour. The bulk of the tumour, and its encroachment on the left auriculo-ventricular communication, afford a reasonable explanation of the signs which existed of obstructed cardiac circulation.

The symptoms and the condition of the patient were altogether singular; nothing, in my previous experience, afforded me assistance in forming an opinion; and, even during the last weeks, the difficulty of the diagnosis was such, that considerable difference and uncertainty of opinion occurred in our consultations.

His symptoms, so manifestly of pectoral distress, in conjunction with his extraordinary muscular development, and his habits of active physical effort, created an impression, shared by all, that thoracic aneurism existed. My first careful examination, however, satisfied me that nothing of the kind was discoverable; and though the urgency of his local distress, with the expression of anguish and dejection, and the perfectly normal state of the respiratory signs, suggested the view that the cardiac circulation was obstructed, I clung to the hope that the cardiac derangement was secondary. After renewed and careful examination, however, on the 2d January, I arrived at the conclusion that there was obstruction of the circulation in the heart; that the obstruction ought to have its seat in the left auriculo-ventricular communication, though the signs of its contraction were incomplete; and that there was defective hypertrophy of the left ventricle.

I have difficulty in stating explicitly the grounds on which I rested my opinion, chiefly because the signs were not of the well-defined character which admits of distinct description—indeed, they were in a great measure negative.

The distressing and long-continued dyspnœa, and the hurry of the heart's action, may be taken as a starting point. The latter threw doubt on the notion that an aneurism existed; and the invariable regularity of the heart's action, considering the degree of cardiac asthma, went far to impress me with the opinion that no valvular disease was present. But a great difficulty arose here, from the disproportion of the impulse and the radial pulse, which was small,—suggesting the view that mitral contraction might be present. Of this condition, however, the most reliable signs, particularly presystolic murmur and apical fremitus, were wanting. In the narrative of the case I have endeavoured to indicate the incompleteness in the contrast of the heart's impulse and the radial pulse,—that the contrast arose rather from irritability than from force of the heart's action. There was indeed a degree of contrast, but not from fully formed hypertrophy, which appeared wanting, as the state of the impulse and that of percussion indicated.

The dyspnœa under which the patient laboured requires special notice. Its persistence and its degree were equally conspicuous, and the stooping forward attitude it led him to prefer; but the most conspicuous circumstance connected with it was the entire freedom of the lungs from the effects of obstructed circulation. Manifestly, this had not been the case throughout his illness; for not only did he give an intelligent account of the opinion of his previous medical adviser (Dr Thetford, of Strangford, county Down), that congestion of the lungs had existed, but the attack of hæmoptysis in the autumn proved that to have been the case. It is sufficiently remarkable, though not unparalleled, that in such a state of the left auricle of the heart, and after a previous attack of hæmoptysis, the lungs should have regained a healthy condition. I abstain from speculation as to the explanation of this occurrence. I think, however, it is highly probable, considering the unobstructed state of the pulmonary veins, that the current of the blood entering the auricle might prevent the growth of the tumour in the direction of the veins, and thus save, in a degree, from direct pulmonary obstruction,—the extension of the tumour being promoted by the current of the blood in the direction of the ventrical or mitral opening.

Another peculiarity of his dyspnœa was its nervous character, and I mention this the more expressly, as in other respects the case was complicated with singular nervous symptoms. It is impossible to describe this feature of his breathing, or the manner of it. It had attracted my notice in connexion with the hurry of the heart's action and the healthy state of the lungs; and, at one of his earliest visits, Dr Christison used this expression:—" There is something hysterical in his breathing." I would suggest that the stooping attitude described should be kept in view in such cases. It might have some relation to the effect of the tumour on the bulk of the heart; and, possibly, it might influence the relation of the tumour to the orifices of the pulmonary veins.

The sounds of the heart were appreciably modified by the disease; but the assimilation in their "clang," which constituted their chief change, is not only, in its different degrees, of various diagnostic import, but it is in many instances most difficult to pronounce upon its presence. I believe this modification of the sounds may arise in merely functional disturbance of the heart; but in such cases it is slight and unimportant. It accompanies cases of dilatation, and in such cases may involve very grave and difficult questions; but its chief value is in relation to obstructive mitral disease. The diagnostic value of this assimilation of the sounds receives additional illustration from the present case, if, as I suppose, it arises from imperfect ventricular systole, the consequence of imperfect filling of the ventricle, due in this case to the obstruction of the tumour, and, in cases of mitral disease, to narrowing of the orifice.

One of the most conspicuous features of the case was the congestion which occurred—first of the lungs, and latterly of the kidneys and liver. In these respects, there was nothing very different from what is met with in many cases of disease of the heart—especially where dilatation has set in. I have already referred to the remarkable recovery from congestion which the lungs seemed to make, and the degree to which they preserved their integrity almost to the last. The obstructed renal function, with abundant lithates, differed in no respect from what is commonly met with; and the superadded hepatic engorgement was simply what is seen intensely developed in no disease of the heart more than in cases of mitral obstruction. It was remarkable only for its impracticable obstinacy, and, when first it occurred, it tended to renew in my mind the conviction that mitral obstruction existed; so much so, that if my colleagues would have agreed to the measure—which, however, I could not urge strongly from a remaining uncertainty in my opinion of the exact seat of disease—I would have applied leeches to the scrobiculus, as I have always found this remedy well borne, even by feeble patients, and very effective for the relief of congested painful liver in cases of mitral obstruction. In reference to the obstruction of the circulation, a point in the case which I desire especially to note, is the entire absence of dropsy till the fifth day before death, and its then rapid development, though it did not proceed to its highest degree. I think that the state of the heart might account for this; but I am unwilling to increase the length of my comments by mere speculation, either on this or the other questions to which the facts of my case point. I have been greatly impressed by the note which M. Cruveilhier makes in describing the case he depicts. Referring to the maintenance of the circulation in his case, in which the right auricle was largely occupied by a carcinomatous tumour, he says:—"The mere penetration of blood to the auricle seemed to suffice."

The last point to which I would invite attention is the nervous symptoms which occurred.

The association of nervous symptoms with disease of the heart has been repeatedly described within the last forty years; but there is still a gap in the explanation of the phenomena. The combination was very singularly illustrated in my patient's case, and I have already referred to the peculiarity which it conferred upon his most distressing symptom—dyspnœa. But there were other symptoms more manifestly and directly nervous, particularly the very remarkable irritation of the stomach, which resulted from the application of fomentations, as well as from manipulation of the epigastrium; also the semi-convulsive attacks which resulted repeatedly when violent attacks of retching occurred. His cough, too, I class with these symptoms. It was very similar to the cough I have often observed in cases of acute pericarditis: In cases hitherto described, the nervous complication has, for the most part, presented itself in the form of chorea; and the pericardium has been found to be the seat of disease—not the endocardium; still there seems to be an analogy in the present case; but additional observations are necessary to give practical value to speculation on the subject. Dr Bright published a case in 1834, in which a widely diffused medullary exudation on the pericardium excited sudden attacks of dyspnœa, with violent agitation and apparent convulsion of the diaphragm resembling a hysteric convulsion. Dr Bright attributed these symptoms in his patient to the phrenic nerve being implicated in the irritation arising from the contiguous disease of the pericardium and heart.

In some of its symptoms, the case described presented a noticeable resemblance to those given by M. Andral and M. Cruveilhier. The pale yellow or straw colour described in Andral's case, seems to have been very similar to that of my patient; and in both, as well as in M. Cruveilhier's case, the signs of cardiac as well as of pulmonary change, were comparatively slight. There were paroxysms of sudden and intense substernal pain. Dyspnœa was probably more urgent in my patient, perhaps, because in him the disease was more rapidly developed. I have already referred to the remarkable absence of the effects of obstructed circulation. In each of these cases the state of the disease, more particularly the extent to which the auricles were occupied, seems at first sight very likely to obstruct the circulation, and to superinduce all the evils arising from such an obstruction; yet, in Cruveilhier's case, where the right auricle was nearly occupied by the tumour, no dropsy existed; in Andral's case, which was of slower progress, urgent symptoms existed for six months before anasarca set in; while in my patient, though the action of the heart had been extremely embarrassed for weeks, anasarca only manifested itself a few days before death. The pulse, which was so conspicuously affected in my patient, is not noted, and was probably not much affected in the cases of Andral and Cruveilhier,—the disease in their patients being of the right side of the heart.

My commentary on this case has exceeded the limits I had intended; and yet in explaining and illustrating the details, a great deal has been left out of view. The value of the case is enhanced by the history of its progress during life; and although extreme uncertainty as to clinical phenomena must still rest on such cases in connexion with the undetermined nature of the disease, and the relation of the tumour to the muscular substance of the heart as well as to its cavities and orifices, the following summary may be given of the signs which existed:—

1. Rapid development of the symptoms.
2. A previously robust state of health.
3. Dyspnœa, in the absence of signs of pulmonary obstruction.
4. Persistent hurry of the circulation, with regularity.
5. Reflex nervous irritation, with a *quasi* hysteric breathing; paroxysmal cough without expectoration; retching, semi-convulsive attacks, and tearing substernal pain.
6. Delayed obstruction of the circulation of the *lungs*, the *kidneys*, and the *liver*.
7. Anasarca delayed, but rapidly developed.
8. Pulse small and regular.
9. Contrast of a more marked cardiac impulse than radial pulse.
10. Absence of cardiac murmur.
11. Assimilation in the "clang" of the heart's sounds.

ARTICLE V.—*Objections to the Cottage System of Treatment for Lunatics as it now Exists, and Suggestions for its Improvement and Elaboration.* By Dr J. B. TUKE, Medical Superintendent, Fife and Kinross District Asylum.

GREAT anxiety is already felt in many of the districts of Scotland at the gradually increasing number of pauper lunatics, and greater reluctance is evinced on the part of the ratepayers to increase asylum accommodation. It is strongly urged by many good authorities that the "cottage system" of treatment for harmless lunatics and idiots provides amply against any further expenditure in adding to the Royal and District Asylums. I propose, very briefly to mention some of the objections which medical superintendents of asylums entertain against the system as it now exists, and to make suggestions for its improvement and elaboration, which, in my humble opinion, would render it more useful as a means, not only for the detention of patients, but for their cure and amelioration.

The following are the objections which most prominently present themselves to the medical-superintendent mind:—

1*st*, That the existing supervision over patients boarded in the houses of cottagers is utterly insufficient. If the patients in a lunatic asylum under the superintendence of a medical man of experience, aided by one or more assistant-surgeons, where a large staff of paid attendants is maintained, need the careful and constant supervision of the General Board of Lunacy, the district board, visiting committees, sheriffs, and visiting justices, how much more does it seem necessary that insane patients consigned to the care of a cottager should be supervised most jealously? At present the visitation of such patients is limited to a quarterly visit from the parochial surgeon, to a half-yearly visit from the inspector of poor, and a yearly visit from a Deputy Commissioner in Lunacy. Either the machinery of asylums is too complicated, or the supervision of insane paupers in private dwellings is proportionately inadequate.

2*d*, The utmost difficulty is experienced in selecting proper patients for boarding out. The class which is said to be best suited for such treatment is congenital idiots or hopeless imbeciles. It is well known how very difficult it is to prevent this class of patients from degenerating and lapsing into the most degraded condition even under very favourable circumstances: how much more likely is this to occur when the lunatic is committed to the care of a guardian who reaps an absolute benefit from a board ranging between 3s. 6d. and 6s. a week?

3*d*, Medical superintendents experience great difficulty in procuring proper persons to act as attendants on the insane. Even under supervision, it takes months to train such a servant, and to inculcate that consideration which is due to the weaknesses of those under his or her care—how, then, is it to be expected that the poor labourer or artisan can at once be fitted to undertake the office for which experience shows special training is absolutely necessary. These are objections which no amount of argument can dispel from the minds of those who have lived much amongst the insane, and whose care and treatment have been their special study.

These are objections to the system as it at present exists, but I believe that they might be entirely obviated were district boards authorized to take over care and supervision of *all* their pauper lunatics. In every district of Scotland there now exists a District Lunacy Board, and in most a district asylum has been erected sufficient for the accommodation of the more urgent cases; still 28 per cent. of lunatics are boarded in private dwellings, subject only to the infinitesimal amount of supervision above alluded to, and in no way under the control of the district board.

The plan I would propose for the elaboration and improvement of the cottage system is as follows:—That all lunatics of the district be placed under the control of the district board, that such patients as are suited for residence in private dwellings be located in villages as nearly adjacent to the asylum as possible, and that the medical officer or officers of the asylum make regular visitations as often as the board may direct, and report the result. By this means security would be obtained against any abuse of the system. The guardians should be approved of by either a deputy commissioner or the medical superintendent of the district asylum, and their houses should be made liable to visitation at any hour of the day or night by the latter officer, in the same manner as lunatic asylums are liable to the visitation of the Commissioners in Lunacy.

It may be said that this is constituting, to a certain extent, the medical superintendent—the district inspector. Why should it not be so? The inspection of the asylum over which he has charge is already sufficiently provided for, and I think it will be admitted that, as a rule, no better judge than a man experienced in the management of lunatics in asylums could be found as to the proper treatment of lunatics in private dwellings. No doubt many minor difficulties would arise in the organizing of a system such as is now proposed, but they would be small in comparison with the advantages to be derived in overcoming them.

1*st*, The public would be guaranteed the proper care of *all* their pauper lunatics by their transference to the care of the district board.

2*d*, A large proportion of lunatics would be provided for by the cottage system, if superintendents could assure themselves of the proper treatment of patients under such circumstances.

3*d*, The system might be made available for the treatment of convalescent cases. I know of many instances within the last few months which would have been benefited by a change from the discipline of the asylum, could I only have assured myself by personal observation of their proper treatment during their term of convalescence.

The cottage system must sooner or later be brought more fully into play, but it will never gain the confidence of the general public, or of those physicians who have made insanity their special study, until more stringent provisions against abuse are instituted, and greater assurances are offered that patients so provided for are looked after in a manner more nearly approaching to those confined in asylums. By its elaboration all necessity for the enlargement of existing asylums would be obviated for many years to come. The machinery for carrying out the plan I have suggested already exists; and, were it adopted, it would secure a reduction of the enormous expenses to which parishes are liable for the maintenance of pauper lunatics. Better still, it would ensure partial liberty to very many who are now subjected to the more rigid discipline of the asylum.

ARTICLE VI.—*Cases of Extra-Uterine Gestation and Uterine Hæmatocele.* By J. MATTHEWS DUNCAN, M.D.

(*Read to the Obstetrical Society, 26th February* 1868.)

MRS N., æt. 32, mother of four children, a healthy, bright-faced lady, came to Edinburgh to visit some friends, and enjoy the gaiety of the season. On November 16th, she sent for me on account of what she believed to be impending miscarriage. She had passed ten weeks since the end of her last menstruation. She had slight pains in the region of the uterus, and inconsiderable bloody discharge. Making a vaginal examination, I found the uterus moveable and not enlarged to the dimensions appertaining to a pregnancy, believed to be already in the third month. Besides, I detected nothing unusual in the pelvis, and I was therefore not in a position to assert my belief in the existence of pregnancy. But my patient would not entertain any doubt on the matter. She was quite certain that she conceived on a certain day, ten weeks previously. Knowing the gravity of her case, she desired to state the reason for her certainty as to her pregnant condition. This was a peculiar feeling—a tremor—which she felt at the time of the fertile connexion, which was so peculiar as to attract her particular attention, and which occurred only four times in her life previously, and, as she was sure, at her four previous conceptions.

During the following two days, pain almost entirely ceased, and the discharge was reduced to mere spotting of the cloths. She was taking the acid infusion of roses, and was kept strictly in the recumbent position in bed.

At 8 P.M. of the 18th, I was hurriedly sent for, and arrived at the bedside in about half-an-hour thereafter. It was with some difficulty that I procured from my fainting and pulseless patient the following history:—Shortly before she sent for me, she felt (as she said) that the miscarriage was now to take place. Intermitting pains, like slight labour pains, though very painful, had come on. Yet there was no bleeding. Then, just after the messenger had gone, the intermitting labour-like pains ceased, and never returned, but there arose in the region of the womb a severe, sharp, continued pain, and she became very weak, giddy, and then half unconscious.

Immediately on seeing her, I whispered to the nurse in attendance that she was probably about to die of flooding, and proceeded to engage myself vigorously with the hæmorrhage. But I in a moment discovered that there was no external hæmorrhage going on. I then asked to be shown the bloody cloths and clots which had, I fancied, been removed, but received for answer that the vulvar napkin then in use had not even been recently changed. The nature of the case then seemed plain. My patient was dying from hæmorrhage into the peritoneum. As I believed my patient to be pregnant, I concluded that the hæmorrhage was from rupture of an extra-uterine fœtal sac.

I sent for Dr Thompson Lowne, who soon arrived, and continued till the end of the attendance to give me his valued assistance. He coincided with my views as to the nature and treatment of the case. We carefully administered moderate doses of brandy, with frequency varying according to the state of the circulation and other symptoms; and we kept cold wet cloths constantly applied over the hypogastrium. For six hours, during which this treatment was instituted, we had difficulty in keeping in the spark of life. Extreme pallor, deathlike coldness, absence of wrist-pulse, or its temporary weak presence, short attacks of sickness, and vomiting, were the symptoms. Then the coldness diminished, the weak pulse could be steadily felt, and it was evident that, for a time at least, she was comparatively safe.

The hypogastric region was very tender, but no hardness or tumour could be felt. Digital examination, per vaginam, discovered tenderness in the whole region of the brim of the pelvis; the uterus not fixed, but pressed forward towards the pubes; behind the uterus an expanse, having a soft boggy feeling, without hardness or distinct special resistance to the gently-pushing finger.

On the 19th, feverish reaction was very slight. She still had occasional attacks of faintness, and was extremely anæmic.

On the 20th, she continued in a favourable state. Pulse below 100. Very gentle examination made, per vaginam, discovered a mass of hardness where formerly only a boggy feeling was perceived. Above the pubes, near the middle line, a hard mass could be felt. In both situations tenderness was great.

On the 22d, some pain came on resembling what she had often previously suffered in menstruation, and a fleshy mass was discharged. This was handed to me as the abortion, come away at last, for so both patient and nurse opined. It was carefully examined, and found to be a complete decidua vera of extreme vascularity, and having every character of that membrane. It was more than two inches long, and was the hypertrophied mucous membrane of the considerably hypertrophied uterus. No trace of an ovum could be discovered, nor any imperfection or laceration of the membrane where a small ovum might have once been inserted. There was little discharge of blood at this time, and slight discharge went on regularly during the further progress of the case.

The retro-uterine tumour, now hard and of diminished tenderness, gradually increased in size, but not to a great extent. Per vaginam, it approached nearer the examining finger, though still high in the pelvis, but felt smaller in consequence of the part projecting downwards, being the only part accessible. In like manner it became more prominent above the pubes. The uterus was adpressed to the pubes and fixed. She had some attacks of severe pain in the iliac regions and in the tumour, but otherwise was well.

On January 4th, there was some metrorrhagia. I incised the tumour, per vaginam, just behind the cervix uteri, with a guarded sharp-pointed bistoury. The finger, introduced through the wound, felt the clots composing the tumour. Some active bleeding occurred, which was restrained by a plug of pieces of linen. But some small amount of fresh bleeding continued incessant for three days.

During the following night she had much pain, and frequent attacks of sickness. Tenderness over the pelvis much increased. Pulse has risen from 98 to 108. During the next four days, there being profuse fœtid dark bloody discharge, and consequent septicæmia, the pulse ranged from 120 to 130, and she was very ill in every respect; but then the fetid discharge diminished, and, on the 11th of January, when it ceased to be bloody and fetid, her pulse was again below 100, and she was comparatively comfortable, and evidently convalescent.

During the days from the 5th to the 10th, I removed, with the finger or with forceps, numerous clots, some very dark in colour, others partially decolorized. These were in the vagina, or near the artificial wound. Along with them, I removed several considerable shreds of ovuline membranes, and masses of chorionic villi, easily recognised as such by Dr Lowne and other physicians. The vagina was frequently washed out with a dilute solution of permanganate of potash, and I repeatedly injected into the retro-uterine cavity a weak solution of carbolic acid.

January 13.—There is now no pain anywhere. Pulse 84. No tumour can be felt in the hypogastrium. A purulent discharge, moderate in quantity, flows from the vagina.

January 16.—No pain. No tenderness. Purulent discharge very slight. Cervix uteri in its natural situation. Post-cervical hardness inconsiderable in extent, and the wound in it will not admit the tip of the finger. She is evidently very anæmic.

February 8.—Says she is quite well. Post-cervical hardness almost gone. Wound in vagina healed. Uterus restricted still in its mobility.

This case suggests many remarks, from which I select the chief for particular statement.

The diagnosis of extra-uterine pregnancy was imperfect in consequence of the absence of metrorrhagia up till within a few days of the production of the retro-uterine hæmatocele, and in consequence of my being unable to detect the presence in the pelvis of any unnatural tumour. This non-discovery of tumour will not astonish the experienced practitioner: for while, in some cases, it is easy to find so small a body as the ovary; in others a considerable mass, if lying high in the pelvis, and especially if mobile, may elude, in a tense or moderately distended abdomen, the most searching examination. But even had metrorrhagia been present, and a tumour of proper size detected, the diagnosis would not have been one producing a high degree of assurance. In this case, pregnancy was believed to exist, and the upshot of the case shows that it did exist. The grounds of the patient's certainty of her pregnancy are very interesting, and are thoroughly reliable, *quantum valeant.*

Whether the ovum was developed in the Fallopian tube or not, is a question. My impression, derived chiefly from considering the frequency of this kind of extra-uterine pregnancy, and the time of supposed rupture of the tube and of the production of the hæmatocele, is that it was in the Fallopian tube. Whatever was the situation of the ovum, the bleeding resulted, with almost absolute certainty, from rupture of its containing sac, and this rupture was what is called spontaneous, and its spontaneity, as well as its being preceded by the feelings of labour-pains, confirm the Fallopian site of the pregnancy. When the bleeding occurred, the patient was lying quietly in bed, and she had been in bed continuously for several days.

In my reading, I do not remember meeting with records of the discharge of the entire decidua, as in this case; although I have read of its coming away in bits.[1] I possess a specimen of extra-uterine gestation, in which the decidua is still everywhere attached to the greatly hypertrophied muscular uterine wall. In my case, the decidua was fortunately in so perfect a state as to prevent the notion being entertained of an intra-uterine pregnancy having existed; and thus it confirmed the already-made diagnosis of extra-uterine gestation.

It is interesting to compare, as far as possible, the amount of blood lost with the symptoms. In my own case, while the woman was as nearly dead as may be for six hours, and while it was evident that the hæmorrhage was the chief cause of her danger, yet it appears to me there were other sources of depression connected with the injury. They may be called *shock*, but that is merely a sort of analogical classification of the sources of depression. The word *shock* explains nothing. I do not find that authors, and I especially refer to the collection of cases by M. Goupil, have raised this interesting point as a question, and I shall only farther say that, while I have no means of correctly estimating the amount lost by Mrs N., I feel sure that it was not at all in proportion to her danger of death, that a larger amount of bleeding otherwise produced would have been a comparatively light affair. This opinion is, in my mind, corroborated by two considerations. Of these the first is, that in several cases of fatal hæmorrhage in lying-in women, more or less under my direct supervision, the amount of blood lost was quite small, to speak generally, when contrasted with the dire results of which it was the only evident cause.[2] The second consideration is, that peritoneal injuries involving certain organs, the uterus for example, are far more dangerous and fatal than other like, or even more extensive injuries as in ovariotomy. This interesting subject has been elucidated by M'Clintock in his paper on Lacerations of the Vagina in the course of Labour, where he shows the greatly less danger of vaginal laceration than of uterine.

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[1] Joulin relates an autopsy by Helié in which the decidua was partially detached only. See his "Traité Complet d'Accouchements," p. 966.

[2] Campbell, in his memoir on Extra-uterine Gestation, when speaking of this point, says (p. 130), "The rapid effusion of blood from the injured structures

Nonat[1] has naturally made the absence or only feeble presence of symptoms of reaction stand as evidence against the intra-peritoneal site of the hæmorrhage in ordinary pelvic hæmatocele. But facts are worth a great deal of reasoning; and the force of Nonat's remark is diminished or destroyed by observing that, in this case and in the next, and I might cite others in my own experience, the hæmatoceles were formed with following symptoms of reaction that were certainly little more than appreciable.

It will be observed that I have pointed out, in the history of this case, the absence of hardness of the retro-uterine tumour or blood before its encystment. The mass then presented only a soft boggy feeling. But though this was true, the blood had sufficient force to displace the uterus forwards before its encystment,—a curious circumstance, and one difficult of explanation.

In cases of retro-uterine hæmatocele, I have abstained from interference with the interior of the blood-cyst, on account of the danger it appeared to me to involve, and because in simple cases of this kind I saw no reason why the discharge of all the decomposing clots should be long delayed; and I think abstinence from interference should be the rule, even when septicæmic symptoms run high. But believing, as turned out to be the fact, that in this case ovuline structures were adherent to the cyst wall, and fearing long continuance of septicæmia in my very weak patient, I was induced to try the washing out of the cyst. This was first done two days after the operation on January 4. A common female piece attached

must be the principal as well as the immediate cause of death; but we are not at the same time to overlook the influence of other circumstances, as the exhaustion of the patient by her previous sufferings, the effects of extensive inflammation so frequently observed, and the extreme constitutional derangement abruptly excited by an injury so severe of an organ engaged in the performance of what may almost be considered next in importance to a vital function."

The following passage shows that M. Goupil goes the length of distinguishing between syncope or fainting, the result of rupture, and fainting, the result of loss of blood. It occurs in the discussion of M. Gallard's case (Clin. Mem. on Dis. of Women, vol. i., p. 268),—" It would therefore seem (says Goupil) that in this case the syncope was dependent rather on the rupture than on the abundance or rapidity of the loss of blood."

Speaking of lacerations of the vagina in the course of labour, M'Clintock says—" By some it is supposed that the severity of the constitutional shock depends on the amount of hæmorrhage resulting from the laceration. Though I would not altogether exclude hæmorrhage from having some effect this way, yet hæmorrhage in any considerable quantity is rarely present; and, besides, both analogy and experience justify us in considering that its influence must be comparatively very small."—*Dublin Quarterly Journal of Medical Science*, May 1866.

[1] Maladies de l'Uterus, p. 334.

to an enema syringe was introduced well within the vaginal wound and many ounces of a weak solution of permanganate of potash injected, care being taken that it ran off freely as it was injected. But in spite of all the care, great pain above the left groin was produced; abdominal tension was much increased, as well as abdominal tenderness, especially above the left groin; my patient's whole symptoms were much aggravated for a whole day; and I repented having used so large a quantity of fluid. It was at least twenty ounces. In my subsequent daily injections, which were done in a like manner, and which the patient positively liked, I passed only about two ounces of a weak carbolic lotion. I believe that my first injection injured the encysting adhesions, and involved the patient in some extent of fresh peritonitis. A like evil result of injection, but with fatal issue, in a case somewhat similar, is to be found in M. Goupil's work. The case is related by M. Gallard, and I shall quote a small part of the report from Dr Meadows' translation.[1] "On the 6th, M. Robert punctured, per vaginam, the fluctuating part of the tumour. A small quantity of fluid blood escaped, and continued to do so during the day, to the evident comfort of the patient. Next day it was injected with warm water, which brought on extreme suffering in the hypogastrium. The injection was accordingly suspended, and eight leeches were applied; she had rigor and vomiting, and the pain continued; ten leeches were then applied, followed by mercurial inunction and calomel internally, but she died in a few hours."

In his chapter on the prognosis in intra-pelvic hæmorrhages occurring in extra-uterine pregnancies, M. Goupil, in his work already quoted, says, "It is but too true that we are authorized in saying that all the cases of intra-peritoneal hæmorrhage arising from extra-uterine pregnancy end in death. Some cases, nevertheless, would give one slight hopes of a happier termination. Lastly, the long continuance of the disease in the cases comprising our fifth variety, its nature, and the example of the case of M. Gallard, are proof of the possibility of a cure; in this case we can well understand that the inherent difficulty in diagnosis may often cause us to doubt the existence of such an event, though we are unable to quote an example."[2] This quotation gives the case just related a unique character, and therefore it is worth while to call attention to the treatment pursued. In this respect it is only the opening of the cyst by bistoury on the forty-eighth day after the hæmorrhage took place, that demands special regard. It may be that the early stage of the pregnancy in my case may have been in the patient's favour; and on this point I wish I had the evidence derived from the fœtus itself to adduce. Of it no part was found; and it has to be added, that though the nurse was an honest careful woman, she said she had, against orders, thrown away the largest mass discharged without previously submitting it to inspection. I have nothing to say regarding the operation except to recall the fact, that there was considerable bleeding at first, and oozing for at least two days afterwards. I must take care to say that, although I resorted to this practice, and, according to Goupil, with unprecedented success, I do not recommend nor discommend it to my brethren. I record the case for its own value, and have only to add, that I was instigated to operate by the growth of the tumour, by the recurring attacks of severe pain in it, and by the occurrence in my practice of the fatal case which I shall immediately relate.

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[1] Clinical Memoirs on the Diseases of Women, by MM. Bernutz and Goupil, Sydenham Society's Trans., vol. i. p. 257.

[2] Cases of supposed survival are mentioned by Goupil, but discredited. They are also mentioned by Grenser, Lehrbuch der Geb., 1866, s. 740.

Speaking of the operation of opening the blood-cyst in cases like this, M. Goupil makes the following remarks:—"When happily, though it be seldom, the patient has not succumbed under the profuse hæmorrhage, and when the clot is encysted, or the hæmorrhage has taken place into the fœtal cyst, without causing rupture, various indications present themselves. Fearing the rupture of the tumour, which so often occurs, and in order to relieve the patient of the symptoms due to its size, the question arises, Will it be right to make a puncture or incision in order to empty the tumour? The few facts I know of prevent my answering this question as directly as I can in the case of hæmatoceles in the unimpregnated state. Still, the fear of rupture of the fœtal cyst through successive hæmorrhages, as occurred in the case of M. Pize, will not settle the question of an operation, the results of which are at least indecisive. The example of puncturing practised on the patient of M. Gallard, if it does not quite condemn the operation, inasmuch as death was due to the injection of water and not to the puncture, certainly does not show that the operation gave any relief. Moreover, statistics prove that extra-uterine pregnancies in which the fœtus has died more often end happily when the disease follows its natural course, uninterrupted by the intervention of art. We ought therefore to hold back from any operation, at least until an advanced period, when the surgeon may assist the work of elimination begun by nature. I am of opinion, therefore, that we must confine ourselves to an expectant treatment in the first stages, and continue to follow the same wise conduct in the latter stages, until observation points out the necessity for intervention."

Mrs R., æt. 34, mother of three children, of which the last was born nine years previously at the seventh month, was admitted on the 24th November 1863 into the Royal Infirmary. She had, she said, menstruated regularly up till three and a half months ago. After that date her courses, she said, did not reappear for ten weeks, but her health she describes as being good all this time. About four weeks ago menstruation returned, and then she had shivering, pain in the back and in the lower part of the belly, on the right side, and pains which she describes as bearing down. These symptoms, and the bloody discharge, continued more or less severe till she came into hospital. When our suspicions regarding pregnancy were mentioned, she laughed at the notion as ridiculous. She had no constitutional symptoms worth recording.

The physical examination made on the 27th revealed the following conditions:—The hypogastric regions are distended and very tense. Although there is a considerable extent of hardness felt over the brim of the pelvis, there is only an indistinct feeling of a tumour, which seems to occupy the right half of the hypogastrium. Absolute dulness is only found covering a space nearly three inches square above the horizontal ramus of the right pubic bone. The cervix uteri is nearly in its natural situation, and is enlarged, but not notably softened. Behind the uterus there is great feeling of fulness, and in some points of hardness, especially towards the middle line. There is not marked tenderness. The probe shows the uterus to lie fixed in front of the mass occupying the upper pelvis, its fundus being pressed forwards, and its cavity is longer than natural.

The case was diagnosed as one of intra-peritoneal hæmatocele, and suspicion was entertained as to its being complicated with extra-uterine gestation. Regulation of the diet, attention to the state of the bowels, and repose in bed, were all the attentions which the case seemed to demand, and they were put in force.

On the evening of the 28th November, the patient complained of severe and suddenly arising pain in the left side of the lower belly, where there arose also great tenderness on pressure. In spite of treatment these symptoms increased, and she soon became much troubled with frequent calls to make water. About thirty hours after the attack began she was seized with sickness and vomiting, under which she rapidly sank, and died about thirty-six hours from the commencement of the seizure.

On the autopsy the following observations were made:—On opening the abdomen, a quantity of bloody fluid escaped. The surface of the peritoneum generally showed signs of recent acute inflammation, the intestines cohering at various points by recent lymph. Above the horizontal ramus of the right pubic bone, and immediately behind the abdominal wall, the region of absolute dulness above described, was a large opening into a cavity filled with dark bloody fluid. This cavity extended from the brim of the pelvis on the right side to the crest of the ileum on the left side. Except above the right pubes, it was covered or roofed in by coherent small intestines. It extended in front as far as the bladder. The bloody fluid and blood-clots were evidently intra-peritoneal. The peritoneum of the sac was in some parts three times its natural thickness, and everywhere it had a rough surface of dense adherent and not recent lymph. The left prolongation of the sac presented an opening into the general peritoneal cavity. On laying back the bowels, one fold of small intestine was found adherent to a mass of about the size of a very large orange, somewhat flattened antero-posteriorly, and lying between the uterus and upper half of the sacrum. This mass was evidently sub-peritoneal, and, on being opened, presented clots of blood in its cavity. The walls of the cyst varied in thickness from a line to about half an inch. It had an opening inferiorly in front, that is, just behind the uterus, above the roof of the vagina, and into Douglas's space. When laid freely open, it was found to contain, besides blood clots, a fœtus of under two months' growth. It was slightly decomposed, and was attached by its cord to the placenta, adhering to the anterior wall of the cyst. The wall of the body of the uterus was about five-eighths of an inch in thickness, and of this thickness the mucous membrane constituted one-half.

This case offers many points of interest. While it adds one to the long list of cases of extra-uterine gestation fatal from intra-peritoneal bleeding, it appears to me to throw a shade of pleasant light over the future of this class of cases; for, with our pretty complete knowledge of it, there is nothing to be found incompatible with ultimate recovery. The case was one of complicated intra-peritoneal hæmatocele; and the complication, though it no doubt generally causes death, does not seem to me to present any very great obstacle to recovery. The recurrence or renewal of bleeding seems to be a principal source of danger. But, besides this, we have a cyst whose contents are not mere blood, but also a dead fœtus, which cannot be absorbed like the contents of a simple hæmatocele, and for whose permanence or discharge from the body special provision must therefore be made. Had this case been treated as that recorded before it, the woman would certainly have had a long illness, but she might have recovered.

Read by the light of the post-mortem examination, the history of the case is easily composed. About three and a half months before her death, Mrs R. became pregnant. The pregnancy was remarkable in causing no symptoms. About four weeks before death she began to lose blood per vaginam, and then the cyst of the abdominal pregnancy was overfilled with fresh blood, and ruptured, and the intra-peritoneal hæmatocele was formed. About thirty-six hours before death, the adhesions on the left extremity of the hæmatocele gave way;[1] the old bloody fluid ran into the general cavity of the peritoneum, and fatal peritonitis was the result.

It is especially to be noted that the formation of the hæmatocele about four weeks before death, when certainly a large quantity of blood was effused into the peritoneum, as well as passed per vaginam, did not cause alarming illness, the woman merely describing herself as commencing to menstruate, with painful accompanying symptoms. In regard to this I shall only recall to mind that, in this case, neither the uterus nor the Fallopian tube was injured, and that the absence of syncope and prostration may probably be thereby accounted for.

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[1] I cannot understand the argument of Nonat in defence of his view of the paramount importance and frequency of sub-peritoneal hæmatoceles, so far as he insists on the rupture of such a tumour into the peritoneum as incompatible with the doctrine that hæmatoceles are mostly intra-peritoneal. His words are:—"Comment se fait-il donc que l'ouverture du foyer dans le péritoine soit admise par ceux qui rejettent et qui nient l'existence de l'hématocèle sous-péritonéale? (Ibid. p. 349.)

The diagnosis in this case was incomplete, the pregnancy being only suspected. But I cannot in this instance discover any means whereby further assurance could have during life been attained.

The physical condition, as believed to exist during life, was exactly verified after death; and there, perhaps, was room for the vain regret that I did not open the cyst by puncture or incision, through the abdominal wall above the right pubic bone.

Mrs M., æt. 40, mother of four children, of whom the last was born eleven years previously, was admitted into the Royal Infirmary on May 8, 1863. About fifteen months before her admission, her courses had ceased. She thought she had fallen in the family-way. Her belly enlarged, her breasts filled with milk, and she distinctly felt the movements of a child within her. Between the time of conception and the time of her expected confinement, in August, she had twice some loss of blood, which she thought was menstrual. In August, she had what she calls a "false conception," blood coming away in clots and in large quantity for three weeks. She, at this time, thought herself to be in labour. Although reduced to great weakness at this period, she soon regained her strength, and has since then menstruated regularly. On April 19, shortly before admission, she says she was taken with shivering, and then grew hot and sick, having much pain across the chest, but no cough. This state lasted for a week. Then she was seized with vomiting and purging, the vomited matters being of a yellow colour, and having a bitter taste.

On admission, the countenance was very anxious, and of a greenish yellow tint. The breath had an offensive odour, and the same was exhaled by her skin in every part. The pulse was very weak and small, but not quick. She occasionally vomited a little, and had some diarrhœa, the stools being dark and offensive. Her complaints were very small, and when asked repeatedly what ailed her, she answered always, "Weakness."

At the lower part of the abdomen there was a very tense and very tympanitic distention. But, in addition, there was evidently much unnatural consolidation about the pelvic brim. A probe was easily passed into the uterus, which was elevated, the fundus being about two inches above the middle of the left Poupart's ligament. The uterine cavity was 3½ inches long. Examination, per rectum, discovered, as high up as the finger could reach, a mass lying in the gut. It projected through a contracted part of the rectum. On pulling it with some force, a part came away, which proved to be the foot, and part of the leg, of a fœtus. This being removed, there could be felt, as high as the finger could reach, a hole in the anterior wall of the rectum about as large as a shilling. Through the hole the leg had projected.

On the following day the patient was put under the influence of chloroform, and the whole child extracted, footling. It measured about 17 inches in length, and the largest circumference of its collapsed and flattened head was about 11 inches. Nothing like a placenta was discovered. The hand, passed into the interior of the extra-uterine cyst, felt a bag of soft flexible walls, and having a rough surface.

The patient was not much affected constitutionally by the operation, and her recovery was not marked by any constitutional disturbance or ordinary suffering worth recording. About a week after the birth she was out of bed, and after four weeks had elapsed from it, she walked out.

Forty-eight hours after the operation, the offensive odour of breath and person were not discoverable.

In six days she began to feel the power of her sphincter ani to return, and about a fortnight after the operation its function was completely renewed.

The discharges from the cyst could not be described from their admixture with the fluids of the diarrhœa, but the fœtor, which seemed to us to result from decomposing matters in the cyst, lasted for several days.

The retro-uterine mass, resulting from collapse of the cyst, diminished in amount rapidly, and, though at first large, it was nearly gone four weeks after delivery. At this time the uterine cavity was still three inches long.

The history of this case can, with great assurance, be made out by a collation of the whole facts of it. She fell in the family-way in December, and, before the arrival of the usual time of delivery, she had twice an attack of moderate metrorrhagia. In August, the time when labour might naturally have been looked for, she had much constitutional disturbance, suffering in the hypogastrium, and severe metrorrhagia. The child died, and underwent a peculiar kind of decomposition. About April 19 of the next year, the the extra-uterine cyst opened into the rectum; fetid decomposition of its contents began and symptoms of septicæmia, which only ceased when their cause was removed.

Although the correct diagnosis of this case was not delayed beyond the completion of the physical examination, it would have presented great difficulty, and might have been erroneously made, had not the fœtal foot in the rectum made the case plain. The chief, if not the only, difficulty was in the fact of the belly being everywhere tense and resonant on percussion. The tension prevented a tumour being distinctly felt or fœtal parts being made out. The tympanitic distention gave the examiner the impression that there was no considerable solid mass in the abdomen. This interesting concealment of the true state of matters arose from the fact that the extra-uterine cyst was distended with fetid gas. The diagnosis of an ovarian dropsy, into which, from tapping not being performed with due care, air has entered, may be, in like manner, rendered difficult to a physician seeing it in its new condition for the first time. This difficulty is removed by waiting for some days till the air is absorbed. In the same manner, difficulty may arise in a natural pregnancy. I have delivered a twin retained long after the delivery of its co-twin, and found myself puzzled by the tympanitic state of the abdominal tumour before its birth; the resonance being subsequently ascertained to be produced by fetid gases generated in utero. [This patient recovered.]

The extreme dilatation of the sphincter ani and its recovery of power are worthy of notice. The child's head, which passed through it, had, when collapsed, a large circumference of eleven inches, and my arm, which had also passed through it, has, at the thickest part passed, a circumference somewhat more than eleven inches.

The observation which struck me most in this curious case was the rapid absorption of the extra-uterine cyst. After delivery, it was very large, quite equal in mass to a full-sized fœtal head. Yet, before a month had elapsed, nothing was discoverable but retro-uterine hardness. It is natural to suppose that the structures had already, at delivery, been far advanced in fatty degeneration, so as to be susceptible of rapid discussion.

ARTICLE VII.—*Case of Post-Mortem Cæsarean Section.* By PETER BROTHERSTON, F.R.C.S.E., Alloa.

(*Read before the Obstetrical Society, 26th February* 1868.)

I WAS sent for hurriedly, on the evening of the 26th December 1867, about seven o'clock, to visit Mrs Martin, King's Street, Alloa, who was said to have fallen down in a fit, and those who were with her could not get her out of it.

My conveyance happened fortunately to be at the door when the messenger arrived, so that no time was lost, and I was with the patient in about five minutes.

I had been engaged to attend Mrs Martin in her coming confinement, which was daily expected, and I had the idea that her illness was in some way connected with her parturient state. When I arrived at the house, I found Dr Dickie there, and he informed me the woman was dead. Satisfying myself that such was the case, I told Mr Martin that if he wished to save the life of the child, there was a slight chance that it might still be living, and that no time was to be lost. On assuring him that Mrs Martin was dead, he at once gave his consent to my proposal to abstract the child by operation. Mrs Martin had been dead for twenty minutes. Having cleared the room of all present, excepting those who would be useful, I commenced operating by making the usual incisions recommended in Cæsarean section, and at once extracted a large healthy female child, apparently at the full time. I then blew into the mouth of the child, by placing my mouth exactly over its mouth, and holding the nostrils. I thus inflated the lungs, and a large quantity of bloody serous fluid escaped from the mouth and nose. By pressing on the sides of the chest, and alternately blowing into the mouth, I kept up artificial respiration for about a quarter of an hour. The child's arms and legs, which were quite flaccid before, now became stiff, and its heart was felt to be beating; it gave a convulsive sob or two, and I had now no doubt that it would live.

As the child was now beginning to get rather cold, I had it placed in a vessel of hot water, and continued to keep up the artificial respiration. I took a cloth, and dipped it in very hot water, and applied it to the head; this seemed to excite the breathing, and I continued to apply the hot cloths for some time. In about half an hour from the time I commenced the artificial respiration, the child was struggling and crying, and was as fine and healthy a child as I have seen.

We then proceeded to examine the condition of parts in the mother, and found that there had been separation of the placenta, for about two-thirds of its surface, from the fundus of the uterus. This separation had caused excessive hæmorrhage, and rupture of the membranes in the vicinity of the placenta, and great distention of the uterus with blood. The child was found lying in a deluge of blood. There was no escape of blood externally from the mother, and no sign of dilatation of the os uteri.

It is important to know the exact time that Mrs Martin died. Mr Martin informed me that he had just left his wife in perfect health in the house, sitting by the side of the fire, before she was seized with the fainting fit, and had gone to the workshop, which is quite adjacent to the dwelling-house. He afterwards heard the children crying in an alarmed and excited manner, and he at once ran back into the house. He found that his wife had fallen forward from the chair on which she had been sitting on her knees, with her face on the ground; her abdomen was not resting on the ground, and her arms were quite powerless under her. He at once felt for her pulse, but could find none, and she gave not the slightest signs of life by breathing or any movement whatever. In fact, Mr Martin had no doubt that she was dead, and I fully believe this to be quite true.

I have stated the time that Mrs Martin was dead before the operation to be twenty-three minutes, and I account for these minutes in the following manner:—Five minutes may safely be allowed for Mr Martin getting a messenger to send for me, another five minutes may also be allowed for the messenger coming down to my house, and another five minutes, at least, were occupied in getting from my house to Mr Martin's in my conveyance; the remaining seven minutes were taken up in urging the necessity of the operation, and performing the operation itself.

I have thought this case worthy of notice from the rarity of its occurrence. I may further add, that the child (whom I saw yesterday) continues to thrive, is remarkably healthy and robust, and is supplied with a good wet-nurse.

ARTICLE VIII.—*Case of Extravasation of Blood into the Pericardium, in which the Individual survived one hour.* By BENJAMIN BELL, F.R.C.S.E.

ON the evening of July 2, 1867, a few minutes past nine, I got a very hurried message to visit a gentleman who had been taken suddenly ill, in the neighbourhood of my house. I went immediately and found Dr Arthur Gamgee, who resided next door, already in attendance, and doing all that was possible for his relief.

The patient, a man of 42, lay on the floor, with a deadly pallor of his countenance and a cold clammy moisture on his skin. The pulse was very feeble, indeed hardly perceptible. He moaned incessantly, as if in great bodily anguish; but he appeared to be insensible, and could not be roused to answer any questions. We observed that the pupils were considerably dilated.

The friends informed us that he had been engaged in writing most of the evening, and that, having finished his work, and being about to leave the house, he was talking cheerfully, when all at once he remarked that he felt an unusual sensation coming over him, and immediately his head dropt upon his breast. They laid him on the ground, applied hot cloths to his feet, and sent for medical advice.

There seemed little for us to do, besides chafing the cold extremities, and endeavouring to get him to swallow small quantities of brandy and water. We succeeded in doing this, and then, for a few minutes, the countenance and pulse seemed to undergo a slight improvement.

His wife, who had been sent for, came in at this stage of the case; but her familiar voice could elicit no symptoms of recognition.

For some time after our first seeing him there appeared to be paralysis of the right arm, as it remained passive at his side, while the left was incessantly thrown about. Soon, however, the jactitation took place on both sides equally,—suggesting the idea of some grave internal hæmorrhage,—and he tried to turn round, first on one side, then on the other. Respiration became more frequent, and soon ceased rather suddenly, the eyelids closing. In a minute or two the latter opened once more; the pupils were seen to be *very widely dilated;* and all was over. He died as the clock struck ten, exactly one hour after the seizure.

Auscultation of the chest, I need hardly say, had been performed under great disadvantages; but Dr Gamgee, who examined the heart several times during the hour which preceded death, states that both sounds of the organ were audible until very shortly before that event. Our diagnosis, therefore, was not more precise than that already indicated—internal hæmorrhage.

We afterwards ascertained that Mr —— had been for a long time very intemperate in his habits.

The body was carefully examined the following afternoon, by Drs Gamgee, Smart, and Brunton. I was unfortunately prevented from being present. The brain was large, firm, and healthy. The sinuses were distended with blood. The pericardium contained a large quantity of serum and coagulated blood, estimated at fifteen ounces. The heart was rather large; the cavities, on both sides, empty. Fatty degeneration of the muscular texture was not discovered. The arch of the aorta was very extensively atheromatous. Large patches on the inner surface of the vessel were marked by numerous cracks and fissures intersecting one another at various angles. After a very careful and prolonged examination, no opening could be discovered from which the blood had escaped into the pericardium.

The foregoing case seems worthy of being recorded, were it for no other reason than that the final catastrophe was witnessed from first to last by two medical men,—a circumstance which, I need not say, is by no means common. The patient was not straining or putting forth any unwonted effort at the time of his seizure; and, so far as we could learn, he had no previous warning of its approach. The post-mortem appearances explain what took place, and the cause of death. Although the orifice through which the blood came was not discovered, we cannot doubt its existence. Probably it was very small and valvular, and so escaped notice beneath the semi-detached and calcareous atheromatous scales, at the root of the aorta. It may be remarked, in conclusion, that although the diagnosis of internal hæmorrhage was so far confirmed, the mere extravasation of blood need not *per se* have occasioned the fatal result. It doubtless did so, by distending the pericardium and compressing the heart.

Part Second.

REVIEWS.

British Pharmacopœia. Published under the direction of the General Council of Medical Education and Registration of the United Kingdom. London: 1867.

In many quarters, and it must be admitted with many valid reasons, the first edition of the British Pharmacopœia received but a lukewarm welcome. The mere task of assimilating the varied matter of the pre-existing Pharmacopœias of London, Edinburgh, and Dublin, imposed a labour of the utmost difficulty, and, although faults both of commission and of omission were conspicuous, the accomplishment of the first great object, with all its consequent benefits, seems to us alone sufficient to entitle the first Pharmacopœia Committee of the General Council, and the local committees associated with it, to the most cordial thanks of the profession.

Criticism, however severe, if regulated by knowledge and fairness, is always beneficial. No evidence could be adduced more confirmatory of this statement than that contained in the last edition of the British Pharmacopœia. The advice and censure of reviewers, and the suggestions and remonstrances of the profession, have been judiciously considered, with the satisfactory, and we believe almost unparalleled result in the history of such a work —that but one opinion exists as to its great merit, and that it is impossible, notwithstanding the variety of its contents, to discover in it any important error.

Preparatory to the special examination of a great national work of this nature, it would be no less useful than interesting to trace its relations to previous Pharmacopœias, and to the contemporary Codices of other countries. This, however, is for us a superfluous task, as the more salient and important points of such an examination are to be found in the review that was published, in this Journal, of the edition of 1864;[1] and we prefer, therefore, being in no way connected with the authorship of that article, to recommend its perusal, and to draw attention to the rare elegance of diction, and the extensive erudition by which it is characterized.

The present edition of the Pharmacopœia is introduced by an excellent preface explanatory of its principal characteristics. Immediately following this is a tabular arrangement of the most important changes that have been adopted; composed of such materials as a list of the articles included in this edition, but not in that of 1864—of those articles contained in the Pharmacopœia of 1864 that have now been omitted—of the preparations of which the composition has been modified—and of the alterations that have been adopted in the designations of substances. By supplying information in this condensed form, the Committee have shown that they fully realize the difficulties which are to be encountered in that comparative study that will be required on the part of the practitioner and pharmaceutist; and they thus greatly simplify and encourage careful consideration. We find the same commendable purpose more or less exhibited throughout the whole work, and conspicuous in the insertion of foot-notes to specify changes in the strength of important medicines.

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[1] Edinburgh Medical Journal, vol. ix., 1864, p. 729.

Besides such differences as were to be reasonably looked for after the careful revision to which this edition has been subjected, the most prominent innovations are the introduction of doses, and the simplification of the general arrangement of the contents. The first of these is a gracious compliance, on the part of the Committee, with the somewhat clamorous demands of the profession. The doses that are given are not authoritatively enjoined: the practitioner must still rely on his judgment and retain his responsibility in using them; but we doubt not they will be considered at least as trustworthy as those hitherto recommended on the sole responsibility of any one of the numerous annotators of our Pharmacopœia. The second important innovation is the adoption of a new system of arrangement, by which the Materia Medica and the Preparations and Compounds are arranged in one alphabetical sequence; in place of being separated into two parts, of which the one comprised the Materia Medica, and the other the Preparations and Compounds. By this plan, a great advantage is gained in the facility of reference. It is an arrangement that already exists in the Pharmacopœias of Prussia, Austria, and other countries, and it at once commends itself as obviating the inconvenience of separating designations, definitions, tests, and other important information relating to certain medicines, from the processes for their preparation.

The greatest amount of interest will probably be bestowed on the new remedies that have been introduced into the Pharmacopœia. An inspection of the list of these will show, in a most satisfactory manner, that the Committee have been guided in their selections by a commendable spirit of intelligent liberality. This list does not, however, correctly indicate the number of the new admissions. It altogether contains one hundred and nineteen names; but a very superficial examination shows that several substances are twice entered, and that a slight amount of inaccuracy has been permitted in the preparation of the list. There are, in reality, only one hundred and seven new substances and preparations, and the discrepancy is owing to the names of twelve articles being twice mentioned. Thus *Liquor atropiæ sulphatis* is mentioned immediately after *Atropiæ sulphas,* and also among the *Liquores; Unguentum cadmii* *iodidi* is placed after *Cadmii iodidum*, as well as in the list of *Unguenta; Tinctura pyrethri* is inserted below *Pyrethri radix*, and again in the group of *Tincturæ:* and a similar system of double entry has been adopted with *Emplastrum plumbi iodidi*, *Extractum physostigmatis*, *Liquor bismuthi et ammoniæ citratis*, *Liquor morphiæ acetatis*, *Mistura spiritus vini gallici*, *Tinctura sumbul*, *Tinctura veratri viridis*, *Unguentum plumbi iodidi*, and *Unguentum sulphuris iodidi.* This repetition may possess certain advantages, though it is also liable to mislead; but we object to its adoption in the haphazard style that is indicated by the occurrence of several exceptions, by no means in accordance with any rule that can possibly have guided the compilers in their selections of those articles which are twice mentioned. *Extractum lactucæ*, a preparation of *Lactuca*, is to be found among the *Extracta*, but not with *Lactuca; Glycerinum acidi carbolici* is only entered as one of the *Glycirina*, not also along with *Acidum carbolicum;* and *Syrupus rhamni*, though a preparation of *Rhamni succus*, is recognised in only the class of *Syrupi*. These inconsistencies are of a very minor character. They, however, detract somewhat from the evidence that is everywhere obtained of the exactitude with which the Committee have performed their task, and we are confident that, by alluding to them, a slight advance towards perfection will be attained in future editions.

The articles that have been introduced into this edition of the British Pharmacopœia are naturally divided into those that were included in one or more of the Pharmacopœias of London, Edinburgh, and Dublin, and those that are now for the first time promoted to an officinal position in this country. The former are forty-eight in number, and the latter fifty-nine. The re-introductions clearly illustrate the principle enunciated in the preface (p. ix.), that the Pharmacopœia has for its object, "not so much the selection as the definition of substances which the physician prescribes." The Pharmacopœia can occupy only a secondary position as a guide to the practitioner. At the same time, if it fail to recognise well-tested or favourite remedies, the use of such will continue, and risks will be incurred by the absence of exactly defined preparations, which their non-recognition permits. Such well-known and generally prescribed articles have been again admitted as *Acetum cantharidis*, *Atropiæ sulphas*, *Infusum aurantii compositum*, *Infusum gentianæ compositum*, *Liquor ammoniæ acetatis*, *Morphiæ acetas*, *Plumbi iodidum*, *Tinctura opii ammoniata*, *Tinctura quassiæ*, and *Pilula aloes et ferri*.

The recent advances in our knowledge of the therapeutic action of substances are amply recognised in remedies that for the first time appear in a British Pharmacopœia. It is, doubtlessly, a trite saying, that progress in rational therapeutics is slow, and it is easy to reproach ourselves with neglect of this important department of medical science. Progress has, however, been made; not only in a more just appreciation of already recognised medicines—with which we have here no direct concern—but also in the valuable labour of investigating the properties of such substances as are likely to prove useful to the physician and surgeon. Among the new introductions, we observe several whose physiological action is as yet imperfectly known, and whose value has therefore been discovered by analogies in their character and composition with other well-known drugs. Conspicuous among these are the salts of cerium and of cadmium, for the introduction of which into practice we are indebted principally to the recommendations of Sir James Y. Simpson. The oxalate of cerium is proposed as a substitute for nitrate of silver, and for subnitrate of bismuth; and the iodide of cadmium for the corresponding salt of lead. The latter possesses the peculiar advantages of never causing discoloration of the skin when exhibited by inunction, and of there being no risk of symptoms being produced similar to those of lead-poisoning under its prolonged use. The claims of carbolic acid as an important addition to the national Pharmacopœia could not easily be overlooked. Apart from its valuable applications in surgical practice, for which so much is due to the writings and observations of Mr Lister, it is an elegant substitute for the crude products of coal-tar that have been long employed in medicine.

Calabar bean (*Physostigmatis faba*) is also very properly admitted. Its special action on the iris, possessed by no other substance, is in itself of so great value in ophthalmic practice that there can be little doubt it will permanently retain its position in the national codex; and, judging by our knowledge of its general physiological effects, there is every reason to believe that it will soon be employed by the physician in a much more extensive manner than at present. The only officinal preparation of physostigma is the extract. We believe this to be an error. This extract can never be a constant preparation as, in common with several other vegetable extracts, it possesses the property of absorbing moisture, and it must, therefore, vary in strength—certainly an objectionable character for a preparation of great energy. We would suggest that a tincture be added in the next edition; and no tincture as yet recommended seems so likely to ensure constancy of strength as that whose formula has been given by Dr Fraser.[1] If the employment of physostigma is to be restricted to the treatment of eye-diseases, an extract is undoubtedly superior to a tincture, and for this object variations in strength within certain limits are unimportant; but, even in ophthalmic practice, the extract is objectionable as it is an extremely inelegant preparation, and one that has been almost superseded by physostigma-paper, or by gelatine impregnated with the alcoholic extract. Either of these might with advantage be recognised. We are also somewhat disappointed to find that neither of these forms of preparation have been introduced with sulphate of atropia as the active ingredient.

[1] Edinburgh Medical Journal, vol. viii., 1863.

The impetus that has been given within recent years to the study of the properties of bromine and of its compounds, has led to the discovery of various valuable and special virtues in bromide of ammonium. On account of its sedative action, this substance has been highly recommended for the treatment of various spasmodic affections of the larynx, and as a local application to precede the employment of the laryngoscope. It is extensively employed south of the Tweed and in Ireland as a palliative in certain stages of ovarian disease, and many practitioners prescribe it, with nearly unfailing constancy, in phthisis and other pulmonary affections. It has, therefore, been very properly introduced into the British Pharmacopœia.

Sumbul radix, another introduction, will probably assert for itself an important position, if we are to rely on the published experiences of Russian therapeutists, and on the statements of such physicians in this country as have tested its value. It is a drug of which we know little in Scotland, but from its resemblance to valerian, musk, and other stimulant anti-spasmodics, it promises to prove useful in hysteria and in allied diseases of the nervous system.

The most positive statements of the effects of *Veratri viridis radix* have been published by several American physicians, who, with great ability, have undertaken the task of introducing it to the notice of the profession. There are few therapeutic actions more required than that which this substance is said to exert. A cardiac sedative free from the complicated effects and uncertainties of digitalis, and having no tendency to cause the antecedent and often unnecessary action, as well as the simultaneous and usually unwished-for effects, of tartar emetic, would prove of great value. In opposition to the greater number of writers in this country who have examined the effects of *Veratrum viride*, we believe that it possesses this valuable property. A careful attention to dosage has always, in our experience, confirmed the statements of Dr Cutter and other authorities in the United States. Its most useful and efficient preparation—the tincture—has been introduced.

It is unnecessary to describe further the therapeutic properties of the other remedies that appear, for the first time, in our Pharmacopœia. We have advanced sufficient evidence of the eminently judicious character of many of the selections, and there are none whose admission could not be successfully justified.

The resources of Pharmacy have, however, been even more fully drawn from, and additions as well as improvements of undoubted value have been thereby effected. The introduction of *Liquor bismuthi et ammoniæ citratis* will be welcomed as an excellent representative of a preparation that has been extensively prescribed under the name of Schacht's *Liquor bismuthi.* The Pharmacopœia solution contains three grains of oxide of bismuth in the fluid drachm, and is, therefore, thrice as strong as that of Schacht. Both as a vehicle for the administration of other remedies, and as a convenient substitute for carbonate of magnesia, the *Liquor magnesiæ carbonatis* will soon practically demonstrate the advisability of its introduction. It is stated to contain about thirteen grains of carbonate of magnesia in a fluid ounce, but we believe that about twelve grains in a fluid ounce more closely represents its strength. Following the example of the compilers of the *Pharmacopée Française*, the Committee have introduced a *Charta epispastica*, which closely resembles the *Papier épispastique.* So many and so evident advantages are possessed by this blistering paper over the well-known, and undoubtedly clumsy *Emplastrum cantharidis*, that we almost expect to find that the latter will be expunged from future editions of our Pharmacopœia. We also consider the *Collodium flexile* a most judicious addition; and it is likewise borrowed, with a slight modification, from the rich codex of France.

Among the other introductions of a pharmaceutical character, there are several new *classes* of preparations. These are *Glycerines*, in which glycerine is substituted for fats in mixtures of various substances designed for external application; *Effervescent solutions* and an *effervescent salt;* and *vapours*, for inhalation. We think the class of vapours may be increased with great propriety. To content ourselves with one example: surely the administration of *Oleum juniperi* is now sufficiently often effected by inhalation to warrant the introduction of a formula for the purpose? It is also deserving of consideration if that important, and constantly employed class of agents—the disinfectants—should not be more disstinctly and fully recognised. We do not refer to the solid or liquid disinfectants; many of these are officinal, and there is no difficulty in employing them. The last statement cannot with propriety be made in reference to the principal gaseous disinfectants, such as chlorine, sulphurous acid, and nitrous acid. The best methods of obtaining and employing these demand a greater knowledge of practical chemistry than we can persuade ourselves is readily available to the majority of medical practitioners. A class of preparations might well be introduced under some such name as *disinfecting gases.*

Throughout the whole work there are many conspicuous improvements, which exhibit the careful and enlightened performance of their duties by the gentlemen who have been entrusted with the preparation of the more purely pharmaceutical details. These improvements are so numerous that it would be impossible to examine them within the limits of this article; we will merely refer to the introduction of benzoated lard and of oil of Theobroma as ingredients of ointments and suppositories.

There are only five articles which were included in the edition of 1864, but which have been omitted from the present one. These are *Catechu nigrum*, *Cocculus*, Nitrite of Soda, *Spiritus pyroxylicus rectificatus*, and *Unguentum cocculi.* Few, we imagine, will regret their removal.

It is a question for legitimate discussion how far a Pharmacopœia should authoritatively enjoin the processes for the preparation of officinal substances. However great may be the knowledge of pharmacy possessed by the compilers, there can be no doubt that the manufacturing chemist, influenced by the requirements of commercial profit, and assisted in many cases by a special attention to the best methods of preparing a limited number of substances, will be able to improve on almost any recommended process. A strict injunction to this effect is, therefore, as injurious to the progress of pharmacy as it is liable to evasion. At the same time, the value of a Pharmacopœia would be greatly diminished were such directions entirely omitted; as, for the majority of substances, distinct and reliable processes are required by those who have not a great practical knowledge of the subject, and who find it necessary or convenient to prepare any of them. For these reasons we believe that the Committee have acted wisely in departing from the principle, followed in the previous edition, of authoritatively enjoining all the processes. In the present edition, the directions that are given for the preparation of a considerable number of substances, such as *Acidum citricum*, *Acidum gallicum*, and *Acidum hydrochloricum*, are preceded by the clause, "it may be prepared by the following process." There are also several substances that can be prepared in only the large establishments of extensive manufacturers, and there are a few that are economically produced in a pure form only by methods discovered or retained by special chemists; for these the recommendation or injunction of a process is unnecessary or impossible. Among such substances are *Acidum nitricum*, *Acidum arseniosum*, *Acidum sulphuricum*, *Ammonii bromidum*, and *Acidum carbolicum*. With a few of these, an indication is given, in the definition, of the methods by which they are obtained; and, in all cases, a sufficient protection against impurity is afforded by the admirable descriptions that are given, and by the tests that are published for their identification.

The designations of a large number of articles have been changed in this edition. As usual, the two chlorides of mercury have discarded their recent names; and *Hydrargyri perchloridum* is now substituted for *Hydrargyri chloridum*, and *Hydrargyri subchloridum* for *Calomelas*. It may be safely affirmed that the alterations are almost invariably improvements. They relate principally to drugs and to parts of plants, and are obviously made with the object of obtaining greater correctness and certainty of meaning. Thus, *Aconiti folia* is substituted for *Aconitum*, *Anethi fructus* for *Anethum*, *Pareiræ radix* for *Pareira*, *Plumbi oxidum* for *Lithargyrum*, etc. These changes will require the careful attention of the practitioner. We may most conveniently at this place point out an error that has now been committed in two editions of the British Pharmacopœia. Nutmeg (*Myristica*) is defined as "the kernel of the seed of *Myristica officinalis*." It is not so,—it is the seed itself.

In representing bodies of definite chemical composition by symbols, the Committee were obliged to consider the relative advantages of the two systems of notation which are at present recognised. They have avoided committing themselves to either system, and have compromised the difficulty by the clumsy, but inevitable, expedient of adopting both. A difference of type has been employed to distinguish the two notations; that of the old system being printed in the lighter Roman type, and that of the new in the heavier Egyptian.

We are encouraged to hope, from evidence contained in this edition, that the pharmacopœial system of weights and measures has also approached a transition stage. In the preparation of medicines, and in prescribing, no important change has been made from the edition of 1864; the old grain weight and the avoirdupois ounce of 437·5 grains are still retained, with the fluid ounce and its subdivisions into fluid drachms and minims for the lower denominations of volume, and the imperial measure for the higher. It is now, however, optional to employ in prescriptions the symbols ʒ and ℈, the former representing 60, and the latter 20 grains. Still an important admission of the advantages of the metric system has been made by its employment, as an alternative, in the processes for volumetric analysis that are contained in the Appendix. The Committee admit that advantages would result from the adoption of one uniform system of weights and measures, "to be used alike for all substances and in all countries." Only one existing system, it is well known, promises to fulfil these requirements. They are, however, deterred from adopting the metric system by the fear of errors occurring from its being "but little used, and to a great extent unknown, in this country." Such objections are surely insufficient to warrant the rejection of a most important improvement. They might with equal justice prevent the adoption of every innovation, however advantageous; and they will always exist until the Medical Council adopt the metric as the system of the Pharmacopœia, and enjoin its use. The metric system is, of all others, the most simple; it requires no laborious study to comprehend it, and its practical details may be mastered during an extremely short apprenticeship. It is already nearly universally employed by scientific chemists, and its adoption as the system of our Pharmacopœia has been, we are confident, merely inconveniently delayed by its non-appearance as such in the present edition.

After a careful examination, we have no hesitation in characterizing the edition of the British Pharmacopœia for 1867 as an admirable exponent of the present condition of our knowledge of drugs. No Pharmacopœia has appeared, in this country, under more favourable auspices. Its preparation was intrusted to two editors of high qualification, supervised by an efficient committee of the Medical Council, and the whole work was submitted before final publication to the criticism of a portion of the medical press, and of experts specially appointed for the purpose. It is in all respects worthy of this careful preparation.

Pocketing the Pedicle: a New and Successful Method of Treating the Ovarian Stump after Excision. By Professor HORATIO STORER, of Boston.

THIS method of treating the pedicle consists in securing its extremity by metallic sutures between the inner lips of the wound, the vessels having been secured singly. The raw surface of the pedicle is thus placed in opposition to the raw surfaces of the wound, the edges of the skin are brought together, and primary union looked for. Dr Storer has tried this in a single very favourable case, in which a long thin pedicle contained only one vessel. It appears to have answered well enough, though there was still suppuration from the wound in the fourth week, and the patient would have recovered at least as well had he used the clamp or cautery, or dropped the pedicle with the ligatures into the abdomen.

Not long ago, Dr Storer had the good fortune to have one of the few successful cases of extirpation of the uterus, and he then promised us great things for fibrous tumours. We fear that his enthusiasm has again carried him too far. His method must certainly prolong the operation. In many cases it will be found unsuitable; and we should imagine that in thick, short pedicles Dr Storer will have some trouble in stopping the bleeding. Though Dr Storer has no faith in the actual cautery, there is no necessity for him to speak so slightingly of the inventor of it. There is no doubt whatever that the employment of the actual cautery in dividing the pedicle is an enormous improvement, and Mr Brown has the entire credit of it. The experience of the most successful operators in this country at this moment is in favour of the clamp in some cases, and of the cautery in others. When the clamp can be used, there is nothing better to be desired; but in short fleshy pedicles, where the clamp sometimes fails one, then the cautery answers admirably. If we take the last hundred cases operated on by Mr Wells and Dr Keith—fifty by each operator—we find that the united mortality is 15 per cent.; and, having such encouraging results to look forward to, we hardly think that operators in this country, or, as Dr Storer has it, "at home," will "pocket" their pedicles.

There are several peculiarities in Dr Storer's method of performing ovariotomy which strike one as unnecessary and not to be imitated. He thinks it safer to operate during the monthly period. He makes his incisions in the track of a rectus muscle instead of in the linea alba, and thinks that by so doing he is more certain of primary union. He closes a wound three or four inches in length by twenty iron wire sutures, and he secures the pedicle as near the umbilicus as its length will admit of, lest the rising of the uterus in a subsequent pregnancy be interfered with.

Like all Americans, Dr Storer, notwithstanding his experience of chloroform in Edinburgh, gives his preference to sulphuric ether for surgical operations. On this point his opinion is of much value. We are glad to put in a word for the old anæsthetic, with which all would have been satisfied had its younger and more favoured rival not appeared.

Rodent Cancer, with Photographic and other Illustrations of its Nature and Treatment. By CHARLES H. MOORE, F.R.C.S., Author of the "Antecedents of Cancer," Surgeon to the Middlesex Hospital, etc. Longmans, Green, and Co.: Pp. 128: 1867.

THE special opportunities afforded by the cancer wards of the Middlesex Hospital, used with untiring industry and acute observation, have enabled Mr Moore already to set his mark on the literature of cancer. This little work on the obscure subject of Rodent cancer is a valuable addition to Mr Moore's previous services.

The keynote of the book, which tells what theory Mr Moore has formed regarding the pathology, and what practical lesson he wishes to teach regarding the treatment of the disease, is struck in the preface in the following sentence.

> "In the extensive ulcerations of the face which are distinguished by the graphic epithet Rodent, I think that I recognise a cancer devoid of every one of the characters which make up our ideal of a cancerous disease, except such as are purely local. These great and disfiguring maladies, a combination of growth and ulcer, possess all the essential qualities of an ordinary scirrhus, without any or with very little of the faculty for dissemination in the body, which may towards the close of life render cancer universal."

The work naturally divides itself into two parts, first an account of the nature and treatment of the disease; second, an appendix of illustrative cases.

A brief analysis of the first part may not be out of place, as, apart from its importance in aiding the diagnosis and treatment of a disease of which we have comparatively little *exact* knowledge, it contains many suggestive observations bearing on the general pathology of malignant growths. After detailing the old, and defining the new, nomenclature of the disease, its origin and progress are described.

Beginning in some small spot of skin which had previously not been quite natural, some mole, wart, or chronic pimple, or at the edge where skin and mucous membrane join, or at the orifice of some duct, a little pimple forms and grows, in the centre of which, or possibly at one edge, a small ulcer forms. This is the *principle* of its increase, *growth*, first centrifugal, *ulceration* following in its train, but always lagging a little behind, so that the growth is to be found if looked for, even in cases where the ulceration seems the only salient fact. In different cases, and in different tissues, the relations between the growth and the ulcerations vary, and also in different parts of the same sore, thus giving the edges a peculiar serrated appearance. Different tissues yield in different proportion —skin most easily; bone very easily; cartilage much more slowly. The depth is only limited by the life of the patient. The hardness differs from scirrhus in having no contractility or tendency to drag the tissues out of their contour. Microscopic characters do not so deviate from those of the normal textures as to deserve the epithet malignant. Its duration is generally long, from five to twenty years as an average, not very exhausting, but dangerous in certain parts of the face, from the risk of opening into large vessels, or affecting brain and dura mater. It generally attacks those who have passed fifty years, and who have been otherwise perfectly healthy. The absorbent glands are rarely affected, being rather *emaciated* than increased in size :—

> "In two cases of Rodent cancer, I have had the opportunity of watching an absorbent gland, which in the course of months attained to three or four times its natural size, and assumed a hardness which would have been held to be unquestionably characteristic of cancer, if found associated with a cancer of the breast. The hardness and enlargement in one case slowly disappeared after the removal of the adjoining primary disease, and they did not recur. In the other case, the gland, which was over the splenius muscle, shrank as the disease slowly encroached on the nape of the neck."—P. 19.

Mr Moore's account of the differential diagnosis between Rodent cancer and lupus is so well done, and at the same time so characteristic of his style, as to justify quotation :—

> "The distinction of Rodent cancer and lupus is shown both in the persons whom they attack, and in the characters of the local ailment. Lupus occurs in the young adult, Rodent cancer in the decline of life. Lupus is exclusively a strumous disease; Rodent cancer originates in persons previously healthy. Whilst both diseases arise in the skin, lupus is peculiar to thin and fair integument, Rodent cancer to skin of ordinary firmness and colour. The aspect of the two diseases is different at their commencement in the skin, and not less so in their later progress. Lupus begins as a pink, low, tuberculous elevation of the skin: Rodent cancer as a firm, uncoloured nodule in it. In lupus there may be more than one tubercle, and the intervening skin may be healthy, or pink, or scaly, or œdematous—the pimple of Rodent cancer is solitary. The surface of lupus first scales or peels before it breaks; the Rodent cancer excoriates, and then scales or bleeds. Both ulcerate; the lupus at one or at several of its tubercles, the Rodent cancer by the mere deepening of its central scabbed excoriation. Lupus may cicatrize and cease at any time; Rodent cancer proceeds with at most but a temporary and partial healing near its edge. When both are far advanced, the lupus has a superficial appearance, though it have destroyed the whole nose; Rodent is precipitous and excavated. Lupus possesses, Rodent cancer is without any, contractility. The margin of lupus, though thickened, is low, and bevelled both outwards by œdema, and inwards towards the shallow ulceration; that of Rodent cancer is firm, and is commonly,

> in both directions, abrupt. The ulceration of lupus is smooth, and may be multiple, being divided by scars; that of Rodent is single and ragged. In the vicinity of lupus, there are separate, rather soft tubercles, and an area of pink, scaly integument; around the Rodent disease the skin is healthy; and, if a separate nodule do exist, it is compact, firm, and in great part subcutaneous. Lupus is not invariably limited to the face, but may at the same time appear on the hands or elsewhere; Rodent cancer is eminently local and centrifugal. The most virulent lupus may, though it rarely does, cause death; Rodent disease is always eventually fatal. Like an old issue, or a chronic ulcer, lupus is liable after many years to a rapid growth of epithelial cancer in its ulcers and scars; the march of Rodent cancer may be accelerated towards the end, but its character is not thereby altered."—Pp. 21 and 22.

He next compares the Rodent disease with cancer, and points out that, while resembling cancer in its property or power of indiscriminately invading all the tissues with which it meets, and in its never being spontaneously curable, it varies from it,—first, in not infecting to so great an extent the apparently healthy tissues in the neighbourhood, as a cancer so often does; and, second, in the extreme rarity of an affection of the lymphatics. In the following very suggestive sentence, Mr Moore gives a theory explanatory of the lymphatic escape:—

> "But it may be that there are causes in the nature of the disease itself which explain its failure to infect the glands. The matters which it yields to them are not viable. In the growth itself there are not usually masses of exuberant and rapidly growing cells, but such as at best have but a feeble and precarious life; all the less, therefore, can these fragments of it which may be dislodged retain any power of further development in the new and less propitious circumstances into which they may be transplanted. They have never any of that activity of growth which is characteristic of ordinary cancer in glands, but are only those scanty, liquid, and effete parts of the primary disease which, as they enter the lymphatics and traverse the glands, are eliminated without harm, and without being noticed in their transition." —P. 35.

On this subject there are several very interesting conclusions drawn regarding the various explanations which may be given in different cases of the occasional escape from gland infection in primary cancer, but for these we must refer to the book itself.

The question of treatment is next discussed. Caustics or excision are recommended in mild recent cases, with a favourable prognosis. The most interesting part of this section is the account of Mr Moore's treatment in one or two cases, which from their duration and extent seemed hopeless. A union of very free and bold excisions, with the use of potential caustics, gave a wonderfully successful result, successful both in prolonging life and diminishing deformity.

Full details of these operations, their dangers and difficulties, will be found in the appendix of cases.

The work is nicely got up, and illustrated with five photographs and four woodcuts.

Though, from the limited nature of its subject, it may not have a very extensive circulation, we are certain that all surgical teachers as well as all hospital surgeons should read, and, having read, will thank Mr Moore for this really instructive work.

A Contribution to the History of the Hip-Joint Operations performed during the late Civil War, being the Statistics of Twenty Cases of Amputations and Thirteen of Resections at this Articulation, in the Southern Service. By PAUL F. EVE, M.D., Professor of Surgery in the University of Nashville, Tenn. Philadelphia: Collins. Pp. 17: 1867.

THIS little pamphlet contains a brief notice of all the cases that could be obtained in which amputation at the hip or excision of the hip-joint had been performed during the late war on the southern side. From the dangers of the campaign, many of the operators are now dead, and the notes of others have been lost, so the cases are brief and imperfect. Of the twenty amputations, four certainly recovered, and in a fifth the issue was doubtful. Of thirteen resections, five were successful. Of twenty cases of amputation reported on the northern side, only two survived, and of thirty-two cases of resection or removal of the head of the femur, only four recovered. Dr Gilmore, who amputated three times and excised three times, reports,—" I believe that one-third of the cases of gunshot wounds of the femur, usually supposed to require amputation at the hip-joint, will recover, if left entirely to the efforts of nature."

Pathological Anatomy of the Female Sexual Organs. By JULIUS M. KLOB, M.D., Professor at the University of Vienna. Translated from the German by JOSEPH KAMMERER, M.D., and BENJAMIN F. DAWSON, M.D. New York: 1868.

THE tendency to specialization in books as in practice increases. The present treatise, though the first of its kind, is one that might have been naturally expected. For a great degree of specialty everywhere now attaches to the study and practice of the diseases of females. The work is one which, if even of moderate merit, will be well received by gynekologists. They will expect in it a handy compendium of the special pathology of the human female. Now we very willingly allow that M. Klob's work has considerable merit. It is evidently the work of a gentleman of extensive knowledge, and research, and ability, and we may expect that, if it reaches a second edition, it will be worked up into a higher state of completeness.

This is only the first volume of Klob's work; it describes affections of the uterus. The next should contain diseases of the Fallopian tubes and ovaries, and of the vagina and vulva. We begin to feel some alarm for its appearance, for there is no announcement when it may be expected, and there has already elapsed a long time since the publication of the first volume.

We have already hinted that the work is not quite complete. In the present volume, we desiderate a good deal of important matter, which ought to have been in it. For instance, the inflammation and some of the degenerations of fibrous tumours are not given. There is a very imperfect description of uterine polypi. The diseases of the decidua are mostly ignored.

We hope soon to return to M. Klob, when the second volume reaches us, whether in German or in translation. The translation of the first volume is praiseworthy. But there are a great many minute errors.

Injuries of the Spine, with an Analysis of nearly Four Hundred Cases. By JOHN ASHHURST, Jun., A.M. M.D., Fellow of the College of Physicians of Philadelphia, etc. Philadelphia: J. B. Lippincott & Co. London: Trübner & Co.: 1867. Pp. 127.

THOUGH this work is a small one, it must have cost the author a very great deal of both time and trouble, and its value must not be gauged by its size. It consists of a collection, arranged in a tabular form, of 394 cases of spinal injury—cases where fracture or dislocation, or both, had either been made out by examination after death, or diagnosed with more or less certainty during the life of the patient; and, secondly, of a statement of the results gleaned by very careful comparison of these cases, results bearing on the *Causes*, *Position*, *Symptoms*, and *Treatment* of Spinal Injury.

Severe accidents to the spine are comparatively so rare, that it is difficult for any single individual to find, in his own experience, materials sufficient to enable him to form a well-considered opinion regarding many of the above points; hence the value of such a synopsis or bird's-eye glance at the surgical experience of centuries as is here given. Many of the results are arranged in a tabular form, thus giving the data from which conclusions may be drawn. For example, the following table gives the frequency with which the different regions of the spine have suffered:—

Cervical region alone affected, . . .	208	or	52·79	per cent.
Cervical and dorsal regions affected in .	4	,,	1.02	,,
Dorsal region alone affected in . . .	106	,,	26·90	,,
Dorsal and lumbar region affected in . .	20	,,	5·08	,,
Lumbar region alone affected in . .	37	,,	9·39	,,
Region not clearly indicated, affected in .	19	,,	4·82	,,

This result does seem fallacious, so far as the excessive proportion of injuries of the cervical portion is concerned. Mr Ashhurst explains this in the following sentence, which shows that, notwithstanding the energy with which he can work at statistics, he can see their possible fallacies.

> "The large proportion of recorded injuries in the cervical region is, I believe, to be accounted for on the ground that these cases being but too often rapidly fatal, excite naturally a great deal of interest in the mind of the observer, and are, therefore, more frequently thought worthy of publication than those of injury in the dorsal and lumbar spine, which linger on from month to month, passing successively under the care of different surgeons, and looked upon by each as equally tedious and unsatisfactory."

Of the ascertained causes, falls (43 per cent.) and blows (15 per cent.) are much the most frequent. The causation is very carefully detailed, and special tables given of the different causes in the different regions.

Of *symptoms*, the most striking and characteristic are the lesions of sensation and motion. The completeness and permanence of the loss of sensation is well brought out by a reference to a case recorded by Dr Purple in the New York Medical Journal for 1853. The patient, several years after an injury in the dorsal region, insisted upon having both his thighs amputated close to the hip-joint, because he regarded them as useless appendages. He did not take chloroform, and absolutely felt nothing.

While the author describes with sufficient detail the other important symptoms, such as dyspnœa, sloughing, retention and incontinence of urine, and the like, we find that, from the insufficient and bald way many of the cases he has collected have been reported, much of the value of the others is lost; as it is impossible to tell whether any given symptom has been absent or merely omitted by the narrator.

On the *prognosis* some excellent observations are made, showing what is well known to every hospital surgeon of experience, that even severe spinal injuries are not nearly so fatal as it is imagined. The following paragraph deserves quotation at present:—

> "A high authority in experimental physiology, who has lately taken up the defence of Cline's operation of spinal resection, has placed the proportion of cases which survive severe spinal injuries under the ordinary treatment at 'perhaps less than 1 per 100.'[1] I have been unable to find any reference to the cases on which the writer's statistics are based; certainly in the cases, the histories of which I have examined, the mortality has not been nearly so large."

Had we room, an interesting table of results might be quoted; we may give briefly the general bearing:—

In injuries of the	cervical region,	77	per cent.	died.
,, ,,	dorsal ,,	63	,,	,,
,, ,,	lumbar ,,	59	,,	,,
,, ,,	region not stated,	26	,,	,,

The author regards the following symptoms as very important as furnishing grounds for prognosis:—

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[1] Brown-Séquard, Lectures on the Central Nervous System, p. 259.

"*Dyspnœa* is a most unfavourable symptom: coming on a few days after the accident, it generally indicates progressive upward disorganization of the cord, and even when arising from other causes, tends of itself to a fatal issue by the accumulation of mucus in the air-passages, the consequent deficient aëration of the blood, and finally the induction of coma.

"*Flushed Face* (unless from coincident pneumonia or such other cause) is a symptom of the gravest import: it indicates serious lesion of the cord in the cervical region.

"*Increased animal temperature* and *priapism* are also serious symptoms, especially the former."

The tables bearing on the duration of life in the fatal cases confirm the well-known idea that the higher up the injury the more rapid the issue.

Of 164 fatal cases in the cervical region, 109, or 64 per cent. died in less than a week; while of 82 in the dorsal region, only 16, or 19 per cent. died in the same stage; and of 34 lumbar ones, 10, or 29 per cent. died. On the question of treatment, Mr Ashhurst gives many interesting tables, and from them draws two principal conclusions:—

1. That extension (combined with rotation or pressure if required) should be employed in every case of dislocation of the spine, or of fracture with dislocation.

2. That resection of the spine is an operation not to be recommended, from its extreme fatality.

We fear that the author is too sanguine in his ideas of the extension plan, as statistics here are apt to mislead, an operator being far more likely to report a successful case after manipulation than one in which it had failed.

On the whole, this little work seems to us of very considerable value. The immense mass of material accumulated will prove a storehouse from which, with much less trouble, other authors will draw facts and theories. It is clearly, pleasantly, and modestly written; the statistics are not tortured to support fancies, or taken for more than they are worth; in fact, they are used, not abused.

Photographs of Eminent Medical Men of all Countries, with Brief Analytical Notices of their Works. Edited by WM. TINDAL ROBERTSON, M.D., M.R.C.P., Physician to General Hospital, Nottingham. The Photographic Portraits from Life, by ERNEST EDWARDS, B.A. Cantab. No. 8, Vol. II. London: Churchill and Sons.

IT is a legitimate and, to a certain extent, a commendable curiosity which seeks to become acquainted with the lives and labours of contemporary men of eminence. During the lifetime of the subjects of notice, a certain delicacy and reticence is no doubt required on the part of the biographer; but, in judicious hands, this does not materially interfere with a faithful and even a full account of all those particulars which would interest the reader. The aid of photography affords evident facilities for illustration of the most valuable kind, superior in truthfulness, and much more effective than the engravings and lithographs which were used in former years for similar purposes.

The work now before us consists of photographic likenesses of the more eminent members of the medical profession; and each portrait is accompanied by a short memoir of the life and writings of the individual represented. The first volume has already appeared, containing portraits and memoirs of Sir Thomas Watson, Professor Owen, Sir James Clark, Sir Henry Holland, Sir Ranald Martin, Dr Burrows, Dr Acland, Sir W. Fergusson, Mr Bowman, Professor Donders, and other celebrities. The second volume includes Professor Huxley, Dr Bence Jones, Dr Carpenter, the late Dr Hodgkin, etc.

These are all names highly distinguished, and worthy of a place in such a work. We shall not be accused, however, of excessive nationality if we say that certain names familiar on this side of the Tweed are conspicuous by their absence from these two volumes. This deficiency may perhaps afterwards be supplied, or there may be reasons which we know not of that prevent their appearance, but meanwhile the series is very incomplete without them. Professor Donders is hitherto the only representative of the foreign element among the medical men "of all countries."

Number 8, which concludes the second volume, presents characteristic likenesses, and short but interesting notices of Dr King Chambers, Mr Druitt, and Dr Moore of Dublin. The photographs are exceedingly good, and the work, as a whole, is very nicely got up, and fulfils excellently the purpose for which it is designed. We recommend it to the attention of our readers as a pleasant and useful addition to their libraries, introducing them *quasi*-personally to the distinguished men with whose writings the profession has long been familiar.

On Certain Moral Aspects of Money-Getting. By W. T. GAIRDNER, M.D., Professor of the Practice of Medicine in the University of Glasgow. Maclehose, Glasgow. Edmonston and Douglas, Edinburgh. 1868.

ALTHOUGH not treating of a medical subject, Professor Gairdner's essay on the morality of money-making has, for several reasons, a special interest to the medical profession. It will be remembered that, in the course of his graduation address of 1867, the author enunciated his views on certain differences between professional and mercantile pursuits with reference to the acquisition of wealth, which excited some attention and controversy at the time. The present paper contains a fuller and more explicit treatment of the subject, addressed not to a medical, but to a mixed and non-professional audience. Professor Gairdner discusses the question from the points of aspect furnished by history, by political economy, and by ethics, with the purpose of discovering the real moral element which ought to characterize money transactions, whether in the pursuits of business or in the practice of a profession, so as to deprive the earning or making of money of mercenary motives and tendencies. We have not space to give his argument in outline, but it may be stated that the principle of service is that on which he insists. The following brief extract will give some idea of the elevated tone of the essay, and will serve to indicate the nature of the conclusions at which the author arrives.

> "It seems, therefore, clear, whether according to the principles brought out in this paper, or according to the highest maxims of the Christian religion itself, that the nobility of an occupation must be tested, not by its conventional rank, and its freedom from externally humiliating conditions, nor by its 'success' in the sense of the profits it yields, and the mastery it gives over others, but, on the contrary, by the services it renders, the extent to which it adds to the happiness, diminishes the bodily sufferings, or advances the spiritual welfare, of mankind. And by adopting this principle as the guide of our commercial intercourse, by studying not so much how to *exact* service as how to *give* service, we shall best redeem the pursuit of wealth from the stigma that was attached to it throughout the ancient world, and which is only half withdrawn in the New Testament. And we shall thus also, and thus only, be enabled to fulfil the duties, as well as to enjoy the real privileges belonging to wealth already acquired."

As no topics are of more general importance than the moral and social status of our profession, the opinions which Dr Gairdner advances will be perused with interest. On the moral aspects of gain and remuneration, his essay is exceedingly complete and able; and it will serve to correct any erroneous impressions to which the more partial statement of his opinions in his graduation address may have given rise in some minds. The more practical questions of the adequacy of professional remuneration, and the position which the medical profession occupies as to emoluments or honours, which excite so much discussion at the present day, are not within the scope of Dr Gairdner's pamphlet; but the considerations he lays before his readers indicate the moral grounds which must not be lost sight of in the settlement of such questions.

A System of Medicine. Edited by J. RUSSELL REYNOLDS, M.D., F.R.C.P., London, Professor of the Principles and Practice of Medicine in University College, Physician to University College, and to the National Hospital for the Paralyzed and Epileptic, etc., etc. Vol. II. London: Macmillan and Co.: 1868. Pp. 971.

WE are glad to announce the publication of the second volume of the System of Medicine, which has been for some time expected. Owing to the recent date of its appearance, we cannot do more at present than refer to the nature of its contents, and must reserve for a future occasion the fuller examination and criticism of the different articles. The greater part of the volume is occupied by a description of diseases of the nervous system; towards the end, the diseases of the digestive system are commenced by a treatise on Diseases of the Stomach by Dr Wilson Fox. As we have recently noticed this author's work on the same subject, it will be unnecessary to repeat the commendation we expressed. In regard to the diseases of the nervous system, when we consider the progress that has been made in their study in recent times both at home and abroad, the appearance of the present volume is especially well timed. The articles are contributed by authors already known to the profession as having enjoyed peculiar opportunities of observation, and as having paid special attention to the subjects on which they write. The volume commences with a treatise on Insanity by Dr Maudsley; Epilepsy and Diseases of the Brain are from the pen of Dr Reynolds; Diseases of the Spinal Cord, Chorea, etc., are described by Dr C. B. Radcliffe; Diseases of the Meninges by Dr Ramskill; Tubercular Meningitis by Dr Gee; Convulsions and Cerebral Hæmorrhage by Dr Hughlings Jackson; Hypochondriasis and Abscess of the Brain by Drs Gull and Sutton; Neuritis, Neuroma, and Local Paralysis, Spasms, and Anæsthesiæ, by Dr Warburton Begbie; Neuralgia and Alcoholism by Dr Anstie; Ecstasy, Catalepsy, and Somnambulism, by Dr Chambers; Sunstroke by Professor Maclean; Wasting Palsy by Dr Roberts; Paralysis Agitans and Metallic Tremor by Dr Sanders; Epidemic Cerebro-spinal Meningitis by Dr Netten Radcliffe, etc.

It is almost unnecessary to add, that the volume, together with much original matter, represents the present state of our knowledge on this important class of diseases, and furnishes a valuable summary for the student and the practitioner. Among practical points, the testimony borne to the efficacy of bromide of potassium in epilepsy is conspicuous. We shall take an early opportunity of noticing the different points of interest presented by the present work in connexion with the recent advances made in the discrimination and treatment of nervous diseases.

Part Third.

MEETINGS OF SOCIETIES.

EDINBURGH MEDICO-CHIRURGICAL SOCIETY.

SESSION XXVII.—MEETING III.

15*th January* 1868.—Dr OMOND, *President*, in the Chair.

Dr Omond, on taking the chair, returned thanks for the honour done him by his election to the Presidency of the Society.

I. REMOVAL OF PINS.

Dr Joseph Bell exhibited a breast-pin which he had removed a few days before from the pharynx of a little girl æt. 15 months. The accident had happened about an hour before he saw the child, and suffocation seemed imminent. The finger introduced into the mouth soon felt the pin, the sharp point of which had passed deeply into the right tonsil, while the head, which consisted of a ring of metal set with eight pieces of cut and angular glass, was pressing on the larynx, the epiglottis being easily felt projecting upwards through the ring. From its position, the risk was evident that any forcible attempt to lift out the head might make the point penetrate still deeper into the tonsil, and perhaps wound the internal carotid. Putting the child deeply under chloroform, it was not difficult, by a pair of strong dressing forceps, so to bend the shaft of the pin as to withdraw it without further injury. The child never had another bad symptom.

Dr Joseph Bell also exhibited a pin about 2 inches in length, which he had removed from the rectum of a girl about 13 years old. She had swallowed the pin about a fortnight before, but as for a fortnight no symptoms appeared, it was supposed she had passed it unconsciously. At the end of that time, however, she felt severe pain in the rectum just above the anus, which was aggravated by walking or by attempt at defecation. On examination, the head of the pin was found just within the sphincter, and the point had penetrated the opposite wall pretty deeply. It was then removed, and no further symptoms followed; the pin was a very large one.

II. BOOT FOR CLUB-FOOT.

Mr Annandale showed a boot for the treatment of talipes varus, by which the elevation of the heel and the inversion and rotation of the foot could be counteracted by simple mechanical means.

III. URETHRAL CALCULUS.

Mr Annandale also showed a urinary calculus the size of a small horse-bean, which had passed per urethram from the bladder of a boy aged 5. The boy had been repeatedly sounded, but the stone was only detected once. Two days after the last sounding the stone was passed.

IV. SYPHILITIC NECROSIS OF SKULL.

Mr Annandale exhibited a calvaria affected with syphilitic necrosis. One large portion of bone had separated before the patient's death, and other two portions were in the process of separating.

V. SHOULDER-JOINT.

Mr Annandale also exhibited a shoulder-joint affected with scrofulous disease. The case was interesting as having given rise to very few symptoms during life. The shoulder was much flattened, and on dissection this was found to be due to almost entire destruction of the head of the humerus, which lay in close contact with the glenoid cavity.

VI. SPRAY PRODUCER.

Dr Cheyne exhibited a spray producer with double action, which had been made by Mr Gardner, and which presented certain advantages over those commonly in use.

VII. WOOD'S SYRINGE.

Dr Grainger Stewart showed a Wood's syringe for hypodermic injections, in which the rod of the piston was graduated, and was moved by means of a screw, so as to determine accurately the amount of fluid discharged.

VIII. *Dr Sanders* read a paper on a

CASE OF POISONING WITH THE RHUS TOXICODENDRON.

Which will be found at p. 714.

Dr Fraser remarked, that the case presented considerable interest, as recent researches in Germany had, by the negative results of experiments, tended to throw doubt on the fact that symptoms of poisoning were produced by the Rhus.

IX. *Dr Joseph Bell* read a paper on

A CASE OF NECROSIS OF THE FEMUR, INVOLVING THE WHOLE THICKNESS OF THE SHAFT, WITH DISEASE OF THE KNEE-JOINT. RECOVERY WITH A USEFUL LIMB.

Dr Bell introduced the patient for inspection by the members of the Society.

X. *Dr Arthur Mitchell* read a detailed and instructive paper on

THE BODILY AND MENTAL STATE OF THE PREGNANT AND NURSING MOTHER IN CONNEXION WITH IDIOCY IN HER OFFSPRING.

SESSION XXVII.—MEETING IV.

5th February 1868.—Dr Omond, *President*, in the Chair.

I. *Dr Halliday Douglas* showed a preparation of a

TUMOUR IN THE HEART.

This case will be found at page 908 of the present number.

II. *Dr Watson* exhibited a specimen of cancer of the bladder in a female.

III. *Dr Watson* showed the preparations of the arteries in a case of spreading gangrene. Their coats were diseased, and the interior obstructed by clots.

IV. *Dr Watson* described a specimen of exostosis of the femur with a bursa enclosing it. At an early period of the case, when the bursa was filled with fluid in consequence of inflammation, and the swelling extended rapidly up the limb, the appearances presented had, to some extent, simulated malignant disease.

V. *Dr Watson* also showed specimens of a tumour of the upper jaw, and of tubercular disease of the testicle.

VI. *Dr Grainger Stewart* showed a recent specimen of

TUBERCULAR ULCERS OF THE STOMACH.

The intestines were also the seat of ulcerations.

VII. *Dr Watson* read a paper on a

CASE OF INTESTINAL CONCRETIONS.

This will appear in an early number.

VIII. *Mr Annandale* then read a

CASE OF RUPTURED PERINÆUM,

with prolapsus vagina, cured by operation.

IX. *Dr Grainger Stewart* then communicated, from Dr Bryden of Hawick, the details of

A FATAL CASE OF INTUSSUSCEPTION OCCURRING IN AN INDIVIDUAL WHO HAD SUFFERED TWO PREVIOUS ATTACKS.

The morbid specimen was also shown.

X. *Dr Matthews Duncan* read a paper on

THE EXAMINATION OF THE BLADDER,

which will be found at page 797.

SESSION XXVII.—MEETING V.

4th March 1868.—Dr Omond, *President*, in the Chair.

I. FIBRINOUS POLYPUS.

Dr Matthews Duncan exhibited several polypi composed entirely of decolorized fibrin. They were rounded and pediculated, and as to their nature there could be no doubt, whatever might be their origin. The woman had had a miscarriage fifteen months before the polypi were removed; but no connexion of any kind could be established between the miscarriage and the present disease. There was a much larger polypus of the like kind removed and broken up before those exhibited were taken away. This large one distended the cervix, so that the os through which it projected, was as large as half-a-crown. Besides the polypi, the uterus contained much dense fibrin, which, like the polypi, was almost as dense and tough as cartilage. The woman suffered much from hæmorrhage, and was extremely anæmic. Since the operation a month ago, she has progressively and rapidly recovered, and the bleeding has not returned. The uterus also is reduced in dimensions to nearly its natural size.

II. TRUE FIBROUS POLYPI.

Dr Matthews Duncan exhibited two large fibrous polypi which he had recently removed. The smaller had a complete capsule of muscular tissue and mucous membrane. It had several lacunæ filled with old chocolate-coloured blood. Its history was interesting, inasmuch as it could be made out to have produced large bleedings only since it descended into the vagina. The larger polypus had also a capsule of mucous membrane and muscular tissue. It produced very large bleedings, which generally came on suddenly. It was removed by scissors. It contained several large lacunæ filled with old bloody fluid, chocolate-coloured in the higher, but gradually becoming redder as the lower lacunæ were reached. The lacunæ communicated with each other, and the lowest (whether through uterine sinuses or not) communicated with the cavity of the vagina. Over the lower part of the tumour was a superficial ulceration covering an area equal to that of a crown piece. On the surface it was easy to discover several openings into venous sinuses which surrounded the fibrous tumour forming the polypus. Communication could also be made out to exist between the sinuses and the lowest lacunæ wherein the reddest blood was. These anatomical details might throw some light on the teleangiectatic myoma of Virchow.

III. SUB-CONJUNCTIVAL OSSEOUS TUMOUR.

Dr Argyll Robertson exhibited a small osseous tumour 3 lines long by 2 lines in breadth, of an oval flattened form, which he had removed from the eye of a girl 7 years of age. The tumour was invested with a capsule, and was situated beneath the conjunctiva about 3 lines from the outer margin of the cornea. It was intimately adherent to the conjunctiva, but was only loosely attached to the deeper textures. On microscopic examination, it presented all the characters of true bone.

IV. STUMP OF AN EYE.

Dr Argyll Robertson exhibited a stump of an eye removed by Mr Walker from a patient on account of sympathetic disease of the other eye. The stump resulted from an injury sustained four years previously, which had given rise to suppurative inflammation. The sympathetic irritation of the other eye only supervened towards the end of last year. On cutting through the coats of the removed stump, no fluid contents were found, but the interior was occupied by a calcareous plate which rested on the entrance of the optic nerve.

V. *Dr Brackenridge* exhibited a form of

SPRAY PRODUCER,

which he recommended.

VI. *Dr Watson* showed a specimen of disease of the os calcis, from a case in which he had performed excision of the entire bone.

VII. *Dr Watson* also exhibited specimens of four calculi, of considerable size, removed from patients upwards of 60 years old, by the operation of lithotrity, during the last few months.

VIII. *Mr Benjamin Bell* read an

ACCOUNT OF THREE CASES OF CONGENITAL CATARACT IN ONE FAMILY, CURED BY OPERATION.

This will appear in next number.

IX. *Mr Bell* next read the description of a

CASE OF EXTRAVASATION OF BLOOD INTO THE PERICARDIUM, IN WHICH THE INDIVIDUAL SURVIVED ONE HOUR.

which will be found at p. 932.

X. *Dr Watson* read a paper on the

REMOVAL OF FOREIGN BODIES FROM THE FEMALE BLADDER.

XI. *Dr Matthews Duncan* read a paper on

PELVIC AREOLAR INFLAMMATION AND SLOUGHING.

These papers will appear in an early number.

PROCEEDINGS OF THE EDINBURGH OBSTETRICAL SOCIETY.

SESSION XXVII.—MEETING VI.

6th February 1868.—Dr Burn, *President*, in the Chair.

I. CASE OF PROLONGED GESTATION. BY DR JAMES YOUNG.

Mrs S., Canongate, was married in the spring of 1848. She had an abortion in January 1849. She menstruated in February, and every month thereafter till 9th July of the same year (1849), and continued till the 14th of the month. At the end of July she complained of sickness, which continued for four months. Fœtal movement was distinctly felt on the 17th of November. Allowing the full time to expire, she expected her confinement about the 24th of April 1850, being nine months and ten days from the cessation of the last menstruation, or five calendar months and a week from the time of quickening. Mrs S. was delivered of a large child on the 3d day of June 1850, being 326 days from the time the last menstruation had disappeared, or 201 days from the time fœtal movement was first felt.

In order that the dates may be more accurately depended on, let me mention that Mrs O. (the sister of Mrs S.) menstruated on the 9th of August 1849, one month after Mrs S., and calculated on her confinement one month later than her sister; whereas Mrs O. was confined on the 18th of May 1850 (nine months and nine days from commencement of menstruation), seventeen days before Mrs S. Both sisters kept particular note of the dates.

Dr Pattison remarked that the case just related might have formed a nice question for medico-legal discussion.

Dr Cairns had met with a similar case in practice. The woman was the mother of four children, and was not confined for about five weeks after the date she expected; and there was nothing in the appearance of the child to indicate that it was over the usual time. He found that usually women were not very particular in noting the menstrual periods, and their information could not always be relied on as correct.

Dr David Gordon was of opinion that a discharge resembling menstruation sometimes occurred after the woman became pregnant; and, on the other hand, he knew of a case in which conception took place six months after menstruation had ceased.

Dr Charles Bell did not believe that true menstruation ever took place after pregnancy. In regard to the duration of pregnancy, the French law allowed three hundred days, and the Prussian three hundred and one days as the longest possible period, and cases were on record in which gestation had been prolonged to the forty-fourth and forty-fifth weeks. In making a series of observations on this subject, he had been struck by the fact that in most of the cases in which the length of pregnancy had been carefully watched, it did not extend beyond two hundred and seventy-five days.

Dr Bryce said that the whole question came to be on what grounds the date of conception was fixed. It was well known that women may conceive who have never menstruated at all. He had been engaged to attend a patient in her confinement on a certain date, but he was not called for about two months after, and in this case the last period had been carefully noted. He believed that many women could tell within a week when conception takes place; but it is always a difficult matter to make out a genuine case of prolonged gestation.

Dr Macdonald asked Dr Young whether, in the case related, any careful examination of the time when the fœtal heart was first heard had been made, and whether any notice had been taken of the ossification of the child's head.

Dr Burn stated that he never put much faith in what women said as to the date of the fœtal movements, for he had seen many cases in which movements had been said to exist where there was no child at all.

Dr Menzies said that he had a patient, the mother of twelve children, who almost always can tell to a day when she is to be confined. She always took a note of the time when movements of fœtus were first felt, and calculated from that.

Dr Cochrane had had a patient who presented all the indications of pregnancy, and got very large; but, on examination, no child was found to exist.

Drs Dickson and *Cappie* mentioned similar cases.

Dr Ritchie had seen a case in which the patient succeeded in deceiving not herself only, but the midwife also. When called in, he found that there was no pregnancy, and that the woman had been menstruating quite regularly. He had seen another case of a similar nature, which might have led to a medico-legal inquiry. It was alleged that an assault had been committed by a man upon a young girl. The man denied the accusation; but, in the course of time, received a letter from the girl intimating that she was in the family way, and had felt the movements of the child. Dr Ritchie saw her, and found that she was not pregnant, and had been menstruating regularly all the time; but, notwithstanding his opinion to the contrary, she persisted in thinking that she was with child. and even gave up her situation in consequence.

Dr Cairns asked the opinion of the Society in regard to the case of a child, aged ten years, a patient of his, who was said to have menstruated. She was a delicate weakly child, and he wished to know if it was likely to be true menstruation.

Dr Burn said that, according to his experience, menstruation in such young girls generally appeared only once or twice, and left as they got stronger.

Dr Bell agreed in this observation.

Mr G. Stevenson Smith mentioned a case which had come under his notice recently, in which there was a regular monthly discharge, resembling true menstruation, the child being only six years old.

II. CASE OF CONGENITAL IMPERFORATE URETHRA IN A MALE CHILD.

BY DR DAVID MURRAY.

When we remember the highly important functions which the genito-urinary organs have to perform, we can easily perceive the injurious effects which will result from any derangement of their functions, whether arising from malformation or other causes. We have ample proof that the neglect of these congenital defects of the urinary organ, or their imperfect treatment, if not immediately fatal, has laid the foundation of a train of symptoms and organic change, which has had the effect not only of rendering the individual miserable during life, but also of greatly shortening his days. Now, though in many of the worst of these defects, such as extroversion of the bladder, we cannot entirely cure—we can, at least, do much to alleviate and render the individual comfortable; in other cases, and these constitute the large majority, we can hold out the prospect of a speedy, effective, and permanent cure. On the accoucheur, it is peculiarly incumbent to know these cases thoroughly, both as regards their cause and treatment. To him they are first of all presented; and on him it very much depends if the patient will apply in sufficient time, before injurious consequences ensue, at a period long antecedent to that when the surgeon's aid will be invoked. Timely counsel from him, therefore, is of the utmost consequence. At some future time, I intend to bring before the Society the other congenital malformations and derangements of these organs. Meantime, I will confine your attention to a few general remarks on the deformity which came under my eye, and a narrative of the case which presented itself to me.

I may, then, make a few remarks on the obstructions connected with the urinary canal, the other parts of the organ being well formed. This class of malformations is very rare indeed. We have no cases recorded in which the urethra had been obstructed all through. The most general form is that in which a sort of membrane covers and entirely occludes the passage. This structure may be either membranous or fibrous in composition. It may vary both as regards extent and thickness. Sometimes it is so strong that it can only be overcome, if at all, by considerable force; at other times it may be ruptured with the greatest ease. In regard to its position, it may be situated at any part of the canal.

1. The most common situation is that immediately over the external orifice.
2. The second is that situated towards the vesical orifice. The catheter may be introduced a certain distance, but cannot be passed into the bladder.
3. The third locality is immediately behind the bulb, about an inch from the orifice. This form is very rare.

In regard to symptoms, the complete retention of urine is sufficient to indicate the necessity for prompt interference. This may vary. In those cases where the obstruction is merely orificial, an incision with a sharp-pointed bistoury, and the introduction of a small bougie or catheter occasionally, if there is any risk of adhesion. When the obstruction is situated farther down the canal, a probe-pointed bougie may be pushed through the obstruction, or, if this is not possible, the urethra must be opened by an incision in the perineum, or the bladder must be penetrated.

The case illustrating this class of obstructions occurred in my own practice, and is as follows:—A. A. was born on the 2d day of November at 10 A.M. At birth nothing abnormal presented itself on a superficial view. On calling, however, some twenty-four hours afterwards, I was informed by the nurse that the child had passed no water. On proceeding to examine, I found that the penis and scrotum were well formed, but I could not discover any external opening. Considering that it belonged to the first class of cases, those with a membranous obstruction over the orifice, with a sharp-pointed bistoury I made an incision, after which I endeavoured to pass a catheter with considerable force. On that occasion, however, I failed to pass it. On calling a few hours afterwards, I was informed that the child was still unrelieved, and was suffering much. Under these circumstances, it was thought advisable to call in the aid of Mr Annandale. That gentleman succeeded in passing a probe-pointed bistoury about an inch, when he experienced a strong obstruction, which quite occluded the canal at that point. On calling next day, about fifty-eight hours after birth, and finding the child in much the same state, he re-introduced the bougie, and with considerble force broke through the obstruction, but still failed in passing the catheter. On calling in the evening, however, the nurse informed me that the child had passed its water freely in a full stream, and, what could hardly have been expected, without any pain. Since then everything has gone on favourably, no farther steps being taken. In this case there can be no doubt that the principal obstruction was caused by a band drawn across the uretha about an inch from the orifice, though the two edges of the glans seemed to be glued together by some glutinous substance.

On inquiry, the child's father told me that he had suffered from a similar obstruction when young. I failed to get any details on the matter; he states, however, that his mother, who resides in Aberdeen, told him that he was operated on when young; that a tube or probe, as he calls it, was introduced, and that he continued to wear it till he was about 16 years of age. During all that period he was seldom free from pain or annoyance of some kind. Mr Annandale mentioned to me on that occasion, that he had scarcely ever met with a similar case, and that he considered it to be very rare indeed. He mentioned to me a case to which he was called, in 1862, of a child that had passed no water for twenty-four hours. On examination, he found the orifice of the urethra completely occluded for more than an eighth of an inch; there was no meatus. In that case he made an incision over the situation of the urethra, and passed a No. 2 catheter about an inch down. The peculiarity of this case consists in the circumstance that the obstruction was situated near the external orifice and not toward the vesical orifice.

Dr Cochrane thought it strange that the urine should have flowed so freely after such interference. It was common enough for young children not to make water for three or four days after birth.

Dr Burn thought that no other treatment could possibly have been adopted. He had never met with such a case; and although he occasionally had been obliged to introduce a probe, he had never seen the meatus entirely closed.

Dr Bell thought this a rare case, and was of opinion that the treatment adopted was justified by the circumstances. It was no uncommon thing for the urethra to be shut up sometimes by glutinous or membranous matter, and sometimes by phymosis.

Dr Ritchie mentioned that Mr Annandale stated to him that he had never before seen such a case.

An interesting conversation on Infantile Deformities ensued, in which

Dr Burn mentioned that he had seen a case in which the posterior wall of the bladder was visible, and the urine could be seen dropping from the ureter. In one stair he had seen five or six children, all belonging to different families, more or less deformed.

Mr G. Stevenson Smith said, that he had recently attended a lady in her first confinement, whose husband had been born with a supernumerary thumb on each hand. These had been removed in early life, but aware of the tendency of such malformations to show themselves in successive generations of the same family, his wife and her friends were very anxious about the baby. After its birth, it was found to the satisfaction of all that the fingers and toes were all right, but strange to say there was a curious malformation of the ears. Instead of having only one lobule each, both ears had several of these appendages, the supernumerary ones being each attached by a slender pedicle. The grandmother of the child firmly believed that this deformity had been caused by the gentleman having been in the habit of playfully pitching crumbs of bread at his wife's face during her pregnancy!

Dr Menzies mentioned that he had a patient who had three times in succession given birth to a child with a sacral spina-bifida. She had three or four children besides, who were well enough.

SESSION XXVII.—MEETING VII.

26*th February* 1868.—Dr BRYCE, *Vice-President*, in the Chair.

I. CASES OF EXTRA-UTERINE GESTATION, WITH REMARKS.

BY DR J. M. DUNCAN.

This paper will be found at page 919.

Mr Brotherston mentioned that he had had a case of extra-uterine pregnancy in Alloa some years ago. Impregnation had taken place in the Fallopian tube, and went till about the third month, when the tube burst and caused death. The preparation was sent to Sir James Simpson, and Professor Turner had examined it.

Dr Sidey stated that he had met with some four or five cases of extra-uterine gestation in the course of his pratice.

Dr Bryce, in name of the Society, thanked Dr Duncan for his valuable paper.

II. CASE OF POST-MORTEM CÆSAREAN SECTION BY MR BROTHERSTON, F.R.C.S.E., ALLOA.

This paper will be found at page 930.

In reply to several questions, Mr Brotherston stated that death had been caused, he believed, by the separation of the placenta, and consequent escape of blood. The placenta was healthy.

Dr Sidey remembered a case similar to the one related, in which the placenta was separated except at the edges, and the clot was very small. The patient died. He had once performed the operation, but the child was dead. The mother had died of lung disease.

Dr Bryce remarked that a patient might bear the loss of a large quantity of blood if slowly effused, whereas a small quantity rapidly poured out often proved dangerous.

Dr Gordon said, that we might learn from such cases that death might result from hæmorrhage without any external symptoms of bleeding.

A conversation on the best means of resuscitating patients from asphyxia took place, in which Drs Bryce, Bruce, Sidey, Mr Stevenson Smith, and others, took part.

The thanks of the Society were awarded to Mr Brotherston for his paper.

DUNDEE MEDICAL SOCIETY.

At the monthly meeting of this Society, held on the evening of Wednesday, 4th March 1868—Dr Gibson, *President*, in the chair—A reprint of the case of Mackie *v.* the Crown, with accompanying remarks from the *Fifeshire Journal*, received from Dr Bonnar, Cupar, was read to the meeting by Dr Rorie. After considerable discussion, the meeting expressed opinion, that the present system adopted by the Crown in paying medical witnesses was extremely unsatisfactory, and that it was desirable that a fixed scale of fees be established.

Part Fourth.

PERISCOPE.

MEDICINE.

DR F. HUGENBERGER ON THE TREATMENT OF ECZEMA.

The author states that this class of diseases is, as a rule, difficult to treat, that the use of arsenic internally is of no avail, and that remedies applied locally are frequently not well borne by the patient. What Dr H. recommends for external application is an ointment composed of one part of diachylon plaster, and two parts of oil, or cod-liver oil, both of which frequently afford great relief, especially in the early stages. Warm bran baths, or baths containing a little of some alkali in solution may be used, but with no greater advantage than the green soap, with flowers of zinc and white precipitate; neither of which methods of treatment are to be considered as effectual in preventing a relapse. To allay the itching, he used with advantage the local application of vinegar, and dusting with starch powder.—*Präg. Vierteljahrschrift.*

E. LANCEREAUX ON THE CHANGES PRODUCED BY THE ABUSE OF ALCOHOLICS.

Tardieu has described in full as the consequences of acute intoxication certain hæmorrhages into the meninges, the ventricles of the brain, and the lungs, as well as certain inflammations rapidly going on to suppuration in the lungs and liver, and also sometimes in the meninges and brain. The changes which occur in chronic alcoholism may be divided into two groups, according as they affect the fibrous tissues, and belong to the class of adhesive inflammations (*Hunter*), or as they affect the functions of the cells and cause a granular, fatty degeneration in them. The changes which occur in the fibrous tissues spare but few organs, although they chiefly occur in the liver, brain, kidneys, and serous membranes. At the commencement, a distinct injection of the small vessels takes place, and the development of numerous nuclei in the *adventitia;* later, cells and fibres are formed, and, as organization proceeds, the newly-formed tissues contract more and more, and so cause the parenchymatous organs to loose their peculiar characters. Cirrhosis of the liver is pathognomonic of chronic alcoholism. In the brain similar changes occur—it becomes smaller, paler, denser; the convolutions atrophy, especially on the convexity of the cerebrum, sometimes also on the cerebellum, and in the cord. The meninges are at the same time frequently altered, being œdematous, thickened, opaque, and covered with patches or small points, either white or coloured by hæmatine. The kidneys occasionally present a similar condition of atrophy to that of the liver. Magnus Huss observed similar changes in the lungs, which he designated chronic pneumonia. The serous or fibrous membranes (peritoneum, pleura, dura mater) exhibit likewise membranous new formations, which consist of connective tissue and vessels, the latter easily torn. Among the mucous membranes, that of the digestive organs is directly affected by the alcoholic irritation, and that of the stomach and cœcum appear to be especially so. The mucous membrane of the stomach, especially in the neighbourhood of the cardia and the lesser curvature, is very vascular, and frequently covered with hæmorrhagic extravasations. At a later stage this mucous membrane is darkly pigmented, denser, and indurated, just like the submucous tissues. The mucous membrane of the air-passages, and especially of the larynx, is likewise injected, and covered with dot-like hæmorrhagic spots. The changes which occur in the walls of certain vessels are worthy of note, especially in those which come in contact with the absorbed alcohol, the vena porta, and pulmonary artery.

The second series of changes consist in the fatty degeneration of particular parenchymatous parts, and here again the liver is the organ most frequently attacked (fatty liver). The kidneys are affected in a similar way, but never become atrophied or granular, their surfaces always remain smooth. The cells of the gray substances of the cerebellum, and even the capillaries of the brain may also become fatty, as likewise the cells of the pancreas, the salivary and stomach glands, the bronchial ramifications, and even the seminal ducts. The muscles degenerate in a like manner, especially the heart. An important point in the diagnosis of chronic alcoholism is the contemporaneous occurrence of all the above-mentioned changes in connection with accumulation of fat in the subcutaneous tissues, in the mesentery, diaphragm, etc.

Both forms of degeneration differ much in their frequency, as, whilst fatty degeneration is almost constant in certain organs, the new formations of connective tissue occur more seldom. The latter occur with greater frequency in the working, the former among the upper classes. The new connective tissue formations appear to be the consequence of direct irritation of the alcohol, for, on one hand, the parts which come in contact with the alcohol imbibed are most frequently affected, the vena portæ, liver, and pulmonary artery, while, on the other, the injection of alcoholic fluids into the tunica vaginalis, etc., causes a similar result. The fatty degeneration appears to be a consequence of impaired nutrition. Hence follows the diminution of the carbonic acid expired. It is remarkable to note the similarity of the changes of this last group with the wasting of the different organs in old people. This resemblance extends also to the pathological state; for in those accustomed to the free use of stimulants, just as in the aged, most acute diseases are severe, and are accompanied by delirium, hallucinations, and adynamic phenomena, and death in both easily occurs by exhaustion.—*Präg. Vierteljahrschrift.*

SURGERY.

ABNORMAL DISTRIBUTION OF THE GREAT VESSELS IN THE NECK.

The following very rare abnormality, which has an important surgical bearing, is reported by Dr Jules Kosinski of Warsaw:—

The common carotid of the right side was completely absent, two arteries almost exactly equal in size rose directly from the innominate. The inner one gave rise in their natural order to the thyroid, lingual, and facial branches, and then divided into the internal maxillary and temporal arteries. The outer one extended to the carotid canal without giving any branches. Both arteries were contained in a common sheath with the jugular vein, and the pneumogastric nerve as usual. The left side presented no anomaly.

The author states that he has not been able to find an account of a similar case: the one which approaches it most closely being that of Malacarne, in which both common carotids and the innominate were absent, and the external and internal carotids of both sides as well as the subclavians rose directly from the arch of the aorta.—*Gazette Medicale*, No. 5, 1868.

[*Note by Translator.*—Two cases at least of this malformation are already recorded. Quain's Anatomy, 7th edition, vol. i. p. 345.]

PRIMARY EXCISION OF THE ANKLE-JOINT. BY DR OLLIER OF LYONS.

The following is the substance of a very interesting little paper on this subject:—

Excision of the ankle-joint is at present nearly given up in France. Should this be the case? Comminuted fractures of the tibia with wound of the ankle-joint are almost always considered cases demanding amputation, from the risks of a prolonged suppuration and the questionable value of the limb, if saved. My own results have led me to differ from the general opinion. I have twice successfully excised the ankle-joint for injury; and in a third case, in which the patient would not hear of amputation, I excised more than half of the tibia in fragments, and the patient died of cerebral mischief due to an injury of the head. The first was a man æt. 50, in whose case I excised five inches of the tibia, leaving the astragalus and a fractured fibula, but allowing the fracture of the fibula to ride so as to shorten the distance between the tibia and astragalus. The patient recovered, with a useful limb. In the second, the ends of both tibia and fibula, including the malleoli, were removed, and in three months the patient had a firm union in good direction, with little shortening.

Both of these operations were performed in the subperiosteal method, leaving all the periosteum and the capsular ligament; which method, besides greatly aiding the regeneration of bone, hastens and increases fibrous union, and also protects the neighbouring soft parts, vessels, nerves, tendons, and muscles. I also attach importance to the dressing of the limb from the first in an immoveable apparatus, either starch or plaster, as preventing inflammation, and favouring repair.—*Gazette des Hopitaux*, No. 11, 1868.

CASE OF SEVERE NEURALGIA OF THE TONGUE, CURED BY EXCISION OF A PORTION OF THE LINGUAL NERVE. BY DR VANZETTI, OF PADUA.

P. M., æt. 64, mother of twelve children, complained, in January 1866, of intense pain in the left half of the tongue. It was thickened, and there was in it a peculiar pricking feeling, while the slightest movement in eating, drinking, or speaking, caused intolerable agony. The use of morphia and hypodermic injections, during a month, did no good. The patient could never sleep for an instant, and dared not eat on account of the intolerable agony. Opium, iodine of potassium, arsenic, ice, acupressure, local anæsthesia by ether, electricity, etc., were all tried without success. Remembering that on a previous occasion the patient had been cured of a severe pain in her tongue and lower jaw by an incision along the side of the tongue, Dr Vanzetti made one along the whole left side of the tongue. This gave considerable relief for a fortnight, at the end of which time the pain returned with redoubled vigour, and over a larger area, now being felt in sides of jaw, ear, and side of neck. She pressed for more decisive treatment; and it was determined to excise a portion of the lingual nerve. This was done. The tongue was pulled well forward, and an incision was made between the gum and tongue from the last molar forwards. By a careful dissection the nerve was exposed as far back as the edge of the internal pterygoid muscle, and nearly an inch of it was removed. The pain ceased from that moment; and when last the patient was heard of, sixteen months after the operation, the pain had not returned. She was, at that date unable to distinguish the taste of quassia from that of sugar, on the left side of the tongue, and the common sensibility of the part was much diminished. M. Vanzetti refers to the researches of Messrs Hilton and Moore on this subject. —*Gazette des Hôpitaux*, No. 8, 1868.

DR OLLIER OF LYONS ON MALGAIGNE'S APPARATUS.

M. OLLIER made a communication to the Imperial Society of Surgery on this subject.

He had used Malgaigne's apparatus, of the metallic point screwed into the bone from an arch placed above the limb, for the purpose of remedying certain displacements of fragments of the tibia in complicated fractures, with the best results, and thoroughly approved of the method. Besides this, he has since invented a modification on the same principle, consisting of two vertical stems firmly screwed to a splint and united above by a horizontal bar, on which plays a metal point, which can be moved on it and also inclined at any angle. He considers this point as the best means which can be used in every case in which it is necessary to make strong pressure on a displaced fragment. All apparatus acting on the surface, however limited in their action, must be rejected, for if they press feebly they do no good, and if strongly, they are sure in a few hours to cause severe pain and mortification of the soft parts. The point of metal passing *through* the skin acts on the bone only, causes no gangrene, gives the most trifling pain, and does not expose the bone even to the risks of inflammation which might have been expected. Though all his cases have been hospital ones, he has never had either erysipelas or phlegmonous inflammation.

In the treatment of false joint (of which a good case is given) the use of this apparatus not only perfectly fixes the fragments, but also by its presence aids in stimulating the formation of new bone.—*Gazette des Hopitaux*, No. 13, 1868.

DISLOCATION OF THE LEG BACKWARDS ON THE FOOT.

M. HUGUIER read a paper lately to the Academy of Sciences on this very rare lesion. Besides reporting a case in which it occurred, and uncomplicated with fracture, he carefully discussed the etiology, proved that two varieties of the dislocation exist depending on the direction of the displacing force, and described the manœuvres necessary for its reduction. An abstract of the paper is given in *Gazette des Hôpitaux*, No. 9, 1868.

DISEASES OF THE EAR.

EXTRACTS FROM THE REPORT OF THE CASES OF DISEASE OF THE EAR, TREATED BY DR JOSEPH GRUBER IN THE GENERAL HOSPITAL, VIENNA, FOR THE YEAR 1866.

FOR *Accumulation of Cerumen* in the external meatus, iodide of potassium was employed with glycerine to soften the mass, when too hard to get away by syringing alone; and for a considerable time after the wax was removed the same was applied by means of a brush to the walls in order to prevent its too free secretion.

Inflammation of the External Meatus was found generally to be caused by cold, and frequently it followed wrong treatment of toothache—*i.e.*, medicaments applied to the meatus itself. The treatment employed was early and free incisions through the soft parts down to the bone, after which warm water was syringed in to aid the bleeding. This procedure lessened the pain at once, and generally prevented suppuration.

Perforation of the membrana tympani often followed this affection, but the aperture always healed up. Several cases ended in opacity of the membrana tympani, with deafness resulting, and in such Dr Gruber used resorbent ointments, as iodine, in the neighbourhood of the ear, after the acute stage was past. Concretions formed on the membrana tympani in two cases without interfering in any special manner with the hearing.

Chronic Inflammation of the External Ear, accompanied with granulations in the meatus, were treated by tents of laminaria, which dilated the meatus, and permitted of the local application of medicaments.

Acute Inflammation of the Membrana Tympani was always ascribed to cold. Feverish symptoms accompanied this affection. Perforation of the membrana tympani always followed, and once its complete destruction occurred. Local blood-letting was employed during the acute period; and for the otorrhœa which resulted zinc solution was dropped in. In two cases an irregular opacity of the membrana tympani was observed.

In Chronic Inflammation of the Membrana Tympani, it was, as a rule, very much thickened, and so covered with granulations that the malleus was hardly visible, hearing was impaired, singing present in the affected ear, and otorrhœa followed. In weakly patients, the disease had a tedious course, and generally developed itself out of previous painful inflammations of the ear, the peculiar nature of which was not well made out. Treatment was symptomatic; the granulations were occasionally touched with nitrate of silver; for profuse otorrhœa, astringents were dropped in; and for the thickening of the membrana tympani, resorbent ointments were employed.

In Thickening of the Membrana Tympani, painting over it with glycerine, iodide of potassium, and iodine was resorted to, and the hearing was often greatly improved. In cases of loss of substance of the membrana tympani, Gruber often obtains healing by making eight, ten, or twelve small incisions or nicks round the margin of the opening, the contraction of which in cicatrizing fills up the aperture.

Abnormal Adhesions of individual parts of the membrana tympani to the walls of the tympanic cavity were freed by the use of the air-douche applied through the Eustachian tube.

Acute Catarrh of the Middle Ear was observed in connexion with long-standing catarrh of the nasal mucous membrane, hypertrophy of the tonsils following scarlet and typhus fevers, and occurring during the course of phthisis pulmonalis. The catarrh had occasionally caused perforation of the membrana tympani. In cases where nasal catarrh was combined with this affection of the ear, Gruber introduced astringents through the nose by means of a glass syringe, and then caused the patient to expire forcibly with mouth and nose firmly closed, by which means a portion of the fluid found its way into the Eustachian tube. Astringents and other remedies were also applied directly to the tympanic cavity *viâ* the Eustachian tube.

Chronic Catarrh of the Middle Ear was found in connexion with hypertrophy of the tonsils, chronic nasal catarrh, syphilis, or consecutive, as in the case of the acute scarlet fever and typhus; and, lastly, in some patients where no special cause for disease of the ear could be discovered. Perforation of the membrana tympani frequently occurs, its seat being generally in the lower part of the membrane. In cases where the Eustachian tube was found to be narrowed, bougies of laminaria were employed to overcome the stricture. If the perforations already existing in the membrana tympani were not sufficient to allow of free exit for the discharge, he enlarged them by incision; and in all cases where two apertures were united by a "bridge" of membrane, the latter was divided to favour healing.

The singing in the ears which often accompanied this affection frequently continued, defying all treatment; while at other times it disappeared, as it seemed, spontaneously.

Polypi were found to proceed in all cases from the mucous membrane of the tympanic cavity. In every case they were removed by means of Wild's snare. Their return was frequent, from the roots again growing, notwithstanding all endeavours to destroy the latter by means of caustics. The persevering injection of astringents was found to do good more frequently than the use of escharotics.

Stricture of the Eustachian Tube was, as a rule, found to remain behind after chronic catarrh. The membrana tympani in these cases was slightly opaque, and deeply drawn in against the inner wall of the tympanic cavity (cupped). Noises in the ears were present in many cases, but not all prominent in others. Treatment consisted in the introduction of bougies of laminaria, with, at a later period, injection of solution of iodide of potassium, and, when considerable swelling of the mucous membrane was present, astringents.

In a case of *Caries of the Mastoid Process*, which occurred in a patient suffering from severe tubercular disease, of which he ultimately died, the facial nerve was found in the Fallopian canal, soaked in pus, and partially destroyed.

In another case there was exfoliation of a large piece of the mastoid process. The facial paralysis which occurred during the course of the disease disappeared of itself, and the patient recovered.

Nervous pain in the ear has, only in a single case, been unaccompanied with carious teeth. It got better under the use of veratria ointment.—*Präg. Vierteljahrschrift.*

Part Fifth.

MEDICAL NEWS.

A PARISIAN CRITICISM ON THE LONDON MEDICAL SCHOOLS AND EXAMINING BOARDS.

Dr Victor Revillou comments in the following terms, exaggerated, but still including a certain modicum of truth, on the facts regarding medical education in London, revealed in Mr Simon's recent address :—

"Our friends across the Channel do not possess anything corresponding to our Faculties : degrees are conferred in a certain number of schools, which compete with each other for the students.

"Is the instruction better? Would that it were so. The free growth in one town of eighteen or twenty schools of medicine presents to the mind a picture of a most remarkable condition of mental activity. We think we see every teacher stimulated by the competition, making the most superhuman efforts not to appear inferior to his rivals. We love to behold in the pupils the judges of these courteous tournaments, decreeing the palm to the victor by their eager presence. To destroy these beautiful dreams, the fact, the *brutal* fact, is needed.

"We come to read the address of M. Simon, the President of the Society of Teachers, and nowhere have we seen a more eloquent exposure of the lamentable condition of the medical schools of London. The number of pupils enrolled by each school is necessarily limited, and the teachers would be exposed to the risk of having no one at their lectures, did they not employ the most vexatious measures to enforce the attendance of the candidates.

"As M. Simon says, if, by marking down the names of those who leave the class before the end of the hour, you have compelled a certain number of young fellows to remain on the benches on which they sleep, you have made them lose time which might have been profitable for their studies could they have employed it rightly . . . . M. Simon has drawn a strange picture of the examinations as they exist in the principal schools. For example, *at the College of Surgeons of London*, the oldest members are the examiners by right. They arrive *at that honour*, most envied and most lucrative, between sixty and eighty years of age, andyq ˙ ıȷ resign it at all before they die, they do so as late as possible.

"These decrepid old men have to set the bounds to which the knowledge of the rising generation is to reach; and their questions ought to bear on subjects which they have never known, as well as on those which they have forgotten."—*Gazette des Hôpitaux*, No. 14, 1868.

CANDIDATES FOR H. M. INDIAN MEDICAL SERVICE,

Who were successful at the competitive examination at Chelsea in August last, and who have undergone a course of instruction at the Army Medical School, together with the total number of marks obtained at the examinations at Chelsea and Netley:—

Name.	*Studied at*	*Marks.*
1. A. S. Lethbridge,	Aberdeen and England,	5451
2. A. Stephen,	Aberdeen,	5386
3. J. J. Ratton,	Ireland,	5305
4. J. H. Newman,	"	4984
5. H. Johnstone,	Aberdeen,	4767
6. W. R. Gordon,	"	4436
7. J. Davidson,	"	4388
8. A. S. Jayakar,	London,	4291
9. J. F. Keith,	Aberdeen,	3972
10. E A. Trinmell,	England,	3829
11. C. A. Harvey,	Edinburgh and Ireland,	3543
12. W. Hanks,	London,	3339
Maximum,	6900	

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A. T. H. Waters, M.D.,—On Diseases of the Chest. London, 1868.

Henry Wilson,—Theory and Practice of the Ophthalmoscope. Dublin, 1868.

Thomas A. Wise, M.D.,—Review of the History of Medicine. London, 1867.

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Births, Deaths, and Marriages,—Monthly Return for December, 1867, January and February 1868;—Quarterly Return, December 1867.

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Brit. Med. Jour.,—Dec. 28 1867, to March 28, 1868.

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Journal of Psychological Medicine for Jan.

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Part First.

ORIGINAL COMMUNICATIONS.

Article I.—*On the Causes of Death in the Scottish Widows' Fund Life Assurance Society, from* 1st *January* 1860 *to* 31st *December* 1866. By James Begbie, M.D., F.R.S.E., Consulting Physician to the Society.

> "Be absolute for Death; either Death or Life
> Shall thereby be the sweeter."
>
> *Measure for Measure.*

Another septenary has run its course; and we are invited to record and review the causes of death among the assured of the Scottish Widows' Fund Society during that period,—comparing the mortality with that of previous corresponding intervals, and endeavouring to deduce therefrom lessons for our guidance in the time to come.

The Society is now of mature growth. The period which has elapsed since the last Investigation has carried us beyond the age of half a century, and placed the few remaining of our earliest members in the venerable rank of those who have far exceeded the usual duration of life.

Our experience is almost that of a lifetime. The average age of those who seek the benefits of life assurance is, at entry, nearly that of thirty-five years; so that in the widespread family constituting the Scottish Widows' Fund, embracing all ranks, from the highest to the humblest citizenship, we have the means of determining, with considerable accuracy, the value of human life between the period of early manhood and extreme old age; of estimating in some measure the influence of professions, trades, and occupations on health and longevity; and the prevalence and fatality of disease in connexion with family predisposition and hereditary descent.

It still remains a subject of regret that, in carrying out these investigations connected with the records of life assurance, our statistics are so imperfect and so unsatisfactory; that the life examined, and selected or declined to-day, does not again, except in occasional instances, come under our observation; that the value of selection is, after all, so short-lived and uncertain; and that subsequent years afford no history of the risks and dangers the life has undergone, of the diseases it has encountered, or of the changes which death has wrought in the family circle; so that in the large proportion of our membership the record is confined to the history of the day of entrance and that of death, when the accustomed schedule tells us that he died not by his own hand, or the hand of justice, but that he died of one or other of those maladies which make up the list of our nosological tables;—the certificate too often bearing that one of the symptoms of a complicated disease has been assigned as the cause of death. It is not less to be regretted that the medical attendant, in certifying the cause of death, is so often satisfied by merely recording the name of the fatal disease, without referring to its connexion with other morbid conditions from which it sprang,—substituting dropsy, for instance in place of liver, kidney, or heart disease; specifying atrophy, debility, and such like, without a clue to their real origin; or natural decay in place of one or other of the many, it may be obscure, diseases which terminate extreme old age.

The septenary recently completed has made an addition of 1398 to the list of deceased members, and raised the deaths from 2307,—the number declared at the former investigation,—to 3705,—the total mortality from the commencement of the Institution in 1815 to the close of 1866,—a period of fifty-two years. The average expectation at entry of the 1398 deceased was 26 years 11 months, the average actual endurance of the policies was 17 years 5 months, and the mean age at death was 58·0954 years. During the septennium the number of members has increased from 9103, at 1st January 1860, to 13,493, at 31st December 1866.[1] The total number of lives exposed to the risk of mortality during the septennium was 15,808. The calculated equivalent number at risk for the seven years is 11,112.

The following table shows the number of deaths from the different causes, according to the arrangement of the Registrar-General, and the difference in each class between the present and two former investigations:[2]—

Causes of Death.	Period.			Period.		
	1846 to 1852.	1853 to 1859.	1860 to 1866.	1846 to 1852.	1853 to 1859.	1860 to 1866.
I. *Epidemic and Contagious Diseases.*						
1. Small-pox, . . .	0	4	5			
2. Scarlatina, . . .	1	5	3			
3. Diarrhœa, . . .	3	7	15			
4. Dysentery, . . .	16	10	11			
5. Cholera, ...	27	17	10			
Carry forward, . .	47	43	44			

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[1] The average age of these lives at 31st December 1866 was 45 years.

[2] For the tables accompanying this report, I am largely indebted to Mr William Braid of the Society, to whom I beg to acknowledge my obligations.

Causes of Death.	Period.			Period.		
	1846 to 1852.	1853 to 1859.	1860 to 1866.	1846 to 1852.	1853 to 1859.	1860 to 1866.
Brought forward,	47	43	44			
6. Influenza,	8	6	6			
7. Fever (Remittent),	2	0	0			
8. Fever (Continued),	63	48	78			
9. Erysipelas,	10	9	12			
10. Yellow Fever,	0	0	3			
11. Hydrophobia,	0	1	0			
12. Diphtheria,	0	0	1			
				130	107	144
II. *Diseases of Uncertain Seat.*						
1. Inflammation,	0	2	2			
2. Hæmorrhage,	1	0	4			
3. Dropsy,	4	4	6			
4. Abscess,	0	3	4			
5. Mortification,	4	6	4			
6. Scrofula,	0	1	0			
7. Purpura,	0	0	1			
8. Cancer,	5	28	42			
9. Tumour,	7	3	1			
10. Gout,	1	4	13			
11. Atrophy,	4	3	4			
12. Debility,	7	0	3			
13. Sudden Death,	7	5	2			
14. Pyæmia,	0	0	3			
				40	59	89
III. *Diseases of the Brain and Nerves.*						
1. Cephalitis,	6	9	11			
2. Apoplexy,	54	67	95			
3. Paralysis,	28	79	75			
4. Tetanus,	1	1	1			
5. Epilepsy,	4	2	7			
6. Insanity,	0	3	3			
7. Delirium Tremens,	7	7	11			
8. Disease of Brain,	50	66	109			
9. Paraplegia,	0	0	1			
10. Disease of Spinal Marrow,	0	0	1			
				150	234	314
IV. *Diseases of the Respiratory Organs.*						
1. Laryngitis,	1	4	2			
2. Bronchitis,	36	44	91			
3. Pleurisy,	7	7	5			
4. Pneumonia,	15	42	43			
5. Hydrothorax,	7	7	1			
6. Asthma,	0	1	3			
7. Consumption,	42	66	96			
8. Disease of Lungs,	21	25	31			
9. Hæmoptysis,	0	0	2			
10. Quinsy,	0	0	2			
				129	196	276
Carry forward,	...	...	...	449	596	823

Causes of Death.	Period.			Period.		
	1846 to 1852.	1853 to 1859.	1860 to 1866.	1846 to 1852.	1853 to 1859.	1860 to 1866.
Brought forward, .	...	...	...	449	596	823
V. *Diseases of Heart & Bloodvessels.*						
1. Aneurism, . . .	11	11	15			
2. Pericarditis, . . .	0	2	3			
3. Disease of Heart, . .	55	122	203	66	135	221
VI. *Diseases of the Digestive Organs.*						
1. Enteritis, . . .	6	9	11			
2. Peritonitis, . . .	2	8	6			
3. Ulceration of Bowels, .	3	3	2			
4. Hernia, ...	2	0	2			
5. Ileus, ...	4	5	4			
6. Hæmatemesis, . .	1	7	6			
7. Dis. of Stomach and Bowels,	34	31	44			
8. Hepatitis, . . .	5	2	3			
9. Jaundice, . . .	3	1	0			
10. Disease of Liver, . .	34	49	87			
11. Disease of Spleen, . .	0	0	2			
12. Intussusception, . .	0	0	3			
13. Disease of Pancreas, .	0	0	2	94	115	172
VII. *Diseases of the Urinary Organs.*						
1. Ischuria, . . .	1	2	0			
2. Diabetes, . . .	1	4	6			
3. Cystitis, . . .	7	2	0			
4. Stone, ...	1	0	0			
5. Disease of Kidney, .	15	39	51			
6. Disease of Bladder, .	5	5	28			
7. Stricture, . . .	0	0	2	30	52	87
VIII. *Childbed and Disease of Uterus.*						
1. Childbirth, . . .	2	1	1			
2. Ovarian Disease, . .	0	1	0			
3. Disease of Uterus, . .	3	0	0			
4. Phlegmasia Dolens, .	0	0	1	5	2	2
IX. *Diseases of Joints.*						
1. Arthritis, . . .	0	0	1			
2. Rheumatism, . . .	3	7	5			
3. Disease of Joints, . .	1	5	0			
4. Disease of Spine, . .	0	2	0	4	14	6
X. *Diseases of Integumentary System.*						
1. Carbuncle, . . .	2	0	4	2	0	4
XI. *Violent Death.*						
1. Suicide, ...	6	6	13			
2. Drowning, . . .	6	4	8			
3. Accidental Injury, . .	6	19	19	18	29	40
XII. *Natural Decay and Old Age.*	21	29	40	21	29	40
Causes not specified or ascertainable,	1	3	3	1	3	3
Total, . .	...	...	...	690	975	1398

The deaths from all causes during the last septennium have, according to expectation, considerably exceeded those of any former period; but regard being had to the large additions made to the membership of late years, by which the numbers exposed to the risk of mortality have been greatly augmented; and to the mature age of the Society, which has brought into operation the causes of death affecting those in the decline of life, it will be found that the *ratio* of mortality is still within the calculations of the Carlisle and other reliable tables.

I. In regard to the class of Epidemic and Contagious Diseases, it will be seen that the mortality from this source is only a little above that of the septennium 1846-52, though considerably above that of 1853-59. Fluctuations in the returns made of this class of disease must be looked for. The prevalence or absence of a severe epidemic of Fever or Cholera must seriously affect them, while, unhappily, the most careful scrutiny cannot afford the means of protecting the Society against the risk of loss from such causes.

Fever, as usual, bears the highest rate of mortality among the diseases which constitute the class—78 deaths out of 144 being ascribed to this cause. The average expectation of these 78 insured, according to the Carlisle Tables, was 30 years 1 month; their average actual endurance was only 12 years 9 months. Little is to be gained by a search of our records regarding the character of the localities in which the deceased resided. A large proportion were the inhabitants of our large towns,—thus, Edinburgh supplied nine; Glasgow, nine; Dundee, five; Aberdeen, one; Perth, one; while the country districts furnished twelve. In England, Liverpool gave four certificates of death from fever; Manchester, three; London, only one; while Birmingham, where there is a considerable membership, furnishes none. The English and Welsh counties furnish twelve. Ireland, regarded as peculiarly the seat of fever, has presented claims arising from the death of twelve members from this cause, three of which are from Dublin, two from Cork, but not one from Belfast; the remaining seven being from the country districts. In addition to these home returns, we have six from our colonies, and one from the sea. Little also can be derived from an investigation into the influence of professions, trades, or occupations in promoting a predisposition to fever. Among the seventy-eight victims, we find ten merchants, seven clerks, seven medical men, six clergymen, four writers, two inspectors of poor, two schoolmasters, two publicans, the remainder, in double or single number, being distributed over a great variety of trades and occupations; three belonging to the female sex. A reference to the Tables appended will show the period of life at which fever has proved most fatal. A family predisposition to fever is supposed by some to exist. One of the seventy-eight had lost his father, one his mother, and a third his brother, from fever. One only appears to have passed through a previous attack of the kind.

Diarrhœa and Dysentery, next to Fever, have proved the most fatal of the diseases constituting the first class,—the former numbering fifteen, the latter eleven, in the list of deaths. The first (in consequence of six of the number being between 70 and 80 years of age) showing an average actual endurance of 17 years 1 month, in place of 20 years 6 months; the other (in respect of the claimants being, with one exception, under 70) giving an average actual endurance of 12 years 10 months, in place of 24 years 4 months,—their average expectation. Diarrhœa in a chronic form terminated the lives of aged men, while Dysentery in an acute shape cut short the expectation of younger lives in a few days.

A large proportion of the cases of Cholera occurred during its epidemic prevalence in the autumn and early winter of 1866. One of the victims was the subject of mental insanity, a condition found to be the most unfavourable to the issue of the disease. The average expectation of the ten deceased was 31 years; their average actual endurance was 18 years 9 months.

Erysipelas proved fatal to 12 insured, whose average expectation amounted to 27 years 3 months, but the actual endurance of whose policies was only 18 years 9 months. Three of the twelve, however, had exceeded their expectation term.

Influenza was fatal to six aged members, who had survived their average expectation upwards of two years.

Diphtheria, a disease of youth, is not likely to occasion serious loss in the business of Life Assurance. Examples now and then occur of its appearance in more mature age. Our Tables record only one instance from the commencement of the Institution. It occurred in the case of an aged member, who had exceeded his expectation by 8 years 6 months.

II. The increased mortality in the second class arises chiefly from two causes—Cancer and Gout,—the former numbering forty-two deaths, as against twenty-eight; and the latter thirteen as against four, in last septennial Report. This is satisfactorily accounted for by the increased accuracy in the returns of the causes of death, and the increasing age of the Society, which must annually augment the number of diseases which are developed in the decline of life. Of "gout" it has been truly said that "few die, but that more die gouty." The many diseases engendered by the gouty habit may take their place in our nosological tables, and exclude to some extent the real origin of them all; but a careful discrimination of the essence of the disease will lead the well-informed practitioner to tabulate gout as not unfrequently the cause of death; and it must ever be regarded as a fruitful source of secondary disease, and therefore a good ground for imposing an extra rate of premium on all applicants who acknowledge and manifest its presence. Of the thirteen cases of gout who became claims on the benefits of our Society, it was known of five that they were subject to the disease, and of four others, that in their immediate family there was the evidence of disorder which is often connected with and springs from it. In two instances the gout was associated with erysipelas, in one with albuminuria, and in an old gentleman of 81 years of age, it terminated in gangrene. The average expectation of the thirteen was 27 years 9 months, the average actual endurance was 19 years 6 months.

Of the forty-two deaths from Cancer, two only occurred before 40 years of age; eleven between 40 and 50; eleven between 50 and 60; thirteen between 60 and 70; four between 70 and 80; and one above 80 years of age. The average expectation of these was 25 years 5 months; the average actual endurance was 16 years 5 months. In none of the forty-two was cancer acknowledged on insurance as affecting any near relative. In three cases the parents of the proposer had died of affections of the stomach and bowels, which probably were of a cancerous nature.

In regard to the seat of the disease, in ten it was in the rectum; in four, in the stomach; in four, in the bowels; in three, in the gullet; in three, in the abdomen; in one, in the liver; in one, in the brain; in one, in the eyeball; in one, in the mamma; in one, in the penis; in one, in the mouth; and in two, in the tongue. In the remainder it was not localized. There cannot be a doubt that, in recording deaths from affections of the bowels, stomach, liver, and other organs, we are too often kept in ignorance of the true cause, and that cancer is frequently the disease which assumes a less hateful name.

III. Diseases of the Brain and Nerves add largely to our mortality returns. Advancing years bring forward an increasing number of those predisposed to these affections; but happily the loss to the Society on this occasion is comparatively light, in consequence of the large number of the insured having reached their expectation term. A reference to the Tables appended show that of the one hundred and nine who died of chronic disease of the brain, and whose average expectation was 27 years 5 months, the average actual endurance was 16 years 2 months;—that of ninety-five who died of apoplexy, and whose expectation was 26 years 9 months, the endurance was 18 years 10 months; while of seventy-five who died of palsy, and whose average expectation was 25 years, the average actual endurance was 22 years 8 months.

The following Table exhibits the mortality from apoplexy, palsy, and chronic disease of the brain, compared with that from all other causes and the percentage of death at the successive ages:—

Age at Death.	From all Causes.	From Apoplexy, Palsy, and Disease of Brain.	Ratio per cent.
Between 20 and 30, .	41	4	9·76
" 30 " 40, .	139	17	12·23
" 40 " 50, .	241	48	19·92
" 50 " 60, .	350	66	18·86
" 60 " 70, .	338	78	23·08
Above 70 . . .	289	66	22·84
Total, . .	1398	279	19·95

Of the deaths through Apoplexy and Palsy, it was known before accepting the risk that twelve members exhibited a predisposition to these affections, that five were subject to gout, and two to rheumatism. In three the fatal illness was associated with heart affection, and in two preceded by cerebral disease. In regard to the influence of the professions, trades, or occupations followed by the subjects of these cerebral affections, in giving any proclivity to the fatal disease, it may be stated that these extended over a great variety, but that no one appeared to have given a decided predisposition beyond another. The church, the law, and the medical along with the mercantile profession, were fully represented. No peculiar conformation of body is noticed in the applicants as giving evidence of a tendency to cerebral lesion. In like manner, as regards those who became claims in consequence of chronic Disease of the Brain, no well-marked predisposition is noticed, though, in a few instances, the circumstance of a near relative having been affected with insanity and confined in an asylum is referred to.

The deaths from Delirium Tremens in number bear the same relation to the general mortality as they did in former investigations; but there are good grounds for believing that concealment of the real cause of death in this, as in the case of cancer, is largely practised; and that under the name of cephalitis, brain fever, gastric fever, or such like, those interested in the memory of the unhappy victims of intemperance contrive to hide their shame. Of the eleven who died under the true name of delirum tremens, the average expection of life was 32 years 6 months. The average actual endurance was only 11 years 3 months. The shortest liver of all survived his acceptance four years. It is needless to say that no taint or suspicion of intemperance is recorded as attaching to any of the eleven when selected for insurance.

IV. In the mortality from Diseases of the Respiratory Organs, there is, during the septennium, a close approximation to the experience of the former periods. Of the 276 deaths from this class, 91 arose from bronchitis, and 96 from consumption; and it is interesting and instructive to observe that, while the former risks show an average expectation of 23 years 3 months, and maintain an actual endurance of 19 years 10 months, the latter promise an average expectation of 32 years 9 months, and present only an average endurance of 12 years 8 months—nearly four years, however, beyond the average of previous investigation. Of the deaths from bronchitis, 12 out of 91 occurred before fifty years of age; of those from consumption, 64 out of 96 occurred during the same period. The most fatal decennium of bronchitis was from 70 to 80, when no fewer than thirty-two deaths occurred, while not a single death from consumption occurred during these ten years. The most fatal decennium of consumption was that from 30 to 40, when twenty-six deaths occurred; but, during the same period, only two cases of bronchitis became claims. In Life Assurance statistics it will be found that the mortality from consumption goes on in increasing ratio up to 45 years of age, and then gradually declines up to 65 or 70. Bronchitis, on the other hand, commences between 30 and 40, and steadily increases its numbers up to 70 years of age. The one is peculiarly the disease of youth and early manhood, the other competes with palsy, apoplexy, and disease of the heart for the possession of advancing years and old age.

The following Table exhibits the mortality from Consumption, compared with that from all other causes, during the last septennium, and the percentage of death at the successive ages:—

Age at Death.	From all Causes.	From Consumption.	Ratio per cent.
Between 20 and 30, .	41	13	31·70
" 30 " 40, .	139	26	18·70
" 40 " 50, .	241	25	10·37
" 50 " 60, .	350	21	6·00
" 60 " 70, .	338	11	3·25
Above 70 . . .	289	...	0·00
Total, . . .	1398	96	6·87

Of these ninety-six deceased, it was known of ten that, before acceptance, they had each lost a father or mother by consumption; of eight, that they had each lost a brother or sister; and of six, that they had each lost an uncle or aunt by the same disease. In several cases cousins had been affected; and in one instance a wife had died of the disease. In fifteen other instances a suspicion attached, either personally or relatively, to the insured. An attack of Hæmoptysis at some distant period, an acknowledged susceptibility to cold, or the marks of a strumous habit, were overcome by the otherwise healthy appearance and soundness on examination; while the circumstance of a mother dying after childbirth, or a brother or sister of tubercular disease of the bowels or brain, or of hip disease, or white swelling of the knee-joint, was not permitted to outweigh the good points in the individual himself or his family history.

An instructive lesson is to be learned from these facts. In nine instances only was an increased rate of premium paid as a small protection against so much loss to the Society.

Of eleven who effected insurance after 45 years of age and died of consumption, one survived his acceptance 20 years; two 18 years each; one 14; one 13; one 11; two 8; one 6; one 5; and one 1½ years.

Our Tables again show, that neither in the case of an early insurance, nor of one effected in later years, did any of the consumptive risks attain their expectation period.

Besides these ninety-six deaths from consumption, we have to record forty-three from pneumonia, chiefly of acute character, and thirty-one from other disease of the lungs of chronic nature. The former having an average expectation of 27 years, had an average actual endurance of 15 years 1 month; the latter having an average expectation of 28 years 10 months, acquired an average endurance of 16 years 6 months. It may be questioned whether these returns of acute and chronic disease of the lungs have been accurately tabulated;—whether a portion, perhaps a considerable portion, did not properly belong to tubercular disease of the lungs, and fall under the table of consumption. It must be admitted that in investigating the family history of the deceased, there are, in some, the records of a predisposition to consumption; thus, a clergyman insured at the age of 45, having lost a brother and sister from consumption, died of pneumonia of six days' duration; he reached the age of 73, exceeding his expectation by five years. A lady insured at 63, having also lost a brother and sister from consumption; she died at 75, after a month's illness, having nearly attained her expectation term. A gentleman insured at 55, having lost a sister from consumption; he died of pneumonia of three days' duration, at the age of 63, falling short of his expectation by nine years. A more doubtful case was that of a gentleman who insured at the age of 31, having lost no near relative, but having more than one affected at the time by consumptive ailments, dying himself, at the age of 35, of pneumonia after an illness of four months. So much as regards hereditary predisposition. Let us look into the personal history of those who died from pneumonia. One had suffered a previous attack of inflammation of the lungs, was accepted at the age of 41, and died of a subsequent attack at the age of 57, after an illness of seven days' duration. Another insured at 40, having suffered from pneumonia; he lived to 64, falling short of his expectation by little more than two years, and died of a subsequent attack after an illness of eight days. Two of the deceased had suffered from rheumatism, and the parents of two others had died of the disease. Nineteen of the deaths from pneumonia occurred during the currency of the first week of illness, and ten during the second week; the more lingering and complicated running their course without any suspicion of consumption, and generally far within the period usually assigned to that disease.

Among the deaths entered under the name of Disease of the Lungs, there were seven insured in whom a predisposition to consumption might have been inferred from the family history; two where asthma had been acknowledged; two who had suffered from rheumatic fever; and one from pleuro-pneumonia. Eight only of these deaths occurred before 50 years of age, the most fatal period of consumption, and twenty-three after that age,—fifteen taking place between 50 and 60; five between 60 and 70; two between 70 and 80; and one above 80, exceeding his expectation term by thirteen years. Still, it must be a ground of suspicion, as well as a matter of regret, that a correct nomenclature in regard to several of these deaths has not been observed, and that disease of the lungs has too often been returned in place of consumption as the true cause.

Pleurisy has only accounted for five deaths against seven in each of the two former septennial returns. They all occurred in persons above 40 years of age. The average endurance fell short by ten years of the expectation. There is reason to believe that pleurisy complicated many of the fatal cases under disease of the lungs, and that, while rheumatism and phthisis may be considered the ordinary sources of pleurisy, cancer, and other malignant disease within the chest, affecting the bony parietes, the mediastinum, or other soft parts, is not unfrequently the true, though obscure, cause of chronic pleurisy.

V. In the class of Diseases of the Heart and Bloodvessels we have again a considerable increase in the number of deaths, namely, from 135, in our former septennium, to 221 in our present investigation. Chronic disease of the heart accounts for 203 of the 221 deaths in this class,—fifteen cases of aneurism, and three of pericarditis, bearing a proportional increase to the numbers at risk compared with former septennial returns. A reference to the Tables appended will show the progressive increase of death from disease of the heart during the six decennial periods, namely from 30 to 80 years of age, and illustrate the truth of observations advanced in previous reports, that as the number of aged risks increases, so will the deaths be found to arise in increasing ratio from disease of the heart. Of the 203 deaths from this cause during our present investigation, eight only occurred before 40 years of age; twenty-nine between 40 and 50; fifty-four between 50 and 60; sixty-six between 60 and 70; forty-two between 70 and 80; and four above 80 years of age,—showing that, as the numbers living diminish, the ratio of deaths from heart disease gradually increases. We ventured, in our last Report, in regard to this source of mortality, to express the opinion that future returns would be found more favourable as regards the survivancy of these lives. On that occasion the average expectation of 122 who had died of heart disease was 25 years 8 months; their average actual endurance was 17 years 11 months. On this occasion the average expectation of 203 who have since become claims was 26 years, while their average actual endurance is 19 years 7 months.

The following Table exhibits the mortality from chronic disease of the heart, compared with that from all other causes, during last septennium, and the percentage of death at different ages:—

Age at Death.	From all Causes.	From Heart Disease.	Ratio per cent.
Between 20 and 30, .	41	0	0·00
" 30 " 40, .	139	8	5·75
" 40 " 50, .	241	29	12·03
" 50 " 60, .	350	54	15·43
" 60 " 70, .	338	66	19·52
Above 70 . . .	289	46	15·91
Total, . . .	1398	203	14·52

Our records in regard to the manner of life and death of the subjects of heart disease must ever be imperfect; still sufficient is known to make the history both interesting and instructive. It was known, for instance, that in affecting insurance, twenty-one had been the subjects of rheumatic fever or rheumatic gout, that three more were hereditarily predisposed by near relationship with sufferers from the disease; that seven had suffered from gout, and six others were predisposed to it, while ten were the descendants of parents who had died of heart disease. An effort had been made in nine instances to cover the increased risk by extra premium. The form of lesion considered to have been the cause of death has in too many instances not been specified. In eleven returns, valvular disease; in nine, dilatation; in four, hypertrophy; in five, angina pectoris; in three, rupture; and in one, embolism; while, out of proportion, fatty degeneration, in fifteen cases, has been assigned as the cause of death. That event was in twenty-three individuals sudden, and we are left to conjecture how and what part of the organism was affected. In nine instances only a post-mortem examination revealed its seat. Dropsy in thirty-seven cases accompanied the fatal lesion; while kidney, liver, and lung disease, especially bronchitis, in numerous instances added to the complications which ultimately ended in death.

VI. The deaths under Diseases of the Digestive Organs have considerably increased during the last septennium, namely, from 115 in our former, to 172 in our recent investigation. This arises chiefly from the large addition made by liver disease, which has nearly doubled its numbers; while chronic disease of the stomach and bowels maintains a ratio of mortality corresponding nearly with that of the last septennial period. It is difficult to account for this discrepancy. We are unwilling to ascribe it to an increase in intemperate habits, seeing that affections of the stomach and kidney arising from such indulgence bear a proportional increase only according to the number of lives at risk. A scrutiny into the facts brought out at acceptance, and disclosed at death, throws no light on the subject. No increase in the amount of foreign residence, and no undue addition to the trades and occupations favourable to indulgence in intemperance can be made out. A more marked attention to the prevalence of disease of the liver, as bearing a chief part in many complicated affections of the digestive system, and a more correct return by the medical attendant to our nosological tables of the cause of death, may, in some measure, explain the fact. Thus Jaundice, which has hitherto appeared in our returns as a cause of death, has no standing in our present tables, but has, in all probability, yielded its numbers to the column of disease of the liver.

The average survivancy of the eighty-seven who died from disease of the liver was 15 years 11 months, in place of 26 years 1 month,—their average expectation of life. The average survivancy of the forty-four who died from disease of the stomach and bowels was 16 years 8 months, in place of 27 years,—their average expectation term,—a return more favourable than the same classes formerly presented.

It is worthy of remark that Hernia, a disease on account of which an extra rate of premium is generally charged, accounts for the loss of only two of our members on this occasion, and that this occurred in the case of two octogenarians whose average expectation of life was 22 years, but whose endurance fell little short of 34 years. Five deaths only from hernia are recorded during the existence of the Society, an experience which has led the directors to reduce the rate of extra premium to a minimum, the mortality bearing a very small proportion to the numbers affected with the disease.

VII. The deaths from Diseases of the Urinary Organs have increased considerably during the last septennium. This is again to be traced to the advanced age of the Society, which is now yearly sustaining more and more the loss of its oldest members. Among these aged risks there is no disease more common than that of the Bladder. Of the twenty-eight deaths from this cause, there were only two before 60 years of age; thirteen between 60 and 70; nine between 70 and 80; and four above 80 years of age. The average expectation of the twenty-eight was 24 years 1 month, the average endurance was 26 years 9 months. The Society has thus experienced a gain on the risks emerged from this cause. Different, however, is the result as regards the Diseases of the Kidney. Of the fifty-one deaths under this head, there were claims at all ages from 30 to 80, the larger proportion, however, being before 60. The average expectation of these was 27 years 10 months; the average actual endurance was only 18 years.. A careful examination of the records of the Society previous to acceptance, fails to elicit any information calculated to raise a doubt as to the habits of these individuals; they bear, however, as on former investigations, that many were dyspeptic, and that several were rheumatic or gouty. As stated in former reports, Dropsy was a frequent termination of these renal diseases.

VIII. The deaths in Childbed are only two in number. In one case puerperal fever, in the other phlegmasia dolens, was the fatal cause,—in the former occurring at the age of 39, one year nine months after acceptance; in the other at 36, ten years nine months after acceptance.

IX. In the class of Diseases of Organs of Locomotion are recorded six deaths,—one from Arthritis, and five from Rheumatism. The former appears to have been gout, complicated with kidney disease and terminating suddenly. In the latter, rheumatic fever or acute rheumatism, to which two of the number were predisposed, shortened the duration of life, and gave an average survivancy of 2 years 2 months to the five, whose expectation was 28 years 8 months.

X. Diseases of the Integumentary System. During the former septennium no death occurred under this head; in the present scrutiny we record four from Carbuncle. It is greatly to be desired that in returning the cause of death under this name, some notice should be taken of the constitutional disturbance under which the disease presented itself, as in many instances it is only the local manifestation of a general affection,—Diabetes and Bright's disease, for instance.

XI. Under the head of Violent Death we record the loss of no fewer than forty of our members, against twenty-nine in our immediately preceding, and eighteen in the still prior investigation, the mortality maintaining a ratio corresponding with the number of lives at risk, and bearing a rate considerably under that experienced by other Institutions. Through suicide, or drowning, or accidental injury, these forty, whose average expectation at entry amounted to 27 years 9 months, show an average actual endurance of only 12 years 4 months. We need not record the numbers or the forms of suicide, "Death's thousand doors stand open." We need not enumerate the modes of drowning, or specify the accidental injuries, by which so many of our members became early claims on the benefits of the Society. Let us rather rejoice that, for the mitigation of these sad calamities, there is to be found the wide portal of Life Assurance, and the provision which it offers to the widow and the fatherless.

XII. Lastly, by a remarkable coincidence, we record forty deaths also under the head of Old Age and Natural Decay—the exact number of the violent deaths, and bearing precisely the same relation to the numbers of two classes which the previous septennium disclosed. Of these aged members four died between 65 and 70; two between 70 and 75; thirteen between 75 and 80; thirteen between 80 and 85; and eight between 85 and 90. The average expectation of these forty insured was, at entrance, 20 years 2 months; their average survivancy was 28 years 3 months,—a great improvement as compared with former investigations.

Of the 1398 deaths of which we have now attempted a passing notice, 289 occurred beyond the age of 70; and of these 69 were on the further side of 80. It is satisfactory to find that a special disease has been returned as the cause of death of the great majority of these old members, and that forty only have failed to find their appropriate column, and are left to rank with old age and natural decay.

I.—MORTALITY TABLE *for the Seven Years* 1860 *to* 1866, *both inclusive.*

CAUSES OF DEATH.	AGE AT DEATH.							Total.	Average expectation at entry according to the Carlisle Table of Mortality.		Average actual endurance of Policies.		Mean Age of Members at Death caused by each Disease, calculated from day of Birth to day of Death.
	All up to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	Above 80.						
1. *Epidemic, Endemic, and Contagious Diseases.*									Ye.	Mo.	Ye.	Mo.	
Smallpox, . . .	2	2	1	...	...	...	...	5	35	1	8	9	37·4818
Scarlatina, . . .	...	1	2	...	...	...	...	3	32	1	10	8	43·3512
Diarrhœa, . . .	...	1	1	4	2	6	1	15	20	6	17	1	67·8484
Dysentery, . . .	...	1	2	2	5	1	...	11	24	4	12	10	57·5200
Cholera, . . .	...	3	1	3	2	1	...	10	31	0	18	9	53·3591
Influenza, . . .	...	...	...	...	1	4	1	6	21	6	23	7	72·4915
Fever, . . .	6	13	24	19	11	3	2	78	30	1	12	9	48·9236
Erysipelas, . . .	...	1	3	2	3	3	...	12	27	3	18	9	59·0152
Yellow Fever, . .	2	1	...	...	...	...	...	3	40	5	6	3	26·9648
Diphtheria, . .	...	...	...	...	...	1	...	1	29	8	38	2	74·3095
2. *Uncertain Seat.*													
Inflammation, . .	...	...	...	...	1	...	1	2	20	4	15	5	72·2369
Hæmorrhage, . .	...	2	...	1	...	1	...	4	28	7	11	4	49·3746
Dropsy, . . .	...	...	...	3	2	1	...	6	22	11	18	5	64·2834
Abscess, . . .	...	1	2	1	...	...	...	4	32	9	14	2	48·2570
Mortification, . .	...	1	1	...	2	...	...	4	28	6	14	10	53·2965
Purpura, . . .	...	...	1	...	...	...	...	1	37	2	19	8	45·3151
Cancer, . . .	...	2	11	11	13	4	1	42	25	5	16	5	58·7482
Tumour, . . .	...	...	...	...	1	...	...	1	27	0	21	0	61·7671
Gout, ...	...	2	1	5	2	2	1	13	27	9	19	6	58·5327
Atrophy, . . .	...	...	...	1	2	...	1	4	25	8	27	3	69·6281
Debility, . . .	...	...	...	2	1	...	...	3	28	1	21	0	59·9335
Sudden Deaths, . .	...	...	...	...	1	1	...	2	25	6	27	7	70·6644
Pyæmia, . . .	...	1	1	...	1	...	...	3	30	7	13	6	48·6082
3. *Nervous System.*													
Cephalitis, . . .	1	5	1	2	1	...	1	11	30	6	11	10	46·0315
Apoplexy, . . .	1	6	14	26	30	14	4	95	26	9	18	10	59·9757
Paralysis, . . .	...	2	6	13	22	27	5	75	25	0	22	8	66·0334
Tetanus, . . .	...	...	1	...	...	...	...	1	31	4	7	6	41·3917
Epilepsy, . . .	...	3	2	1	...	1	...	7	30	6	14	10	49·0917
Insanity, . . .	...	...	2	...	...	1	...	3	23	0	13	2	59·7958
Delirium Tremens, .	1	2	4	3	1	...	...	11	32	6	11	3	43·7258
Disease of the Brain, .	3	9	28	27	26	15	1	109	27	5	16	2	56·2946
Paraplegia, . . .	...	...	...	...	...	1	...	1	25	8	35	11	78·3945
Disease of Spinal Marrow,	...	...	...	1	...	...	...	1	34	4	26	5	56·2000
Carry forward,	16	59	109	127	130	87	19	547	Carry forward.				

MORTALITY TABLE *for the Seven Years* 1860 *to* 1866, *both inclusive—continued.*

CAUSES OF DEATH.	AGE AT DEATH.							Total.	Average expectation at entry according to the Carlisle Table of Mortality.		Average actual endurance of Policies.		Mean Age of Members at Death caused by each Disease, calculated from day of Birth to day of Death.
	All up to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	Above 80.		Ye.	Mo.	Ye.	Mo.	
Brought forward, .	16	59	109	127	130	87	19	547	Brought forward.				
4. *Respiratory Organs.*													
Laryngitis, . . .	...	...	...	...	1	1	...	2	26	2	25	2	66·8638
Quinsy, . . .	...	...	1	...	1	...	...	2	25	0	20	3	63·5972
Bronchitis, . . .	...	2	10	14	24	32	9	91	23	3	19	10	66·0812
Pleurisy, . . .	...	...	1	2	1	1	...	5	24	3	14	11	59·9468
Pneumonia, . .	...	6	8	15	7	5	2	43	27	0	15	1	55·8495
Hydrothorax, . .	...	...	...	...	...	1	...	1	18	6	19	3	72·6740
Asthma, . . .	...	1	1	1	...	...	...	3	31	5	12	1	45·6557
Consumption, . .	13	26	25	21	11	...	...	96	32	9	12	8	44·9170
Disease of Lungs, .	2	4	2	15	5	2	1	31	28	10	16	6	54·5372
Hæmoptysis, . .	...	...	...	2	...	...	...	2	34	5	22	1	51·6602
5. *Organs of Circulation.*													
Pericarditis, . .	...	1	1	...	1	...	...	3	30	10	12	8	47·5999
Aneurism, . . .	...	2	2	4	6	...	1	15	28	2	18	3	57 8317
Disease of Heart, .	...	8	29	54	66	42	4	203	26	0	19	7	61·1107
6. *Digestive Organs.*													
Gastritis-Enteritis, .	...	1	2	4	3	1	...	11	28	9	16	3	54·6988
Peritonitis, . . .	...	1	2	...	3	...	...	6	26	4	8	6	50·3958
Hernia, . . .	...	...	...	...	...	...	2	2	22	0	33	10	81·4849
Colic or Ileus, . .	...	1	...	...	1	2	...	4	21	3	15	1	64·6542
Intussusception, . .	...	...	...	2	1	...	...	3	31	0	22	5	57·0152
Hæmatemesis, . .	...	...	...	5	1	...	...	6	24	7	11	2	55·9170
Disease of Stomach and Bowels, . . .	1	4	6	14	12	5	2	44	27	0	16	8	57·2384
Disease of Pancreas, .	...	...	1	...	1	...	...	2	33	0	19	7	50·8424
Hepatitis, . . .	2	...	...	...	1	...	...	3	33	8	6	1	37·6867
Disease of Liver, .	...	8	23	24	20	11	1	87	26	1	15	11	58·1147
Disease of Spleen, .	...	...	...	1	...	1	...	2	25	1	20	6	64·4301
Perforation of Bowels,	...	...	1	...	...	1	...	2	22	1	13	6	61·9492
7. *Urinary Organs.*													
Diabetes, . . .	...	...	1	3	1	1	...	6	27	10	15	8	55·2286
Disease of Kidneys, .	1	2	5	25	15	2	1	51	27	10	18	0	56·9563
Disease of Bladder, .	...	...	...	2	13	9	4	28	24	1	26	9	71 7519
Stricture, . . .	...	...	...	1	...	1	...	2	25	5	21	1	64·1918
8. *Organs of Generation.*													
Childbirth, . . .	...	1	...	...	...	...	...	1	29	0	1	9	39·3863
Phlegmasia Dolens, .	...	1	...	...	...	...	...	1	37	2	10	9	36·7041
9. *Organs of Locomotion.*													
Arthritis, . . .	...	...	...	1	...	...	...	1	31	0	21	2	55·8054
Rheumatism, . .	...	3	1	1	...	...	...	5	28	8	2	2	40·4082
10. *Integumentary System.*													
Carbuncle, . . .	...	...	1	2	1	...	...	4	26	2	11	6	54·2856
11. *Violent Deaths*, .	4	8	9	9	8	...	2	40	27	9	12	4	52·0141
12. *Old Age and Natural Decay*, . . .	...	...	...	...	4	15	21	40	20	2	28	3	79·5698
13. *Cause not ascertainable*,	...	...	...	1	...	...	...	1	38	7	30	1	57·8904
14. *Cause not ascertained*,	2	...	...	...	...	...	...	2	38	3	1	3	25·5562
Totals, .	41	139	241	350	338	220	69	1398	26	11	17	5	58·0954

II.—ABSTRACT OF MORTALITY TABLE,

For the Seven Years 1860 *to* 1866, *both inclusive.*

CLASSES.	AGE AT DEATH.							Total.	Percentage of the Deaths in each Class.	Average expectation at entry, according to the Carlisle Table of Mortality.		Average actual endurance of Policies.		Mean Age of Members at Death caused by each class of Disease calculated from day of Birth to day of Death.
	All up to 30.	30 to 40.	40 to 50	50 to 60.	60 to 70.	70 to 80.	Above 80.			Ye.	Mo.	Ye.	Mo.	
1. Epidemic, Endemic, and Contagious Diseases,	10	23	34	30	24	19	4	144	10·30	28	8	14	2	52·7779
2. Uncertain Seat,	...	9	17	24	26	9	4	89	6·37	26	0	17	0	59·0846
3. Nervous System,	6	27	58	73	80	59	11	314	22·46	27	0	18	2	58·8428
4. Respiratory Organs, . .	15	39	48	70	50	42	12	276	19·75	27	11	16	4	55·5413
5. Organs of Circulation, . .	...	11	32	58	73	42	5	221	15·81	26	2	19	5	60·6827
6. Digestive Organs,	3	15	35	50	43	21	5	172	12·31	26	5	15	9	57·3925
7. Urinary Organs,	1	2	6	31	29	13	5	87	6·22	26	9	20	6	61·3248
8. Organs of Generation, . .	...	2	...	...	...	...	...	2	0·14	33	1	6	3	38·0452
9. Organs of Locomotion, . .	...	3	1	2	...	...	...	6	0·43	29	3	6	11	44·2575
10. Integumentary System, . .	...	...	1	2	1	...	...	4	0·28	26	2	11	6	54·2856
11. Violent Deaths, .	4	8	9	9	8	...	2	40	2·86	27	9	12	4	52·0141
12. Old age and Natural Decay, .	...	...	...	...	4	15	21	40	2·86	20	2	28	3	79·5698
13. Cause not ascertainable . .	...	...	...	1	...	...	...	1	0·07	38	7	30	1	57·8904
14. Cause not ascertained, . .	2	...	...	...	...	...	...	2	0·14	38	3	1	3	25·5562
Total, . .	41	139	241	350	338	220	69	1398	100·00	26	11	17	5	58·0954

III.—TABLE *Showing the Diseases of which Persons Assured by the Scottish Widows' Fund Society have died, from* 1815 *to* 1866 *inclusive; and the Age, by Decennial Periods, at which Death occurred.*

CAUSES OF DEATH.	AGE AT DEATH.							TOTAL.
	All up to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	Above 80.	
1. *Epidemic, Endemic, and Contagious Diseases.*								
Smallpox, . . .	3	3	2	1	...	...	...	9
Scarlatina, . . .	1	5	6	1	...	...	...	13
Diarrhœa, . . .	1	2	2	8	6	9	1	29
Dysentery, . . .	3	2	6	7	17	6	1	42
Cholera, . . .	4	7	14	22	10	2	...	59
Influenza, . . .	...	1	4	3	10	11	2	31
Fever, ...	15	50	66	68	39	6	2	246
Erysipelas, . . .	2	4	8	8	7	8	2	39
Yellow Fever, . .	2	1	...	...	...	...	...	3
Diphtheria, . . .	...	...	...	...	...	1	...	1
Hydrophobia, . .	...	...	...	1	...	...	...	1
Ague, ...	...	...	1	...	...	...	...	1
2. *Uncertain Seat.*								
Inflammation, . .	...	...	1	...	2	...	1	4
Hæmorrhage, . .	1	2	2	2	1	2	...	10
Dropsy, . . .	...	2	4	9	5	3	...	23
Abscess, . . .	...	2	4	2	...	1	...	9
Mortification, . . .	1	1	1	5	11	2	...	21
Purpura, . . .	...	...	1	...	...	...	...	1
Cancer, ...	1	3	17	20	32	6	2	81
Tumour, . . .	...	2	1	6	4	3	...	16
Gout, ...	...	2	1	8	5	3	1	20
Atrophy, . . .	2	3	1	2	3	1	2	14
Debility, . . .	...	1	2	4	10	3	...	20
Sudden Deaths, . .	...	4	3	3	3	4	...	17
Pyæmia, . . .	...	1	1	...	1	...	...	3
Scrofula, . . .	...	...	1	...	...	...	...	1
3. *Nervous System.*								
Cephalitis, . . .	3	11	17	13	7	...	1	52
Apoplexy, . . .	4	16	45	72	84	37	9	267
Paralysis, . . .	1	7	17	41	59	67	11	203
Tetanus, . . .	...	...	2	2	...	...	...	4
Epilepsy, . . .	...	3	5	4	...	2	...	14
Insanity, . . .	...	3	4	3	...	1	...	11
Delirium Tremens, .	2	8	14	5	2	...	...	31
Disease of the Brain, .	7	29	63	56	58	29	3	245
Paraplegia, . . .	...	...	...	...	...	1	...	1
Disease of Spinal Marrow,	...	...	...	1	...	...	...	1
Convulsions, . . .	...	...	1	1	...	...	...	2
Carry forward, .	53	175	317	378	376	208	38	1545

TABLE III.—*Continued.*

CAUSES OF DEATH.	AGE AT DEATH.							TOTAL.
	All up to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	Above 80.	
Brought forward, .	53	175	317	378	376	208	38	1545
4. *Respiratory Organs.*								
Laryngitis, ...	...	4	4	1	4	1	...	14
Quinsy, ...	...	...	1	...	1	...	...	2
Bronchitis, ...	2	10	19	34	55	52	17	189
Pleurisy, ...	1	3	5	4	6	1	...	20
Pneumonia, ...	1	11	24	34	22	15	3	110
Hydrothorax, ...	...	1	9	9	8	5	2	34
Asthma, ...	...	1	1	3	2	1	...	8
Consumption, ...	37	89	73	54	22	1	...	276
Disease of Lungs, . .	3	14	19	30	18	10	4	98
Hæmoptysis, ...	...	...	...	2	...	...	...	2
5. *Organs of Circulation.*								
Pericarditis, ...	...	3	3	1	2	...	...	9
Aneurism, ...	...	3	9	12	14	...	2	40
Disease of Heart, . .	1	25	59	117	138	75	11	426
6. *Digestive Organs.*								
Gastritis-Enteritis . .	2	6	7	10	6	2	...	33
Peritonitis, ...	5	2	6	3	3	...	...	19
Hernia, ...	...	1	1	...	1	...	2	5
Colic or Ileus, . . .	...	2	3	4	4	5	...	18
Intussusception, . . .	...	...	...	2	1	...	...	3
Hæmatemesis, . . .	...	1	2	7	5	1	...	16
Disease of Stomach and Bowels, ...	5	15	17	44	38	18	2	139
Disease of Pancreas, .	...	...	1	...	1	...	...	2
Hepatitis, ...	2	1	3	5	3	1	...	15
Disease of Liver, . .	1	15	48	58	34	25	2	183
Disease of Spleen, . .	...	...	...	1	...	1	...	2
Perforation of Bowels, .	...	...	1	...	...	1	...	2
Jaundice, ...	...	1	1	1	4	...	...	7
Ulceration of Bowels, .	...	3	4	1	4	2	...	14
7. *Urinary Organs.*								
Diabetes, ...	...	2	3	5	3	1	...	14
Disease of Kidneys, .	1	12	12	49	31	5	3	113
Disease of Bladder, . .	...	...	...	3	18	20	5	46
Stricture, ...	...	...	...	1	...	1	...	2
Ischuria, ...	...	...	...	2	...	2	...	4
Cystitis, ...	...	...	...	1	3	6	1	11
Stone, ...	...	...	...	...	1	1	...	2
8. *Organs of Generation.*								
Disease of Uterus, . .	...	...	4	...	1	...	...	5
Childbirth, ...	1	4	2	...	...	...	...	7
Phlegmasia Dolens, .	...	1	...	...	...	...	...	1
Ovarian Dropsy, . .	...	...	...	...	1	...	...	1
Carry forward, . .	115	405	658	876	830	461	92	3437

TABLE III.—*Continued.*

CAUSES OF DEATH.	AGE AT DEATH.							TOTAL.
	All up to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	Above 80.	
Brought forward, .	115	405	658	876	830	461	92	3437
9. *Organs of Locomotion.*								
Arthritis, ...	...	...	...	1	...	...	...	1
Rheumatism, ...	...	4	6	4	3	...	...	17
Disease of Joints, . .	...	2	2	...	1	2	...	7
Disease of Spine, . .	1	...	...	...	...	1	...	2
10. *Integumentary System.*								
Carbuncle, ...	...	...	1	3	1	1	...	6
11. *Violent Deaths*, . . .	8	26	27	30	11	1	2	105
12. *Old Age and Natural Decay*,	...	...	...	...	6	43	47	96
13. *Causes not specified or ascertainable*, ...	...	...	...	...	...	...	...	34
TOTALS, . .	124	437	694	914	852	509	141	3705

IV.—TABLE *showing the Mortality from Consumption, Disease of the Heart, and Apoplexy, Palsy, and Disease of Brain, compared with that from all other causes, and the percentage of Death at the successive Ages, from* 1815 *to* 1866.

I. CONSUMPTION.

Age at Death.	From all Causes.	From Consumption.	Ratio per cent.
Between 20 and 30, .	124	37	29·83
„ 30 „ 40, .	437	89	20·36
„ 40 „ 50, .	694	73	10·51
„ 50 „ 60, .	914	54	5·91
„ 60 „ 70, .	852	23	2·70
Above 70 . . .	650	...	...
Causes not ascertained,	34	...	...
Total, . . .	3705	276	7·45

II. HEART DISEASE.

Age at Death.	From all Causes.	From Heart Disease.	Ratio per cent.
Between 20 and 30, .	124	1	0·80
„ 30 „ 40, .	437	25	5·72
„ 40 „ 50, .	694	59	8·50
„ 50 „ 60, .	914	117	12·80
„ 60 „ 70, .	852	138	16·19
Above 70 . . .	650	86	13·23
Causes not ascertained,	34	...	...
Total, . . .	3705	426	11·49

III. Apoplexy, Palsy, and Brain Disease.

Age at Death.	From all Causes.	From Apoplexy, Palsy, and Brain Disease.	Ratio per cent.
Between 20 and 30, .	124	12	9·67
„ 30 „ 40, .	437	52	11·90
„ 40 „ 50, .	694	125	18·01
„ 50 „ 60, .	914	169	18·49
„ 60 „ 70, .	852	201	23·59
Above 70 „ . .	650	156	24·00
Causes not ascertained,	34	...	...
Total, . . .	3705	715	19.29

Article II.—*On Intestinal Concretions.* By Patrick Heron Watson, M.D., F.R.S., F.R.C.S., Lecturer on Surgery, Surgeons' Hall, Surgeon Royal Infirmary and Chalmers' Hospital, Edinburgh.

(*Read before the Medico-Chirurgical Society, February* 5, 1868.)

The formation of concretions within some portion of the alimentary canal is of no infrequent occurrence in the lower animals.

The history of comparative pathology is replete with instances of the bezoar stones, formed in the camel, goat, and antelope. And the confident belief which at one time attributed to the padi-zeher or bezoar stone the virtues of an alexipharmiac, or poison antidote, has imported into most of the ancient and mediæval works upon pharmacy extended notices of remarkable specimens of these formations, which have sometimes, even in Western Europe, fetched a price so high as 6000 livres, and the hire of a ducat a day when lent out for a single occasion.

The substance so familiarly employed in the perfumer's art, under the name of ambergris, is originally formed in the large intestines of the spermaceti whale, a single mass of which has been known to attain to the gigantic size of 5 feet 8 inches in diameter, weighing 182 pounds, and fetching as a first price 11,000 dollars.[1]

The large masses of felted hair which form the ægagropiles of herbivora,[2] and the masses or balls of debris composed of the claws, feathers, hairs, and bones of their victims ejected by birds of prey, are sufficiently familiar to all the members of the Society.

In the human subject such like formations have been frequently enough met with,[1] and were, in Scotland more especially, at a former period far from being uncommon. Since, however, the preparation of the oat for domestic use has been conducted by something more effectual than the imperfect thrashing, winnowing, grinding, and sifting in familiar use in every farm throughout the country in the early part of the century, cases of enteroliths in the human subject have become rarer and rarer.

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[1] Philos. Trans., 1734. Id., lxxxi.

[2] La Fosse, Cours d'Hippiatrique, p. 158. Annales de Chimie, tom. xx. Philos. Trans., xxiv. 1703, Thoresby. Id., xliv. 1746, Bailey. Id., xlviii. 1754, Watson. Id., xxxiv., No. 398.

The vegetable and fibrous character of the nucleus of these human enteroliths was long ago indicated by Fourcroy and Vauquelin,[2] while Drs Duncan, Thomas Thomson, and John Davy, from their analysis of some specimens belonging to the unrivalled collection of Dr Munro, state distinctly that they contain a substance analogous to or identical with vegetable fibre. The source of this vegetable fibre was, however, a matter of doubt until the sagacious suggestion of Mr Clift, conservator of the Royal College of Surgeons of England, "whether it might not proceed from oats," led to the examination of the microscopic structure of this cereal by Dr Wollaston, with the result of verifying Mr Clift's conjecture.[3]

It was reserved for Dr Douglas Maclagan to elucidate this subject fully so as to settle beyond a doubt the real sources of the vegetable matters contained in these cases. His paper, in the September number of the Edinburgh Monthly Journal for 1841, contains a most valuable description and analysis of two enteroliths of this kind, the one sent him by Mr Turner of Keith, the other belonging to Mr Syme, and obtained from the Highlands. In regard to these cases, Dr Maclagan observes, "From these observations, there can be no doubt that the correct statement as to the constitution of these concretions is that they are composed essentially of the hairs of the caryopsis of the oat, mixed with fragments of the husk of the grain" (p. 648).

For the formation of the intestinal calculi, two circumstances seem to be necessary:—

1. The existence of a foreign body in the intestines for a sufficient length of time to permit the accretion of a deposit on its surface.

2. The presence in the economy of a sufficient amount of calcareous salts to furnish a calcareous concretion.

The foreign body, which acts as an irritant, and upon which the calcareous deposit is collected, can constantly be recognised in the centre of these enteroliths when a section is made of the concretion. The properties essential to its fulfilling that nuclear function are, that it should be insoluble in the gastric or intestinal juices. Thus, animal hairs and insoluble vegetable fibres and hairs have been discovered in a large proportion of the cases composing the nucleus, but a gall-stone, or fruit-kernel may serve the purpose and afford the necessary surface for further accumulations. Pins, needles, small nails,[1] pieces of bone, as well as stones of fruit, have been observed forming the nuclei of alvine concretions.

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[1] M. Lawer, by Dr Konig, Philos. Trans., 1686, weighed 5 lbs. Martineau, Philos. Trans., 1722–23, 8 inches circumference, 6⅝ in. length, weighed 2 oz. 16 dwts. 12 grs. Mr Mackarness, Philos. Trans., 1739–41, 10½ in. circumference, weighed 8½ oz. Samml. Med.-Wahrn. Band ix., p. 231, weight ½ lb. M. Daabal, Discursus Academ. de Esthera Norra, Lund. 1715, 8vo. Sir H. Sloan refers to Birch Hist. 1685, 200 concretions. Edin. Med. Comm., vol. viii., p. 329, weight, 8 oz. and 3 drs.

[2] Annales du Museum, vol. iv.

[3] Marcet, Calculous Disorders.

In illustration of the capacity of gall-stones for forming the nucleus of an intestinal concretion, I may mention the following instance which occurred to me about a year ago:—

I was requested to see Mrs ——, who had suffered from a very severe attack of hepatic colic, due to the passage, it was believed, of a gall-stone, but who had not, when I saw her, passed any such formation in the excretions. There had been, however, after the subsidence of the colic, a good deal of intestinal uneasiness characterized by shifting pain, flatulence, and constipation. These symptoms had abated under the influences of a dose of castor-oil and laudanum, which had afforded a loose bilious evacuation. The patient complained of pain at the fundament so severe as to cause her to scream when the bowels threatened to act, together with the sensation of a solid mass which came down to the orifice, as she expressed it, and then went up again. On placing the patient under the influence of chloroform, and examining the bowel, I at once recognised a fissure of the anus extending up the depth of the sphincter. This ulcerated chap I divided in the ordinary manner, and on carrying the finger higher in the bowel came in contact with a dense mass of the consistence of putty, about the size of a large orange, but of an ovoid form. This was situated too high up to reach its distal side with the finger. I accordingly made use of the shank of a silver tablespoon, by means of which I broke it down, and extracted it piecemeal from the rectum. The mass consisted in great part of fæces of a putty or pipe-clay colour, but as thickly studded with gall-stones as a plum-pudding with raisins and currants. Had this mass remained unremoved for a lengthened period of years, there can, I think, be little doubt it too would have been encrusted with calcareous matter, and thus have given rise to the formation of a true enterolith.

I suspect however, that the rectum is not, in all probability, that portion of the intestinal tube best suited for affording a calcareous incrustation upon matters long retained in its cavity, as is illustrated by the following case.

I was requested by Dr Husband in 1860 to see with him a young man suffering from retention of urine, in whom some difficulty existed in introducing the catheter. The patient had long been the subject of paraplegia, and the atrophied limbs were so curled across each other, and folded over the pubis and abdomen, as to render it a matter of difficulty to gain access to the urethral orifice. The catheter when inserted passed easily enough as far as the membranous portion of the urethra, but here its further progress was completely arrested. On introducing my finger up the rectum to determine the cause of this anomalous state of matters, the progress of my finger into the rectum was opposed by a mass contained within the bowel, but occupying the entire limits of the true pelvis, and of a consistence so hard that I could not introduce my finger into its substance. Scraping the surface with the nail, I brought away a sufficient quantity of the mass to convince me that its bulk was composed of hardened fæces. As this was manifestly the obstacle which prevented the escape of urine and the introduction of the catheter, I determined, with the approval of Dr Husband, to break it down and extract it from the bowel. I effected this, after much labour, by means of the handle of an iron spoon, and now found the catheter could be introduced into the bladder without difficulty. On examining the broken-down mass which formed the ball, I found it was in great part composed of hardened fæces, combined with which there was intermixed a quantity of fibrous substance like tow. There was however no manifest calcareous coating in any portion of the mass. But in the operation of breaking down, the shank of the metal spoon seemed constantly to grate against calcareous particles mixed with the general mass. This patient had long suffered from constipation of the bowels, but for some years past had experienced alternating attacks of constipation and diarrhœa of a dysenteric type, or with symptoms of proctitis. I could obtain no history to account for the fibrous substance in the fæculent mass. The patient had never swallowed such material intentionally, nor had it ever been inserted into the anus as a plug to check a troublesome diarrhœa, a plan I have known resorted to among soldiers when they suffered in this way.

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[1] Philos. Trans., xlviii. 1754, Watson. Id., xliv. 1746.

The felt-like residuum of the calculus in cases of regular bezoars, or enteroliths, which has been so constantly observed, need occasion no difficulty in those cases where hairs, capable of being formed into felt, form the mass of the concretion. The difficulty has chiefly existed to explain the accumulation of such delicate fibres and hairs as those met with in the vegetable kingdom, which are not specially endowed with fine processes projecting from the surface, on which the facility in producing felt with the hair of rabbits and hares is now known to depend. This difficulty will, however, be removed when we consider two facts:—1. That in the drain-pipes of domestic houses obstructions are constantly produced by masses or balls of human hair, which have apparently accumulated from the combings of hair-brushes. 2. Marine ægagropiles, again, are apparently due to the rolling motion of the waves, weaving together into ball-like masses the fibres of marine plants subjected to their action. Similarly, in our own country, the needle-like leaves of the larch dropping into fresh-water lakes, are susceptible of being moulded into these singular balls, composed of woody fibre, resembling the *fir-wool*, or brownish cottony fibre, employed in some parts of Germany for the manufacture of textile fabrics. Clay balls are frequently met with in the interior of the common garden roller, and may even contain a nucleus which rattles within the outer layers when the mass is shaken.

The different appearances presented by the enteroliths, are apparently to be referred to two causes. 1. The original formation of the concretion; and, 2. The length of time it has been allowed to remain in the intestine. If the foreign substance has not been long retained, it will present the aspect of felt, more or less mixed with fæculent matters. If, again, the foreign accumulation remains lodged for a long period, the ball becomes coated with calcareous salts, mixed with the elements of the bile, presenting a curious varnished or enamelled look, or, in some instances, encrusted with a calcareous shell, formed of a number of layers of saline matters arranged in concentric circles.

The form of the enterolith is apparently modified by (1.) The form of the nucleus, when such exists; (2.) The effects of the peristaltic movements of the gut in which the calculus is contained; and (3.) The existence of *one* or more of the concretions—in the latter instance the form being modified by mutual friction.

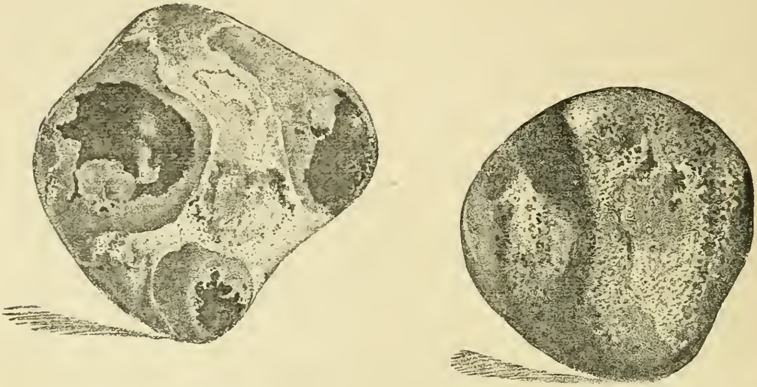
All these features will be illustrated by the history and appearance of the intestinal concretions I have now the honour of bringing before the notice of the Society.

In the early part of the month of August, I was requested by Dr Bowie to examine a patient, J. T., by whom he was accompanied. He complained of a sense of weight and fulness in the region of the rectum, frequent desire for stool, and a complete inability to pass anything except flatus and fluid excrement, saying that a lump came down with every effort, and blocked up the passage. The patient was a spare, thin, pallid man of upwards of fifty, who had long been in bad health, suffering for more than twenty years from indigestion, characterized by abdominal pains, sickness, and vomiting, with frequent constipation. Gentle aperients, which he required to take frequently, alone afforded relief.

On examining the anus, nothing wrong could be perceived; but on introducing the finger up the bowel, two large calculi could at once be recognised, conveying much the sensation experienced on introducing the finger into the bladder in the operation of lithotomy.

Feeling that, with one finger, it was impossible to steady the calculi so as to effect their extrusion, I introduced the fore and middle fingers together, and at once effected the ejection, first of one, and then of the other stone. Whenever this was effected, the patient experienced complete relief from all the painful symptoms which had annoyed him for so long a period.

The only further fact in the history of the case which I was able to elicit was, that some years ago a hard lump had been recognised in the right hypochondriac region. He had consulted, as he said, everybody in regard to this swelling, which could be distinctly felt through the abdominal parietes. But while he received various opinions and different advices, the swelling moved gradually towards the left hypochondriac region, and then disappeared. Since then he had not recognised the existence of any swelling. Coincidently with the disappearance of the tumour, the uneasy sensations in the rectum commenced, for which he sought advice in various quarters, and had the symptoms treated as piles by some, as constipation by others, and as dysentery by those who saw him when the irritation was accompanied with the discharge of liquid mixtures, mixed with mucus and blood.



External Aspect of the Calculi.—The left hand calculus was below the other.

The calculi extracted were two in number, and occupied relatively the position of an upper and an under stone. The under stone was the larger, and presented dark eyes or facets on several sides; the upper was lenticular and concave, the hollow surface lying in contact with the facets of the lower stone, which, as it turned with the peristaltic action of the bowel, was obviously polished by friction. The close resemblance of the calculi to flints, as they were forcibly ejected from the bowel, made me almost ask the man how he came to have flints up his fundament, whether he put them there, or when he had swallowed them; but the lightness of the calcareous mass, when I held them in my hand, at once undeceived me, and assured me that I had no natural pebbles to deal with, but a case of genuine intestinal concretions.

For the following accurate description, together with the analysis and microscopic appearances of these calculi, I am indebted to my friend, Dr Fraser:—

"*Report of Examination of Two Intestinal Concretions from a Patient of Dr P. H. Watson.*

"Each concretion was obtained in two portions. One concretion consists entirely of a dense stone-like body: the two portions of this are labelled '1 *a*' and '2 *a*.' The other consists of a dense crust and shell surrounding a large soft and friable nucleus: its two portions are labelled and referred to in this report as '1 *b*' and '2 *b*.' They have both an unmistakable fæcal odour.

A. “*Weight*, ... ‘1 *a*’ = 123 grs.
‘2 *a*’ = 415 grs.

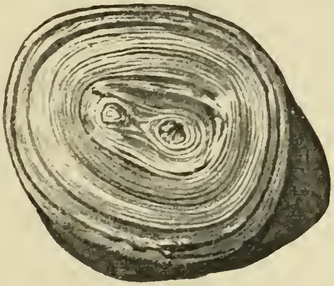
“Total weight, ... 538 grs.

As more or less must have been lost in making the section of this concretion, its original weight was above 538 grs.

“It has an irregular, slightly nodulated form, and has a length and breadth of about one inch and three-eighths, and a thickness of one inch and an eighth.

“The surface is principally of a stone colour, and covered with minute, glistening crystals; but there are several irregular patches of a grayish-brown colour, smooth, and without crystals.

“The section shows this concretion to consist of concentric layers of alternating pale-brown and grayish-white. The *crust* is an extremely thin pale-brown layer; the *shell* forms the great portion of the concretion; and the *nucleus* is a minute, dense, pale-brown body, with a long diameter of about an eighth of an inch, and apparently similar in composition to the pale-brown concentric layers of the shell. From the arrangement of some of the rings, it is probable that a second nucleus existed, but it has been destroyed in making the section.



“The *composition* has been found to be in great part phosphate and carbonate of lime, and ammoniaco-magnesian phosphate. Small quantities of soda, cholesterine, and fatty matter, and traces of silica and of bile pigments, were also discovered. The nucleus consists principally of phosphate of lime.

“This concretion is not, therefore, of biliary origin, and its composition agrees with that of previously described intestinal calculi.

B. “*Weight*, ... ‘1 *b*’ = 289
‘2 *b*’ = 338

“Total weight, ... 627 grs.

As some loss must have occurred in dividing this concretion, its original weight was more than 627 grs.

“Its form is an irregular cube. The two largest opposite surfaces have each three unequal prominences, the summits and a considerable portion of whose surfaces are of a shining black colour.

"The greatest length and breadth of this concretion is about one inch and three-eighths, and the greatest thickness rather more than one inch and two-eighths. The *crust* and *shell* together vary from two-eighths to one-eighth of an inch in thickness, and consist of concentric rings, one of a pale brown alternating with one of a dirty gray colour.

"The greatest portion of this concretion consists of a brownish-gray, soft and friable *nucleus* of an apparently cubical form, and in the exposed section, having a diameter of from one to one-and-an-eighth inch.

"The *crust* and *shell* were found to be composed chiefly of phosphate and carbonate of lime, and of ammoniaco-magnesian phosphate; traces were also obtained of cholesterine, of bile pigments, of silica, of fatty matter, and of an extractiform body having a strong fæcal odour.

"The *nucleus* was found to consist of at least 85 per cent. of organic matter; the remainder was, in great part, composed of phosphatic salts and of carbonate of lime, and it contained a minute quantity of silica and of fat. When a small portion of the nucleus was examined with a high magnifying power, an immense number of short hairs were seen mixed up with fragments of cellular tissue, with well-marked spiral vessels, and with amorphous granular bodies.

"The hairs were about one-fifteenth of an inch in length and 1-1000th of an inch in thickness; but some were found to vary considerably from these measurements. They have a finely-pointed apex, and a somewhat rounded base, the latter being often similar to the bulb of an ordinary animal hair, and therefore at first suggesting an origin of this kind. A minute canal runs from the apex to the base, and, when water is added during the examination, it is seen to gradually extend along this tube.

"The addition of sulphuric acid and tincture of iodine produced a blue colour in the fragments of cellular tissue, and more or less distinctly in the spiral vessels and hairs. They were thus shown to be of vegetable origin; and a careful comparison has proved these hairs to be identical with those that occur on the surface of the seed of the oat (*Avena sativa*)."

These analysis and microscopic examination will be seen to correspond in every particular to that given by Dr Maclagan in the two cases he subjected to analysis.

	Mr Turner's.	Mr Syme's.
Water	10	10
Albumen	2	2
Fæcal Matter	6	2
Soluble Vegetable Matter	8	8
Lactate of Soda	2	2
Salts (Muriates and Sulphates)	2	2
Fatty Matter (Stearic Acid)	8	4
Phosphate of Lime, with traces of Sulphate	20	20
Fibrous Matter	36	44
Silica	6	4
Loss	0	2
	100	100

Dr Maclagan, in speaking of the microscopic appearances of these two specimens he had an opportunity of examining, says—"On submitting portions of these concretions to examination by the microscope, the hairy matter of the oat was at once recognised as forming by far the largest proportion of the constituents of the concretions. When a portion of the mass, scraped down, was viewed under a magnifying power of 50 diameters, it presented the appearance of numerous hairs mixed with broken fragments of other matters, some of which were opaque, others being more or less translucent-like portions of vegetable membrane. The hairs varied in size, but were on an average about 1-16th of an inch long, and 1-1000th of an inch broad at the centre. When viewed with a magnifying power of 300 diameters, their tubular structure became at once apparent. The cavity contained what looked like dried cellular tissue. They were closed at the points, but readily admitted water by imbibition, which could be seen moving along the tube. Dr Wollaston thinks them to be pointed at both ends. This is not strictly the case. The apex is pointed, and they become thicker towards the centre, but the other extremity I found to be invariably broken off and open, and though they tapered towards the end, they were never found to be pointed."

The diagnosis of this case, so far as it came under my observation, was unattended by any difficulty. At an earlier period also, when the lump was recognised in the right hypochondriac region, and shifted thence to the left, any one acquainted with Professor Munro's observations should have found no difficulty in determining the nature of the affection. The early history of enteric concretions, consisting simply of a long period of digestive disturbance and uneasiness, may readily elude the diagnostic skill of the most painstaking and careful practitioner. If the nature of such a case were satisfactorily determined, beyond avoiding the cause of the formation in ill-ground and worse sifted oatmeal, the use of oleaginous substances which seem to interfere with the felting process, and the use of simple laxatives, there seems no room for interference. Nor while traversing the small intestines, if recognisable at that period, is there anything which can well be attempted, unless indeed gastrotomy were employed in a hopeless case of obstruction produced by such a cause. After the concretion has reached the large intestine, however, there seems no reason why the distending enemata recommended by M. Jules Cloquet should not be resorted to as a means of facilitating their onward progress towards the rectum. This process he well describes under the name of "*Dilatation graduelle de l'intestin*," consists in separating the walls of the intestine gradually by an increasingly large quantity of liquid. For this purpose he recommends that a conical tube should be introduced into the rectum, so as to prevent the escape of fluid from the anus, while the forcible injection of fluid is employed for the purpose of distending the bowel. The quantity of liquid which may be safely injected in such circumstances amounts, he says, to two litres. By means of this the walls of the intestine are separated from the concretion, and the whole passage rendered more capacious, and with less irregularity of surface and fewer curves. When accordingly the fluid is allowed suddenly to escape, the concretions are floated onwards, and sometimes even expelled from the anal orifice. These injections should day by day be increased in quantity until the desired result is attained.

When lodged in the rectum, the employment of lithotomy forceps will undoubtedly afford a very great facility in effecting their removal; but, as we have seen, when these are not at hand, the fingers, either alone or aided by the shank of a spoon, will suffice for this purpose.

ARTICLE III.—*On Intermittent or Paroxysmal Hæmaturia.* By EDWARD HEADLAM GREENHOW, M.D., Fellow of the Royal College of Physicians, Consulting Physician to the Western General Dispensary, Assistant Physician to the Middlesex Hospital, etc.

WITHIN the last ten years, I have had under my care no less than seven cases of the remarkable disease generally known as intermittent hæmaturia. Judging from the experience of other physicians, rather than from my own, this disease, which has been observed only of late years, would seem to be of very rare occurrence, for ten or eleven other cases, published by as many different observers, are all that I have been able to find in English medical records. M. Rayer also refers to the subject in his work on Diseases of the Kidneys, and quotes a case, published by M. Gergères of Bordeaux, which appears to have been exactly of the same character as those which have come under my own observation. In all the cases which I have seen of this disease the attacks have been excited by the same cause, have taken the same form, exhibited the same general group of symptoms, and run the same definite course. The immediate exciting cause of the attacks has been invariably some definite exposure to cold or wet. The form taken by the attacks has been invariably paroxysmal, the paroxysms coming on suddenly, almost immediately after the chill was experienced, and passing off rapidly when the effects of the chill had been counteracted and the patient had become thoroughly warm. The general group of symptoms has been invariably the same, the additional ones occasionally exhibited being apparently due to an unusual degree of constitutional disturbance. In every instance the paroxysms have begun with coldness of the extremities, followed by general chilliness, amounting in the severer attacks to rigors. In like manner, in every case, the chilliness or shivering has been attended by a feeling of weight and pain in the loins, and by pain or a sense of weakness or stiffness in the lower limbs. The chilliness has been usually, though not always, followed by an imperfectly marked febrile hot stage. The patients have invariably passed, during the paroxysms, urine looking as if it were mixed with blood, and identical in general character. And, lastly, the same definite course has been run by the paroxysms in every case. From half an hour to two hours after the chilliness or rigors the patient has never failed to pass the first dark-coloured urine, which has always been highly albuminous, and has contained numerous crystals of oxalate of lime, with more or less of brownish, or yellowish-red, amorphous granular matter and a few hyaline casts, but only occasionally some stray blood-corpuscles. At each succeeding micturition after the chilliness, the urine has invariably shown more or less diminution of colour, of albumen, of oxalate of lime, and of its other abnormal contents; resuming its natural character and appearance by the second or third micturition after slight attacks, and usually by the fourth or fifth after severer paroxysms. By the second day after an attack the patients, as a rule, have regained their ordinary degree of health and strength, and have continued well until some fresh exposure has brought on a new attack of their complaint. To this summary I may add, that, so far as my observation extends, all the patients suffering from this disease have had the same pale, sallow, cachectic aspect, two of them having been distinctly jaundiced, and the others having all had at times an icteroid tint of skin.

It appears to me that this train of identical circumstances, in so considerable a number of cases, places it beyond question that intermittent, or, as I think Dr Pavy more accurately terms it, paroxysmal hæmaturia, is a definite disease, due in all cases to the same remote constitutional cause. It is quite certain that the only medical treatment which appears to have any effect in diminishing the liability to suffer from the paroxysms on every exposure, is treatment directed, during the intervals, towards the improvement of the general health and the strengthening of the patient's constitution. Once the paroxysms have set in, the most simple means for restoring warmth are all that I have found it advisable to use. The short duration of the paroxysms, the very general absence of blood-corpuscles from the urine, even at the time of the paroxysms, and the complete recovery of the normal character of the urine within a few hours after their subsidence, seem to show that there can be no cause of hæmorrhage in the kidneys themselves, but rather that the disintegrated blood transudes through the walls of the bloodvessels in the Malpighian bodies. Indeed, I regard the kidneys rather as the organs of elimination than as the seat of the disease, and I consider the renal irritation, the existence of which is manifested by the pain in the loins and by the presence of casts, and occasionally also of stray blood-corpuscles in the urine, to be a secondary affection consequent upon congestion of the kidneys produced by the sudden and unusual strain thrown upon them. Possibly the paroxysms may consist in the sudden disintegration of an unusually large quantity of blood-corpuscles, setting free so considerable an amount of hæmato-globulin that it cannot undergo the normal changes, but is eliminated through the kidneys in a comparatively unaltered state. I would, however, by no means dogmatize on a subject at present so obscure, and will now subjoin the reports of the four latest and most accurately observed of my seven cases.[1]

Case I.—James Farrer, aged 31, a greengrocer, of temperate habits, was admitted into the Middlesex Hospital under my care on the 17th of September 1867.

History.—The patient lived in London, and had never lived in a marshy district, nor had ague, but had had syphilis four years before his admission. In June 1866 he had had the first attack of rigors, pain in the loins and hæmaturia. Remained well for a year, but three months before his admission was laid up with rheumatism in the knees, ankles, and loins. A fortnight later, he had a second attack of shivering, with pain in the loins, followed shortly by hæmaturia, and five similar attacks between that time and the date of his admission, all but one subsiding again entirely in a few hours. He attributed every attack to some exposure to cold or wet, and said that the first urine passed after the shiverings was always blood-coloured, the second paler, and the third or fourth was usually quite clear and normal again.

State on Admission.—The patient was sallow, unhealthy looking, slightly jaundiced, and emaciated. His left knee was weak, painful, and swollen by synovial effusion. His pulse was 80, small and thready, his skin was cold, and he was shivering and suffering from pain in the loins. Said that he had got chilled in coming to the hospital, and was about to pass bloody urine. Was sent to bed, and two hours later I found that he had passed some dark-coloured urine not unlike porter, but with a redder hue. His pulse had risen to 110, and his skin had become hot, the temperature in the axilla being 103°·2. There was tenderness on firm pressure in both hypochondria, but especially the left, and the same, though less marked, in the loins. The liver dulness was normal. The *first* urine passed after his admission was faintly acid, sp. gr. 1021, and deposited on boiling a copious chocolate-coloured precipitate, partly soluble in nitric acid. Under the microscope abundant crystals of oxalate of lime were seen, some coloured granular matter, and a very few hyaline casts, some of which contained dark granules. No free blood-corpuscles were found. The *second* urine passed after the chilliness was of a port-wine colour, sp. gr. 1021, and yielded an abundant deposit of albumen, with heat or nitric acid. The *third* urine had the colour of port-wine and water, and was still very albuminous, sp. gr. 1009. The *fourth* urine was paler, sp. gr. 1004, became cloudy with heat or nitric acid; but the deposit was entirely re-dissolved in an excess of acid. The *fifth* urine was pale-coloured, sp. gr. 1004, and showed a mere trace of albumen.

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[1] The four reports of cases, together with the concluding remarks, form an abstract of a paper communicated to the Clinical Society of London on the 14th of February 1868.

Progress of the Case.—On September the 18th, the patient had a slight attack of chilliness and pain in the loins, after which the pulse rose to 86, and the temperature to 100°. The *first* urine was of a reddish hue, sp. gr. 1015, and exhibited a small deposit of albumen, but no casts. The *second* urine was pale-coloured, sp. gr. 1005, and free from albumen. On the morning of the 20th, the patient had a return of rigors at half-past eight o'clock, after which the pulse rose to 90, and the temperature to 100°. The *first* urine passed at 9 A.M. was of the colour of port-wine, and very albuminous; the *second* was much less deeply coloured, and less albuminous; the *third* was of a dark amber colour, and quite free from albumen. All the specimens contained more or less oxalate of lime and amorphous granular matter. On September 23d, the patient had catarrh and sore throat, and both on that day and the 24th he had slight attacks of chilliness, and passed urine containing oxalate of lime and a small quantity of albumen. From that time he had no return of hæmaturia, though he had an attack of bronchitis attended by rheumatism. The left knee was eventually blistered without causing renal irritation, and the urine remained free from albumen and oxalates. The patient was discharged convalescent on October 31st.

State when last seen.—He continued under observation until November 29th, remaining quite well, and having gained 20 lbs. in weight whilst under treatment.

Treatment.—The treatment consisted at first of full doses of quinia with tincture of perchloride of iron, and later of half-drachm doses of syrup of iodide of iron with two grains of iodide of potassium, three times a day, and a pill containing two grains of sulphate of quinia night and morning, until he was discharged.

CASE II.—Francis Bray, aged eight and a half years, was admitted an out-patient at the Middlesex Hospital, under my care, October the 6th, 1866.

History.—He was born in Camden Town, and had scarcely ever left London. His father had had rheumatic fever some years before, and was now suffering from pain in the knuckles, which were much enlarged; one of the father's sisters also suffered from rheumatic gout. The patient himself had always been delicate, and was only two years of age when his mother first observed his urine look red, as if mixed with blood; this appearance had been preceded by shivering and thirst. From that time she noticed that the boy had similar attacks whenever he got chilled. Sometimes, she said, the urine looked more like black-beer with a froth on it, and then it always deposited a sediment. Shiverings, fever, thirst, and pain in the loins and abdomen, had always preceded the attacks, which were also attended by great weakness and stiffness of the legs, and frequently also by vomiting and pain in the left side. Sometimes the feet, and more rarely the hands, swelled and became purple during the attacks, and the ears often became cold, purple, and painful to the touch. The first urine passed after the shiverings had always been dark-coloured, the second less so, and the third or fourth had recovered the normal appearance.

The boy had usually been pretty well again by the day after an attack, and remained so until he took some fresh chill, which occurred very frequently in winter.

State on Admission.—Was pallid and delicate-looking. Pulse 79; tongue clean; bowels regular; urine normal. Heart and lung sounds perfectly normal.

Progress of the Case.—On 9th October, the patient had a slight shivering and passed some dark red urine, containing a considerable proportion of albumen. On 13th October, he had a very severe attack of rigors, with much pain in the limbs and intense thirst. The urine was dark red, contained much albumen, and under the microscope showed numerous crystals of oxalate of lime, some blood-corpuscles, a few casts, and epithelium. On 25th October, he had another severe attack, attended by vomiting. The urine brought for examination was very albuminous and contained crystals of oxalate of lime and coloured granular matter, but the urine passed next morning, whilst the boy was at the hospital, was pale-coloured and normal. On 5th and 16th November, he had slight attacks, when the urine contained only a small quantity of albumen, and showed under the microscope only a few epithelial casts and no crystals of oxalate of lime. From that date he remained quite well until February 1867, when, after getting wet, he had rigors unattended by pain in loins or limbs, and the urine passed after the attack was free from albumen. The following month he ceased to attend, but I heard from his mother, two months later, that he had had no return of his ailment.

Treatment.—The case was watched for a fortnight without definite treatment. At the end of that time I prescribed a combination of tincture of perchloride of iron, diluted hydrochloric acid, and sulphate of quinia, which was continued with slight modifications throughout the period of the boy's attendance at the Hospital.

Case III.—Jane Sibley, aged 35, a married woman, pallid and sickly-looking, was admitted an out-patient at the Middlesex Hospital, under my care, on 4th May 1866.

History.—Her father was very rheumatic and asthmatical, and the patient herself had often had bronchitis. She had been subject to hæmaturia for two and a half years previous to her admission, but had never had ague, nor had she ever lived in a marshy district. She stated that the attacks occurred only after exposure to cold or damp, and that they always began with chilliness usually passing into rigors attended by pain in the loins, and followed in an hour or two by the passing of blood-coloured urine. She often had pain in the side and cold feet. Said that she had had an attack, and had passed urine of a deep purple colour the day before her first attendance, but she had not preserved it. The urine brought at her second visit to the Hospital was of a pale amber colour, sp. gr. 1022, and free from albumen.

Progress of the Case.—The patient attended irregularly during three months, without having any attack. On the 16th of August, having got chilled during a long drive in the evening in an open cart, she had rigors on reaching home, followed by burning heat, but no sweating, and she soon afterwards passed some urine of a deep port-wine colour, which she brought to the Hospital next day for examination. It had a specific gravity of 1022, and contained a large proportion of albumen, which was of a deep chocolate colour when thrown down by heat or nitric acid, and left the supernatant urine clear and of a pale straw colour. Under the microscope some blood casts and oxalate of lime crystals were seen, and the urine deposited, on standing, a large amount of coloured granular matter. For two months from this time the patient remained well, but on 17th October, after standing all day in a damp wash-house, she had an attack of rigors, attended by pain in the loins and followed by heat and by the passing of dark-coloured albuminous urine, which, under the microscope, showed some oxalate of lime crystals and blood-casts and a few blood-corpuscles. The urine passed on the following morning was paler but of the same general character. That passed on the morning of the 20th was pale-coloured and normal. On 23d November, she had another but slighter attack, and from that date had no return whilst under observation. She ceased to attend early in 1867.

Treatment.—The medical treatment consisted throughout of some form of chalybeate, with or without quinia, according to circumstances.

CASE IV.—Martin Nichols, aged 34, single, a blacksmith by trade, was first seen by me, 7th November 1866, at Mr Nunn's request, whilst in the Middlesex Hospital under his care, for a hard tumour of the testicle, which he considered to be syphilitic.

History.—The patient was born in Cornwall, but had long lived in London, and had never had ague. He had been accustomed to drink beer freely. Had enjoyed good health until he became subject to hæmaturia four years before his admission. The attacks had come on with a sense of coldness in the feet, followed by shivering, pain in the right hypochondrium, and pain and great weakness in the loins. Soon after the shiverings he always passed urine of a dark red colour, as if mixed with blood. At the next micturition the urine would be less deeply coloured, and by the third or fourth it had usually returned to its natural colour. The cause of these attacks he stated to have always been some exposure to cold or wet, and he had suffered from them more or less frequently during the last four winters, but had become free from them each year in April or May, and had continued well until October or November. He had been an out-patient at University College Hospital, under Dr G. Harley's care, from December 1864 to May 1865, during which period he did no work, and had only three attacks of shivering and hæmaturia. After his discharge in May, he continued well until the autumn, but during the winter 1865–6 had seven or eight very severe attacks. He, however, lost them as usual about April, and had had no return up to the date of my seeing him.

State when first seen.—The patient had a sallow, cachectic, slightly jaundiced appearance. His pulse was 88, compressible; his urine was transparent and non-albuminous, but, under the microscope, showed characteristic crystals of oxalate of lime.

Progress of the Case.—He left the hospital without having had any attack of hæmaturia, and was re-admitted under my care about Christmas for an attack of jaundice. The urine was deeply coloured with bile, and deposited a large amount of lithates, but remained free from the colouring matter of blood, and only once contained a trace of albumen. After a few weeks he was again made an out-patient, and on 21st March 1867, whilst walking home from the hospital, he had a shivering fit, and on reaching his house passed some blood-coloured urine. By the next day he was well, and remained so until 28th March, when he became chilly in the out-patient room, and soon passed some dark red urine closely resembling port-wine. It was very acid and albuminous, and had a specific gravity of 1026. On microscopical examination, a few blood-corpuscles were found with oxalate of lime-crystals, and blood-casts. The second urine passed was of the same character, with more casts and amorphous granular matter, but fewer oxalates. The third and following urines showed the usual gradual diminution of colour and of albumen, and that passed the next morning was amber-coloured, deposited no sediment on standing, and exhibited a mere trace of albumen.

State when last seen.—The patient is still under observation, evidently in much better health, and working constantly at his trade. Since March 1867 he has had no return of hæmaturia. His urine has been examined almost weekly up to January 24, 1868, and has been always of normal colour and free from albumen.

Treatment.—The remedies prescribed for the hæmaturia were the sulphate of quinia and tincture of perchloride of iron. For several weeks after the last attack he took half-drachm doses of syrup of iodide of iron, with three grains of the iodide of potassium, three times a day.

Amongst the English medical authors who have related cases of intermittent or paroxysmal hæmaturia, are Sir Thomas Watson, Dr Elliotson, Dr Prout, and, more recently, Dr G. Harley, Dr Dickinson, and Dr Gull. Several of these authors have considered the disease to be of malarious origin, and there is no doubt that in some of the cases published the patients had also suffered from ague. But, on the other hand, not one of all my patients had suffered from ague, and some of them at least had never visited a malarious district. The same absence of malarious influence has been noted by Dr Pavy and Dr Murchison, in cases published by them in the Pathological Transactions. The disease, in fact, merely resembles ague in its paroxysmal form and in its commencement with rigors, which are followed, however, by only an imperfect hot stage, and rarely by a sweating stage. It differs from ague in not being periodical, and in requiring, apparently, a fresh exposure to cold or damp to excite each separate paroxysm. The history of the foregoing cases, as well as that of the others which have come under my observation, seems to me clearly to indicate the existence, in all the patients, of some form of dyscrasia, upon which the external chill acts only as the immediate exciting cause of the paroxysm. One fact, which I cannot but consider as very significant, though I am unable at present to give it any definite interpretation, is the constant presence of oxalate of lime crystals in the urine during the paroxysms, and their absence in almost all cases at other times. It may be remarked, however, that the sallow, unhealthy aspect of patients suffering from this disease is not unlike that attributed by Dr Prout to the oxalic acid diathesis, of which he further mentions a tendency to hæmorrhage from the kidneys as one of the concomitants. Another fact which appears to me worthy of notice is, that all the four patients suffered from so-called rheumatic pains during the paroxysms, and the first and fourth at other times also. In these two last mentioned cases it is indeed possible that secondary syphilis may have had to do with the occurrence of the rheumatic pains at ordinary times; but the same authority (Dr Prout) has remarked the frequent co-existence of chronic rheumatism with the oxalic acid diathesis, and Dr Salusbury, an American physician, has lately described a form of rheumatism due, in his opinion, to the presence of oxalate of lime in the blood. No opportunity has occurred to me of investigating the morbid anatomy of this disease, but it seems to me tolerably certain, from the clinical history, that there is no permanent lesion of the kidneys, notwithstanding the temporary congestion of those organs which takes place during the paroxysms, and I strongly incline to believe that the true pathology of the disease is to be sought in the condition of the blood itself rather than in any local organic change.

ARTICLE IV.—*Pelvic Areolar Inflammation and Sloughing.* By J. MATTHEWS DUNCAN, M.D.

THE following case I describe as a contribution to the history of the diseases known as pelvic abscess, or pelvic cellulitis.

These names are very misleading, inasmuch as they suggest an erroneously partial pathology of the affection they are intended to denote. The cases described under the names pelvic abscess and pelvic cellulitis, are frequently not collections of pus, and are far from being ordinarily inflammations of areolar tissue. Their most common pathology may be indicated by the name pelvic peritonitis. In March 1866, I read to the Medico-Chirurgical Society an account of some cases of this last kind, in which the local peritonitis following delivery ended in serous effusion. The paper was entitled, "On Encysted Serous Pelvic Effusions following Puerperal Inflammation," and was published in the "Edinburgh Medical Journal" for April 1866.

The present case is one in which, after a tedious history of pelvic peritonitis following delivery, and which eventually ended in at least one large collection of pus, an extensive sloughing of cellular tissue in the lower part of the pelvic cavity took place, the locality being remote from the pre-existent inflammation, which continued long after the alarm produced by the sloughing had passed. This is not a case of mere sloughing of some cellular tissue in an abscess. It is, indeed, one of a class of cases of great interest, which has been described in the male; but of which I know of not a single other example in the female. The "Lancet," some years ago, contained a brief notice of some apposite clinical remarks on this subject made by Mr Paget, in St Bartholomew's Hospital.[1] He there pointed out that some of the cases of acute inflammation and sloughing of the scrotum and adjacent tissues, which are commonly ascribed to infiltration of urine, are not connected with it, or with any other affection of the urinary organs, but are examples of the same disease as, in other parts, we call phlegmonous erysipelas, or diffuse inflammation of cellular tissue. Now while, in the case to be narrated, the appearances could not but suggest the idea that urinary infiltration was the cause of the disease, no sufficient reason could be found for seriously entertaining the notion. The bladder, in all its functions, was undisturbed during its progress. Some misconception may possibly arise from the analogies of the affection suggested by Mr Paget, if I do not point out that, in my case, the cellular tissue affected was greatly swollen up by an effusion into the areolæ of ichor or dirty-brownish serum, and that this, as well as death of the large mass of cellular tissue, took place before any noticeable suppuration occurred. Profuse suppuration followed the sloughing, and accompanied the slow process of separation and discharge of the gangrenous masses.

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[1] Vol. ii. for 1865, p. 482.

The report of Mr Paget's case and remarks, is given in the "Lancet" in the following words:—

"On the 20th of last September, we saw a man, named George B., forty-six years of age, a carman by occupation, placed upon the operating table. His scrotum and pelvis were greatly swollen, œdematous, and of a dull-greenish colour. The perineum was not involved. He was much collapsed.

"Before resorting to any operative procedure, Mr Paget directed especial attention to the patient's appearance. It so closely resembled, he said, the effects of infiltration of urine from rupture of the urethra, that it was difficult at first sight to imagine that this lesion was not present. Against this view, however, was the man's history. It seemed that the swelling had existed for four days; but that, until the preceding day, he had voided urine without difficulty. Mr Paget ventured to predict that this was a condition independent of any urethral lesion, and that a catheter would pass without difficulty; and, in effect, he succeeded immediately in passing a good-sized instrument, through which a considerable amount of urine flowed. He then, with a scalpel, freely incised the œdematous tissues. There was a little serous exudation, which had no urinous smell. Mr Paget remarked that he had met with a case of similar description the year before last, and one also a few months ago. The man was removed to bed, and poultices applied; but he failed to rally from the state of collapse, and died a few hours afterwards.

"Mr Eccles, the house-surgeon, tells us that, on making an examination of the man's body, he found all the organs healthy. The urethra was quite sound, and the kidneys were free from disease. The man, it seemed, had led a very intemperate life.

"There are points of great interest, and, may we not add, of obscurity, in a case of this kind. No cause could be discovered for this access of inflammation. There was no stricture, ulcer, or sinus upon which, according to Mr Liston, such an attack of inflammatory œdema usually supervenes. No local reason, indeed, could be found to explain the origin of this attack, and we are left in complete ignorance of the cause of its selection of this particular site; and if we fall back upon the idea that it was probably of an erysipelatous character, we still cannot explain why inflammation of a few square inches of cellular tissue at this spot, so far removed from vital organs, should cause death. We are left, indeed, to the choice of two alternatives. The local inflammation may have been a result—not a cause—of the depressed nervous force so unmistakably marked in this patient. We know that gangrene of the scrotum and pelvis is not an uncommon result of typhus fever. Or, remembering the fatality which often attends a very similar inflammatory attack arising from infiltration of urine in this quarter, we may concede the probability of a closer connexion existing between this region and the great nervous centres than physiology is yet able to explain."

Mrs N., a healthy young lady, recently married, and residing in India, was delivered, in a natural labour, of her first child in September 1863. After her confinement she suffered from what were described to me as repeated attacks of pelvic cellulitis, which rendered it necessary for her to keep bed almost constantly. Having still extensive disease in the pelvis, she was sent home and placed under my care.

I first saw her in August 1864. She had no complaint of pain, although she ached in various parts of the lower half of the body. Above the horizontal ramus of the right pubes, there was a rounded hard mass, giving the impression of something beneath as large as a big orange, scarcely tender, having an ill-defined outline, and presenting no distinct absence of resonance on percussion. Digital examination per vaginam easily made out that this mass bulged out of the right anterior quarter of the pelvic brim. It did not project downwards into the pelvic cavity. The uterus was behind it, immovably fixed. The cervix could be well reached by the examining finger, which also discovered hardness of the whole posterior half of the upper pelvis.

A blistered surface was kept open over the tumour just described, and it diminished in size, the health simultaneously improving.

As the lady resided at a great distance from me, I only knew by letter that, on September 5, 1864, she was taken with rigors, which were not severe, and that her pulse rose to 120, and remained so for many days subsequently. She was very ill. On the 7th she complained of great pain in the pelvis, and an irritation of the rectum was set up, with frequent mucous discharges. These conditions continued till I saw her on the 9th September. I then found the perineum occupied by a large, bulging tumour. The skin over it was dusky red. Examination showed that it was in size equal to a very large orange. In the middle of the mass, at its bulge, was a smaller protuberance, like the half of a small hen's egg. This was of a pale and death-looking colour, and its centre presented a red, prominent spot, like the pointing of a little abscess. The anus was displaced forwards, and to the right side, and was a long, curved opening, that would easily admit two fingers. The tumour had everywhere a soft, œdematous feeling, and there was little complaint of pain produced by the manipulations. Examination per vaginam showed that it nearly filled the pelvis, displacing the vagina forwards and to the right, as well as the cervix uteri.

I freely opened the smaller projecting mass, and there issued a small quantity of dirty-brownish serous ichor, which was not fetid, but had a disagreeable odour. There also appeared to escape a little gas, which was scarcely fetid. Being disappointed at not finding matter, I pushed my bistoury into the mass, but nothing new came forth. I then passed my finger through the wound, and found it enter into a soft, lacerable mass of areolar tissue. There was no loss of blood in the operation—there being only the slightest bloody soiling, such as a single drop of blood might cause.

I did not see this patient again for some time; but she was under medical supervision. The sloughs were long in separating and coming away, and a profuse suppuration accompanied the process. The largest gangrenous masses came away last. The skin around the artificial opening had sloughed away to a considerable extent, making the original incision now an irregular open wound.

On October the 6th I found the general health much improved. On examination I discovered a large excavation on the left side of the pelvis, communicating freely with the rectum. All traces of the sloughy swelling previously described had disappeared. In this excavation the left sacro-sciatic ligament could be felt so denuded that the finger could be passed around it. The fæces escaped freely by the incised external wound. Between the cavity of the rectum and the excavation there was a strong band just above the internal sphincter. I divided it with the knife. I subsequently also divided the sphincter ani where it separated the anus from the lateral wound. Before a month had elapsed the sphincter had completely regained its power.

In the month of November a large abscess formed on the site of the peritonitic hardening first described in the history of the case. It does not form an essential part of the case, and I shall only say that it was treated by incision through the roof of the vagina on the right side. Even after this the patient had other attacks of inflammation and abscess.

In the month of December 1864 the rectum was believed to be healed. But she has never ceased to have occasional discharges of pus per anum.

Now, in February 1868, she is fat, rosy-cheeked, and has long been in what she regards as perfect health. But there are still discharges of pus per anum, and the left side of the rectum feels unnaturally excavated, hard, and with elevations and depressions. Menstruation, which was suppressed during her long illness, has for more than two years been regular. The uterus is still only partially mobile, and there is much dense hardness in the posterior half of the brim of the pelvis.

This is the history of a lesion very rare in either sex—spontaneous, or apparently spontaneous, sloughing of a mass of cellular tissue in the pelvis, a sloughing that suggests the question of urinary infiltration, a question which is answered negatively; coming on also quite independently of any erysipelas or any surgical interference. In the male, such sloughing is not extremely rare in connexion with slight or severe operations in the perineum or its neighbourhood; but, as I have already said, and as I otherwise know, such cases as I have here recorded of spontaneous sloughing do occur in the male. My case is also remarkable as an instance of recovery, for almost every case, whether spontaneous or not, of which I have heard, has proved fatal.

Since this paper was written, I have had to thank Dr Watson for referring me to Mr Liston's paper, entitled "Remarks on the Acute Form of Anasarcous Tumour of the Scrotum,"[1] which contains some interesting cases of an affection in the male closely resembling the case I have described; cases of rapid distention of the scrotum with serosity, in which destruction of the cellular tissue and skin can be arrested only by very early and free incisions.

"This distention (he says) is or is not attended by redness or erythema of the surface; but there is reason to think, from the suddenness of the accession, and from the appearances on exposing the cellular tissue, that there is no actual inflammation of its texture; there being no induration, nor any appearance of lymph or puriform fluid in the areolæ. The affection has generally supervened upon abscess or ulcer, perhaps trifling, in the perinæum or groin. Its accession has been sudden, the swelling and tension becoming very great and alarming even within a few hours. The most dependent part, generally the posterior, will be found at a very early period to present one or more deeply-seated ash or tawny coloured spots: these extend; the integument is speedily involved; and, unless active measures be adopted, the entire coverings and investments of the testicles will be destroyed, and these organs exposed. . . . .

"The fluid effused, and which falls, as it were, into the cellular tissue of the scrotum, is often dark, putrescent, and acrid; it causes destruction of all the parts it comes in contact with, and subsequently of the skin, as certainly, and sometimes as rapidly, as if the parts were infiltrated by urine. Cases of sloughing of the coverings of the genital organs are very generally supposed to arise only from the latter cause; and it is with a view of directing attention to the diagnosis that the following cases are brought before the Society."

In conclusion, I shall, for an example, quote Mr Liston's third case:—"W. R., aged 40. Admitted into the Royal Infirmary July 21, 1834. Received a kick on the perinæum from a cow about a fortnight ago. There was much pain at the time; but the injury was followed by no further inconvenience until about eight days ago, when the pain returned, and was followed by considerable and rapid swelling of the scrotum. Nothing was applied to the swelling at the time; but, as it continued to increase, he applied for admission into the hospital.

"Upon examination, the scrotum was found to be much swelled and extremely tender. At the lower part it was of a dark livid colour; and, on pressing it, an emphysematous crackling was distinctly felt. There was little swelling in the perinæum; but on the right side, about an inch anterior to the rectum, there was a small opening, irregular in its appearance, through which the dead cellular tissue protruded, and a small quantity of what was at first believed to be urine escaped. The pulse was small and rapid, the tongue dry in the centre, and moist round the edges. Bowels reported open; and states that he passes his urine freely. Immediately after his admission, free incisions were made into the scrotum, and the opening in the perinæum enlarged. In both places the cellular tissue was found in a state of gangrene, and a considerable quantity of thin fetid fluid mixed with air escaped from the scrotum.

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[1] Medico-Chirurgical Transactions, vol. xxii. p. 288.

"The man was an habitual drunkard, and of weak intellect. He gradually sunk, and died on the 28th of July, before the sloughs had separated.

"On dissection, the whole urinary apparatus was found in a perfectly healthy state."

ARTICLE V.—*A Contribution to the Therapeutics of Croup.*

By R. W. CRIGHTON, M.D. Edin., Chapel-en-le-Frith.

THE medical treatment of croup has been so diversified in its nature, and in general attended with so small an amount of success, that I consider the results of the treatment of fifty cases, all of which I watched carefully throughout the course of the disease, cannot be altogether uninteresting or destitute of some practical value. They occurred in my practice during the past eleven years; and, with a very few exceptions, in the north-western extremity of Derbyshire—generally in deep valleys, where the surface was always to a considerable extent saturated with moisture. In the earlier years of my practice, I adopted the method of treatment which I am afraid is yet too generally followed, viz.:—local bleeding, with the warm bath, and emetics of ipecacuanha and tartarized antimony; and in some instances the mercurial plan was fairly tried after these means had failed. Recovery under the use of these remedies was so exceptional as to give me almost a dread of encountering the disease. Since 1856, however, when I first became acquainted with the treatment of croup by the sulphate of copper, as recommended by Hönerkopff, Samter, and Luzsinsky, I have employed it with gradually increasing confidence; so that, if seen moderately early, I can undertake the treatment of the disease with more prospect of success than in most of the acute diseases of infancy and childhood. The dose which I have given has never exceeded a grain—generally half a grain—and has been administered every ten or fifteen minutes until free vomiting has been produced. The relief to the most urgent symptoms has generally been so decided after the first three or four doses as to render it unnecessary to give it so frequently, or even in some cases to repeat it at all; in several instances, however, where great improvement occurred at first, I have found it useful after a few hours to give an occasional dose where the stridulous inspiration returned.

In only two instances have I seen any bad effects follow which could fairly be attributed to its employment; and in these the remedy had been given in too large doses, and after all indications for its usefulness had passed.

In both cases violent diarrhœa occurred, which proved fatal in one of the cases which I saw in consultation when the little patient was moribund. In general I have endeavoured to see cases of croup while the urgent symptoms lasted several times a day, often when possible every two hours; and the possibility of the sulphate of copper acting violently on the intestinal canal when too freely given renders the close watching of the patient an imperative duty.

In the treatment of two cases of diphtheric croup, where the sulphate of copper failed to act as an emetic, a small piece of sponge saturated with the muriated tincture of iron, diluted with about twice as much water, was passed through the rima glottidis into the larynx. Both children were livid in the face, and apparently *in extremis;* and in one of the cases the little patient was almost asphyxiated from want of power to expel the large amount of false membrane detached.

Both, however, recovered rapidly under the use of frequent small doses of beef-tea and brandy, with quinine and iron.

I tried the same practice in a third case of diphtheric croup, but although relieved for a time, the patient ultimately sank from asthenia.

When the more urgent symptoms have been relieved by the sulphate of copper, I have given every two or three hours four or five grains of chlorate of potash, with two or three grains of iodide of potassium, and ten or fifteen minims of the camphorated tincture of opium, sometimes with the addition of a few minims of liquor potassæ. This combination has appeared to act very efficiently as a solvent to any farther deposit of false membrane.

In six cases it was deemed necessary to apply a small blister to the top of the sternum for two or three hours on the second or third day of the treatment, where the symptoms, though broken in violence, did not pass off so as to remove all anxiety as to the issue.

In all, well marked improvement resulted, especially as regarded the voice.

Of fifty[1] cases treated in the manner I have described, all of which were examples of genuine croup—many of them very acute and unpromising in their characters—six died, one occurring after scarlatina, and two during an attack of diphtheria. The results which I have stated afford, I think, strong grounds for hoping that the medical treatment of croup is not by any means so hopeless as to warrant the early recourse to tracheotomy which is recommended by so many eminent authorities of the present day; and I trust that the following statement made in one of our best text-books on the Practice of Medicine may ere long receive considerable modification:—"In country districts the performance of tracheotomy in a case of croup is almost imperatively called for in the majority of cases, *if some symptoms of amelioration do not follow the steady use of bleeding, emetics, the warm bath, and calomel purgation, pursued for twelve or sixteen hours.*"[1]

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[1] All were under eight years of age.

ARTICLE VI.—*Contributions to Dermatology.* No. 1. *On the Pathology of the so-called Eczema Marginatum.* By Dr M'CALL ANDERSON, Lecturer on Practice of Medicine in Anderson's University, Physician to the Dispensary for Skin Diseases, Glasgow.

THE disease to which the name of Eczema Marginatum has been given commences on the inside of the thigh where it is in contact with the scrotum, in the shape of a small round patch, which is red, elevated, itchy, and which may, as the disease extends, become the seat of papules, vesicles, excoriations, or crusts. The eruption heals in the centre, leaving the skin discoloured, owing to pigmentary deposit, while it extends at the edges in the shape of a circle, until it may reach the size of the palm or more. Similar patches often form in the neighbourhood, which coalesce with the circle first formed in such a way that a huge circle of eruption may result, which extends nearly to the umbilicus above, the knee below, and the sacrum behind. Often inside this circle new ones form in a concentric manner, or the surface may be studded with great numbers of minute rings of eruption which, when present, constitute a very striking and characteristic feature. In many cases, too, similar patches are detected on other parts of the body.

It is met with almost exclusively amongst shoemakers and dragoons (Hebra), a circumstance which is easily accounted for by the continued moisture and friction which these occupations entail in the situations referred to.

This disease is described by Devergie[2] under the name of Herpes. He evidently suspected that a fungous growth lay at the root of the mischief, but, like our Indian colleagues, who treat of it under the title of Burmese ringworm, he judged from the appearance which the eruption presents, and had never been able apparently to demonstrate microscopically the correctness of the opinion.

Hebra treats of this complaint under the head of Eczema; indeed it was he who applied to it the term Eczema marginatum. In an elaborate review, in the last number of the Journal of Cutaneous Medicine, of a recent publication of mine, my learned friend, Mr Erasmus

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[1] Aitken's Science and Practice of Medicine, 4th Edition.

[2] Traité pratique des Maladies de la Peau. Ed. ii. p. 273.

Wilson, referring to the fact that I take a different view of the pathology of this disease from Hebra, says,—"Again, in another place, where our author ventures upon a dispute with Hebra, Hebra clearly comes off in triumph;" but, on reading further, the only reason apparently for such a statement is to be found in the following sentence:—"We assert, without hesitation, that Hebra is correct in regarding the somewhat remarkable eruption in question as an eczema;"[1] in other words, because Mr Wilson agrees with Hebra on this particular point, every one who disagrees with Hebra must be wrong.

In another place,[2] he makes the remark, that "its supposed cryptogamic origin is altogether unfounded." But it is right to remember that Mr Wilson is quite consistent in holding this opinion; for he denies the existence of vegetable parasitic skin diseases altogether.

Let us now turn to an article in Virchow's Archiv for 1864,[3] by Dr Heinrich Köbner of Breslau. In this a case of the so-called Eczema marginatum is most carefully described, the epithelial scales from which were found to be loaded with spores and tubes identical with those met with in tinea circinata. In order to verify the results of the microscopic examination, Köbner inoculated himself on the forearm with some of the scales, and in about a fortnight three very beautiful rings of tinea circinata made their appearance, the scales from which were found to be loaded with the spores and tubes of the trichophyton.

In conclusion, the following cases, which recently occurred in my practice, show that Köbner's case is not exceptional, and, taken in connexion with his, prove most conclusively that the affection under consideration is parasitic in its nature, and that in all probability it is a mere variety of tinea circinata.

A gentleman, æt. about 35, apparently in the most perfect health, consulted me on 11th December 1867, on account of what he described as an irritation of the skin, of a couple of weeks' duration. He was a great huntsman, but was obliged to give up hunting owing to its increasing the irritation.

He first showed me two small nearly circular patches on the calf of one leg, which were not elevated at their edges, and which exactly corresponded to spots of erythema. Then he showed me a painful itchy eruption on the inside of each thigh, in the situation where the scrotum lies in contact with these parts. This eruption had also an erythematous appearance; but the patches were *circular*, were *healing in their centres*, *and spreading at their edges*, which were *raised*. Struck by these circumstances, and also by the fact that at the spreading edges the small hairs were broken and unhealthy, I removed some of the scales, and examined them with the microscope, when I discovered spores, some of them isolated, some in chains, and tubes of a fungous growth. (See Fig. 1).

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[1] Journal of Cutaneous Medicine, No. 5, p. 51. London, Churchill.

[2] Ibid., No. 4, p. 377.

[3] Archiv für pathologische Anatomie und Physiologie, 29ter band, 2e folge; 9ter band, p. 226. Georg Reimer, Berlin.

The eruption rapidly spread and extended to the buttocks, while on each thigh it occupied an area larger than the palm of the hand. At one visit, within the spreading edge the deeply pigmented skin was observed to be the seat of a great number of beautiful little circles of eruption, identical with those observed in ringworm of the body.

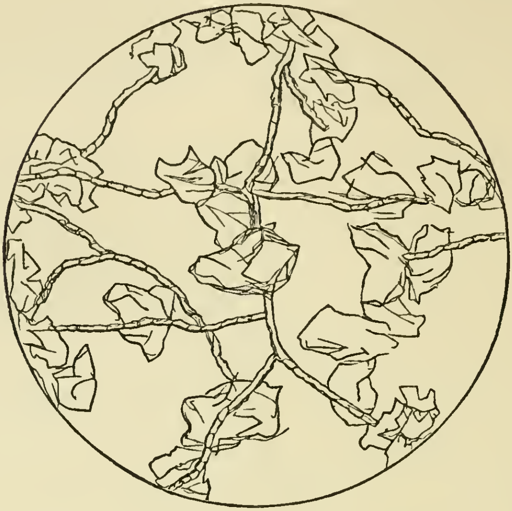
It is right to state, that this patient had a few months previous returned from a journey to India, where this complaint is said to be common; and he stated that he may have had a slight irritation of the parts when there, although it escaped his observation. He informed me, too, that he paid monthly visits to a friend in England, who had also recently returned from India, and that he knew that he had the same complaint. At these times he used the same bath and the same bath-towels as his friend.

The treatment of this case was almost exclusively local, and consisted at first of a lotion of liquor carbonis detergens, until my suspicion of its being parasitic was verified by the microscope, after which he had lotions of bichloride of mercury (gr.ii. to the ℥i.) of pure sulphurous acid and of hyposulphite of soda (ʒi. to ℥i.). Of these there can be no doubt that the corrosive sublimate lotion was far the most effectual, and it was continued for about eight weeks before it was considered safe to stop it. Finally, as it produced some irritation of the skin, a little zinc ointment was used to allay this.

During the treatment, I accidentally discovered what promises to be the most perfect black dye for the hair which has yet been seen. After having used the bichloride lotion for some weeks, I changed it for the lotion of hyposulphite of soda, and the morning after the first application the hair of the part, which before was bright red, had become nearly black. One or two more applications rendered it jet black, while neither the skin nor the clothing were stained. I saw this patient a couple of weeks later, and there was not the least deterioration of colour, although of course, as the hair grows, the new portions will possess the normal tint. He was by occupation a turkey-red dyer, and was much interested in the discovery, though rather grieved to find, what medically must be considered one of its greatest advantages, that it did not dye the linen, and was therefore unavailable for his purposes.

About ten days after this patient's first visit (on December 22, 1867), a friend of his, a young gentleman, æt. about 26, came to me saying he feared that he had the same complaint; and sure enough, on examination, I found a round patch of eruption on the inside of each thigh where the scrotum is in contact with these parts. They were red, slightly infiltrated, dry and scaly, and the edges were elevated. A microscopic examination of the scales showed that they were loaded with the spores and tubes of a fungous growth, as delineated in the accompanying woodcut (Fig. 2), which, like

Fig. 1.



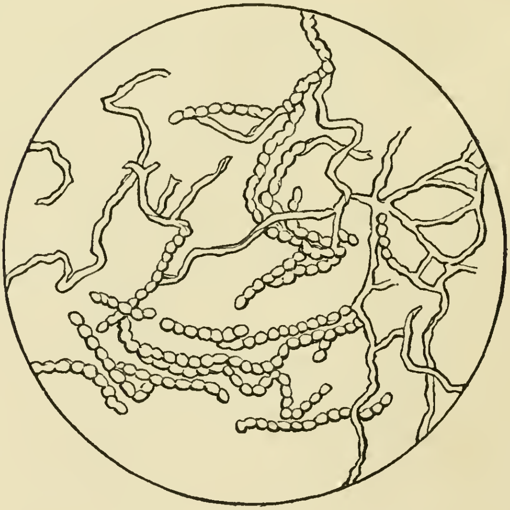


Fig. 2.

the last was drawn by my friend Dr John Wilson with the aid of the camera lucida.

After a few days' treatment the eruption had disappeared from the centre of each patch, but the disease was extending circumferentially. The following ointment was used twice daily.

℞ Olei rusci.
Glycerini amyli, a.a. ʒii.
Ung. hydr. ammoniati, ʒvi.

In about three weeks, thinking that the eruption was quite cured, he stopped all treatment, and I saw nothing more of him until the 14th of February, when he returned, the disease having reappeared in the same form as before, evidently from the application being stopped too soon. I now gave him a lotion of corrosive sublimate (gr. ii. to the ℥i.), which in two or three weeks completely removed the affection, and he has since continued well.

ARTICLE VII.—*Preliminary Education, or the General Culture required by the Student of Medicine.*

A Discourse delivered before the Harveian Society of Edinburgh on the 13th of April 1868, by ALEXANDER WOOD, M.D., F.R.C.P.; Chancellor's Assessor in the University Court, Edinburgh; Member of the General Council of Medical Education and Registration of the United Kingdom; Honorary Member of the College of Preceptors; and formerly President of the Royal College of Physicians of Edinburgh.

THE great man whose memory we meet to-day to celebrate left to his professional brethren this valuable advice, "Study and search out the secrets of nature by way of experiment, and, for the honour of the profession, continue mutually in love."

Aware how much social gatherings tend to remove the asperities which conflicting interests or differences of opinion too often generate, besides endowing the College of Physicians of London handsomely, in the fond hope of enabling its Fellows "to study and search out the secrets of nature," he bequeathed to it what was in those days considered a sum amply sufficient for the purpose, "that they might have a meeting and a collation once a month."

Tread we then in his steps, and as on this the evening of his natal day we encircle the festive board, and celebrate the circulation of the blood by the moderate circulation of the wine-cup, the emblem of union and harmony and peace, let it be the pledge that "all bitterness, and wrath, and anger, and clamour, and evil-speaking, shall be put away from us," not only here, but wherever we may be, and that, "for the honour of our profession, we shall continue mutually in love."

Nor let us neglect the other portion of this valuable advice; let us "study and search out the secrets of nature by way of experiment;" let us each in his own sphere, and according to his own opportunities, assist in building up the great temple of truth, assured of this, that there is no work which does not bring its own reward, and no labour, however humble, if carried out in a reverent and loving spirit, which will not do something to advance the great cause, the furtherance of which every votary of science professes to seek.

Startling as the discovery of Harvey must have been to his contemporaries, and calculated as it undoubtedly was to revolutionize all the physiological science of his day, we need only study the products of the great minds immediately preceding and contemporary with him, to see how for his, as for all other great discoveries, the minds of men had been gradually educating; that Sylvius, for example, had pointed out the existence of the valves of the veins, while Fabricius of Aquapendente (Harvey's teacher at Padua) had shown that they are all turned towards the heart; reserving for Harvey only the glory of demonstrating that the blood moves in a different direction in the arteries and in the veins, and thus of being the discoverer of its circulation.

Let us for a moment consider what were the mental qualities which such a discovery implied, and from the record of Harvey's life endeavour to ascertain how they were acquired. In the first place, he must have possessed distinct and clear views of physical science, of the laws of hydrostatics, of the motions of fluids circulating in ramifying tubes, of the effect of the opening and closing of valves; in the second place, he must not only have carefully observed and noted the actual phenomena of the circulation as it is carried on in our own bodies, and in those of other living animals, but he must have performed many experiments in order to discover the truth which he afterwards promulgated. That this *was* his method, it is not left for us to conjecture, for in the preface to another of his works he tells us:—"In every science, be it what it will, a diligent observation is requisite, and sense itself must be frequently consulted. We must not rely upon other men's experience, but our own, without which no man is a proper disciple of any part of natural knowledge."

But mere observation and mere experiment could not alone have evolved the great truth which has immortalized the name of Harvey. An inductive philosopher, as all true philosophers must be, he pursued the method of his illustrious friend and patient Bacon before it had been promulgated; and possessing the powers of observation which enabled him to note and register his facts, and the power of invention which enabled him to contrive and perform his experiments, his was also a mind capable of generalization, which enabled him to evolve from those facts and those experiments the true theory of the circulation.

One other quality of the philosophic mind he also possessed in large measure. In him the philosophy of nature had not extinguished its poetry; and while minutely examining the fearful and wonderful structure of the body in which he dwelt, and, interpreting its mysteries, he was led, as many another has been to look from nature up to nature's God; or rather, beginning and ending all his scientific pursuits with humble faith and reverent adoration of the Almighty, he arrived by the argument from design at his great discovery. "I remember," says Boyle, "that when I asked our famous Harvey what were the things that induced him to think of a circulation of the blood, he answered me, that when he took notice that the valves in the veins of so many parts of the body were so placed that they gave a free passage to the blood towards the heart, but opposed the passage of the venal blood the contrary way, he was incited to imagine that so provident a cause as nature had not placed so many valves without design, and no design seemed more probable than that the blood should be sent through the arteries, and return through the veins whose valves did not oppose its course that way."

Distinct knowledge of laws, careful observation of facts, patient trial of experiments, the union of these in a mind capable of generalization, and a humble and living faith in his great Creator, were the qualities which enabled Harvey to realize his grand conception. Is this genius? Not, if by genius we mean, as some do, an original gift which almost without training or cultivation enables men to arrive at great results. But if by genius we mean that state of mind in which its various powers are so altered as to become with due cultivation and exercise most powerful, and yet most harmonious, then was his genius of the highest order.

He was a ripe and finished scholar; educated in classical knowledge at the Grammar School of Canterbury, he perfected his acquaintance with ancient lore, and studied philosophy at Cambridge; then, at the age of nineteen, he commenced his medical studies at Padua. He subsequently returned to his native country, became physician to St Bartholomew's Hospital, and in succession to James I. and Charles I., the latter of whom he accompanied twice to Scotland. Of the advantages he thus enjoyed he thoroughly availed himself, and we are not surprised to find that so accomplished and learned a physician associated on equal terms with Bacon and Hobbes, Dryden, Cowley, and Boyle, and the other distinguished philosophers and poets, who were the ornaments of that brilliant court with which he was so long connected.

And what the study of nature and of scientific medicine, as founded on that study, required of her votaries in his day, she still imperatively demands in ours.

By much previous training, by much earnest study, by much laborious preparation, can the mind alone be disciplined fittingly and reverently to search into the arcana of nature. How shall this fitting state of the mind be best produced? or, in other words, In what does true education consist? is becoming more and more agitated in our day; and I cannot think it a task unbefitting the commemoration of the memory of one who was so distinguished an example of what all true physicians should seek to be, if I devote a little time to consider the objects and aims of the preliminary education of the physician, how it should be directed, and what it should embrace.

"Perhaps there is no science," says Professor Vogel, "which requires so penetrating an intellect, so much talent and genius, so much acuteness and memory, as the science of Medicine. For the full attainment of its ultimate and proper object, it requires also indispensably the possession of stability of judgment, rapidity of decision, and immoveable firmness and presence of mind." The studies which the range of professional inquiry embraces are also varied and intricate, and to be properly mastered require that the mind shall be duly disciplined for their reception. It is for this object that a liberal education is so essential a preliminary to medical study.

If there be any truth in this, can we wonder at the ignorance of many of our practitioners, and the unphilosophical and pernicious character of much of the practice of the art, when the state of mind of those who enter upon its study is considered. To comprehend the very earliest subjects which his professional course enjoins, the student must bring to their study a disciplined mind. Intelligently to profit by the instructions of his teacher, he must see for himself with philosophic eye; he must discriminate truth from error,—a process which requires a clear judgment and an ability to reason; he must understand the principles of practical logic, and be trained in the use of the inductive philosophy. Without this he cannot intelligently master the most simple and elementary of his professional studies, and the pursuing of them without these qualifications will not only be a waste of time, but will have the effect of rendering him a careless and inefficient student, and of preventing him from profiting by the teaching even of the ablest instructor.

Contrast with what I have shown to be a necessary pre-requisite to the philosophic study of Medicine, the actual mental state in which the great mass of students enter on their professional studies. Their minds are not only generally unfurnished, but often actually unformed. They are utterly unable to group together or arrange the facts with which their teachers supply them: they have never been trained to follow a process of reasoning, to comprehend analogies, to compare the like and contrast the unlike in different bodies, and so to name, sort, and classify the materials of which their sensible and conscious experience is composed. If, then, they have never been taught the uses of abstraction and generalization, if they have never exercised their judgments so as to ascertain the mutual relations of things, then their reasoning powers must have lain dormant, and in this state they are expected to become fitting investigators of philosophic truth!! Not only is a mind in such a state utterly unable to grapple with the subjects that will be presented to it, but the development of the intellectual faculties will be arrested, and the whole growth of the mind stunted, and its vigour weakened by the attempt. To bring to the task of mastering science minds undisciplined in that knowledge which is the foundation of all sciences, is like attempting to gain information by travelling in a foreign land, with the language, the currency, and the mode of life of which we are utterly unacquainted.

The Medical Council has done great service by compelling an examination in what is called preliminary education to be undergone before medical studies are entered upon; but they must go a great deal further: they must prescribe more clearly what these studies are to be, and they must exercise the same control over, and take the same care to improve the examination in them, as they have done in regard to those examinations which ascertain the amount of professional acquirement.

There are two theories of education which have found supporters at the present day, and very much according as one or the other is adopted are the views taken of what education should be. The one regards education exclusively as a means for disciplining the understanding; cares little for the subject; attaches great importance to the form. The knowledge is nothing; the mental exercise spent in acquiring it is everything.

The opposite theory we shall perhaps best state in the language of Mr Lowe. "And first," said this gentleman in his Edinburgh address, "I will endeavour to explain to you what I conceive to be the business of education. It seems to me, if one can form an abstract idea of what ought to be taught, that it is to teach a person everything important to know, and at the same time to discipline his mind."[1] This idea is the true parent of the cramming system, which, fully developed at the competitive examinations for Indian and other civil appointments, is extending to all others, poisoning the very appetite for knowledge, and rendering its acquisition too often a mockery, a delusion, and a snare. Walking last summer in the West Highlands with a young friend who had just honourably passed the competitive examination for the Indian Civil Service, and who had received high marks in geology, I ventured to ask him some question about the remarkable geological district which we were traversing; he replied, "Oh, I know nothing about it, and never did; all I got up for the examination was a cram, and I forgot the very terms before a week was over." If Mr Lowe's idea of education is that it should fill the head of the pupil with as much as it is necessary or possible for him to know, we are not surprised to find that he is chiefly guilty for the style of these examinations. Mr Lowe was secretary to the India Board at the time the writer-

[1] Primary and Classical Education, page 13.

ships were thrown open to public competition, and he frankly tells us the system on which the examinations were framed,—a system in which he glories, on which he has conferred the imposing name of "the science of ponderation." "We took everything we could think of that a well-educated man could learn. We took all the languages, we took Latin and Greek, we took French and English, and all the modern languages of Europe; we took the principal branches of physical science; we took history, English literature, philosophy of mind as taught in Scotland, and at Oxford, and at other places; we took everything, and we gave marks to each according to their relative importance, as near as we could arrive to it; and under that system all persons have been admitted equally and fairly to the benefits of those offices, whatever their line of study may have been."[1] Mr Lowe is fond of judging by results, but the system he thus inaugurated has not been long enough on trial to enable us to do this. Certainly what we hear from those who have been longest acquainted with our Indian Empire does not lead us to believe that the "Competition Wallahs" are regarded as a superior race, or that their polyglot, or rather their encyclopædic acquirements command the reverence of the acute Parsee. It is not long since, that in a meeting of natives, it was actually debated whether an Englishman should any longer be saluted by the title "sahib" (sir). I cannot help thinking that the whole idea of education which regards it as a means of imparting knowledge is a mistake; that, as Coleridge long ago showed us, "as the forms of all organic existence, so must all true and living knowledge proceed from within; it may be trained, supported, fed, excited, but can never be infused or impressed." The same idea is more figuratively and beautifully but not less strongly expressed by Jean Paul Richter, who says, "The child walks hand in hand with a nature which never returns; this nature without education is a wintry desert, full, however, of spring buds, wherever a sunbeam strikes it (for all teaching is warming into life rather than sowing); there the green leaves burst forth and the whole child's life consists of warm creation days."

The great object of preliminary as opposed to professional education appears to be to cultivate rather than to inform the mind, to train men to think for themselves rather than to be the depositaries of the thoughts of others. It was in this that the excellence of the Platonic method, as opposed to the Sophistical, consisted; refusing to treat the mind as a mere passive recipient of various knowledge, Plato regarded it as possessing a germinal power which craved only for the knowledge which it could appropriate.

It may be difficult in an utilitarian age to maintain such a doctrine, but he will be the really successful educator who, having regard to the etymology of the word, uses instruction as a means of drawing out, of developing the mental powers of the pupils; seeks not to make a display of present acquirements, but rather to strengthen those forces which will enable his pupils to discover and classify for themselves, and to illustrate and expand for others. You must train, and prune, and invigorate the vine in early spring, and look for the fruit in the distant autumn, instead of forcing precocious grapes. "Behold, the husbandman waiteth for the precious fruit of the earth, and hath long patience for it, until he receive the early and latter rain. Be ye also patient."

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[1] Address, p. 30.

This doctrine has the high authority of John Stuart Mill, who says, in his Rectorial Address at St Andrews, "What professional men should carry away with them from an university is not professional knowledge, but that which should direct the use of their professional knowledge, and bring the light of general culture to illuminate the technicalities of a special pursuit."—(Page 5.)

One general idea, in short, is this, that by education we should seek to confer wisdom rather than knowledge, not to make the mind a dungeon, where all sorts of acquisitions lie huddled together in unavailable confusion, but rather to give it the power of methodizing, arranging, and rendering available its various contents. The poet wisely observes,—

"Knowledge and wisdom, far from being one,
Have oftimes no connexion. Knowledge dwells
In heads replete with thoughts of other men;
Wisdom, in minds attentive to their own.
Knowledge, a rude, unprofitable mass,
The mere materials with which wisdom builds,
Till smoothed, and squared, and fitted to its place,
Does but encumber whom it seems to enrich."

To avoid all ambiguity, I would state again, that the objects of general education, or that which is intended to confer culture, as distinguished from special education, or that which is intended to train for special employments and trades is, first, to train and develop all the faculties of the mind; secondly, to communicate that kind of information which forms as it were the common ground of literature and science, and the possession of which will smooth the way to further progress in these departments.

I am opposed altogether to the views enunciated by Mr Lowe.[1] I would rather see as the result of education the human mind made an instrument of sound and enlightened power, than an encyclopædia of all sorts of useful knowledge. The latter seems the aim of Mr Lowe's science of ponderation; and as a practical educator has well remarked, "I have little faith in a science of education the four fundamental principles of which severally condemn grammar, poetry, logic, and history to a subordinate place in her curriculum; and which, when their united light is concentrated into a focus, reveal to us the study of the art of making shoes as standing 'on a higher pedestal than the study of the Paradise Lost.'"

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[1] Mr Lowe's Educational Theories examined by Hely Hutchison Almond, M.A., page 6.

The various branches of knowledge which experience has taught us to employ as educators, may be classed under four heads :—

1. Languages.
2. Mathematics, including Geometry, Arithmetic, Algebra.
3. Natural Science, whether exact or experimental.
4. Mental Science.

The first of these—Language—has long been considered as the most efficient instrument of education ; and I am glad to find that the Schools Inquiry Commission, in their Report published the other day, endorse this opinion, which they state to be almost universally held both by parents and teachers.

The study of Language presents a twofold aspect.[1]

Firstly, The structure of the language, if highly organized, may recommend its grammatical study as a formal instrument of intellectual training of minds, that may not necessarily sympathize with the wisdom, thought, and experience which the writer may embody.

Secondly, The wisdom of the philosopher, the beauty of the poet, the succinctness of the historian, the eloquence of the orator, the wit of the satirist, will refine and elevate the taste of the student, and open to him a new world of beauty, conferring on him at the same time the pleasure which intellectual culture bestows.

In the former, the mind is much more powerfully exercised than in the latter, " while accurate scholarship builds up and strengthens the active faculties of the understanding, the study and enjoyment of literature awakens, expands, and educates the intellectual and moral sympathies."[2]

But this strong and general conviction on the mind of teachers as to the supreme power of language, and especially of the Latin language, as a means of intellectual exercise, may after all be a prejudice. The training which they employ with others is the same which they received themselves ; and, perhaps, it may be supposed that they have never thought of questioning the intrinsic value of that which has been so long accepted as the main part of all educational machinery.

Let us sift the matter to the bottom ; and as the question is now one of paramount importance, I make no apology for considering it at some length. Perhaps the best mode of procedure will be to consider, in the first place, the various faculties of the mind, for the educing and forming of which education is required, and then we shall be the better prepared to consider the real value of our educational instruments as means of operating on these faculties.

In the Table No. I, I have placed before you a concise summary of the intellectual powers of the mind, of the most elementary kind, and avoiding as far as possible all metaphysical ambiguities. To it let me direct attention with a view of considering the nature of these powers and the effect of education in developing them.

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[1] See this subject discussed at length and with great power and eloquence in Professor Sellar's Theories of Classical Teaching.

[2] Sellar, *op. cit.* p. 24.

TABLE I.—*Intellectual Faculties of the Mind,*

I. PRIMARY.

A. Apprehension—expressed by "a term" derived from
B. Sensation, or Perception, or Reflection, ... } informed by the senses or consciousness.
C. Memory—for retaining and recalling.
D. Judgment—expressed by "a proposition."
E. Reasoning— „ "an argument."

II. SUBORDINATE.

A. Attention.
B. Conception.
C. Association.
D. Abstraction and Generalization.

III. CREATIVE.

A. Imagination.
B. Taste.
C. Genius.

The mind can only operate on facts supplied from one or other of two sources,—the world of matter outside, in regard to which it is informed by the external senses; and the world of spirit within, the operations of which are revealed by consciousness. The first part of education which nature herself undertakes in the infant, is the education of the senses to quickness and accuracy of perception by the objects around us; and the senses are also specially addressed in the most rudimentary form of scholastic education which deals first with words, either written or oral, as the representation of things.

The seizing of the external object by the sense is called Apprehension, while the mental faculty by which it is appropriated is called Sensation or Perception, which accordingly we find at the head of the Primary intellectual faculties in the Table. It is my object to show how every one of the Mental Faculties is exercised in the study of language. You must therefore note, that when a language is studied orally, the sounds which compose the words are apprehended by the ear, and become the objects of Sensation or Perception in the mind; that if the language be written, the only difference is that the eye rather than the ear is the sense which apprehends it. If the language be studied partly orally and partly graphically, then both ear and eye are exercised.

The next mental power in the table is Memory, and it is scarcely necessary to show how constantly it is exercised in the grammatical study of a language. When the pupil becomes sufficiently familiar with the word or the sound as to recognise it again when presented to him, the memory has been called into play, although its further development requires that sensations shall be awakened by past sensations without the recurrence of the sensible object. Memory gives stability, permanence, and effect to the acquisition of knowledge, and has been well called "the cement of the edifice of truth." It is the storehouse of those experiences which guide us in our conduct, and of those ideas which reflection converts into knowledge. It is an important truth in regard to the exercise of memory, that the vividness and capability of being retained, of the ideas which it has received, will be in proportion to the amount of Attention exercised in acquiring them. "Nec dubium est," says Quintillian, "quin plurimum in hac parte valeat mentis intentio, et velut acies luminum a prospectu verum intuetur non aversa."

This introduces us to another, though subordinate, power of the mind—Attention—one which especially requires and benefits by diligent cultivation.

Recurring to the example of the child, we find that the development of memory is followed by that of another power. He soon begins to compare one object with another; the taste of salt and sugar at first undistinguishable by him, soon cease to be alike, and a marked preference is shown for one over the other. He will instinctively turn to the most striking of two objects,—the brightest light, the loudest sound, will attract his attention. At a further advanced age he will measure by the eye and correct the sight by the touch, the sizes of objects, or will compare the taste and smell of different bodies. This is the commencement of the exercise of Judgment, one of the most important faculties of the mind. Closely allied with Judgment is Reasoning, that mental power or combination of powers by which the results of judgment are compared, conclusions drawn, and truth arrived at. Whately thus clearly explains the operation of this faculty,—"There are three operations of mind in argument: Apprehension, that is the notion in the mind analogous to perception by the senses; Judgment, the comparing of two notions that are the objects of apprehension; and Reasoning, the proceeding from one judgment to another founded thereon, reproducing the result. The expression of the apprehension is a Term; the expression of the act of judgment is a Proposition; the expression of an act of reasoning is an Argument.

We have thus passed to certain qualities of the mind of a higher order, by which man is distinguished from the lower animals, and which constitute him a Reasonable Being.

The inferior animals have Sensation and Memory, but are devoid of reason and judgment. Therefore it is that language, and its representation by characters, which are in fact the commutation of our perceptions for a significant sound or word communicable to others, is comprehensible by man alone of all animals—"Words are the floating currency of the mind, the efficient materials for the perspicuous expression of thought."

If the truth of the preceding be admitted, you will estimate at its true value Mr Lowe's sneer at the study of words instead of things in modern education. As well might he reclaim against the use of the symbols in Algebra, or the figures in Geometry in the teaching of these sciences. Surely when he penned his unfortunate contrast between "things" and "words," deliberately giving his preference to the former over the latter,[1] his eyes must have blinked or been blinded for the time by the flood of light which Philology, so largely developed in our day, is throwing on the past, connecting the present with it by lineal descent, and in its great modern development doing much, and promising to do still more, to solve many of the obscurest problems of the origin and early history of our race.

There are two subordinate faculties, on the operation of which Judgment is dependent—Abstraction and Generalization. Abstraction designates the process by which, in contemplating any object, our thoughts are directed to some one part or property exclusively. Generalization indicates the process by which the mind occupies itself with like parts or properties in different objects; and in consequence of their possessing these like parts or properties, includes them in one genus or kind. If the Judgment be that faculty which compares objects so as to ascertain their mutual relations, abstraction and generalization are the powers which it employs to carry out its operations; for by them is performed the training, sorting, and classifying of all the materials of which sensible and conscious experience is composed.

If there be any truth in the preceding remarks, you will readily see what an important place Grammar must hold as a means of educating the mental powers.

The first of our mental faculties which awakes is Perception, which enables us to note some existences external to our senses, and the first part of speech is the Noun Substantive, expressive of substantive or independent existence. Nouns are, in fact, as Vossius tells us, either perceptions of substances—Substantives; or perceptions of attributes,—adjectives.

In Table II., taken from Stoddart, the various parts of speech are exhibited, showing how the logical faculties of the mind are necessarily trained by their study.

TABLE II.—*Classification of Words.*

WORDS
- I. Used in enunciative sentences.
  - A. Principal Words.
    - *a.* The Noun, or name of a conception.
      - Primarily,
        - α. If expressive of substance (Substantive).
        - β. If expressive of quality,
          - Without action (Adjective).
          - With action (Participle).
      - Secondarily (the Pronoun).
    - *b.* The Verb, or expression of an assertion.
  - B. Accessory Words.
    - *a.* The Article defines extent of conception.
    - *b.* The Preposition, relation of one substantive to another.
    - *c.* The Conjunction, connecting one assertion with another.
    - *d.* The Adverb, modifying conception or assertion.
- II. Used in passionate sentences or as expression of Passion (the Interjection).

From Sir J. Stoddart.

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[1] "I think it will be admitted by all who hear me, that, as we live in a universe of things and not of words, the knowledge of things is more important to us than the knowledge of words."—Lowe's Address, page 14.

In the following Table (III.), constructed from the same author, the nouns are exhibited according to the alterations which they undergo in actual use, in changes in gender, number, and case, usually expressed in the ancient languages by alterations of the terminal syllable, often expressed in modern languages by the introduction of particles.

Table III.—*Differences of the Noun in Language*—i. e., *of the term in a Syllogism.*

Essential	In Kind,	Corporeal. Mental.
	In Gradation,	Genera. Species. Individuals.
Accidental	In Number,	Singular. Dual. Plural.
	In Relation,	To other Nouns. To Verbs.

1. *From their differences in kind* we learn to distinguish between the sensations produced by ideas from without or from within.

2. *From these differences in gradation* we learn to divide them by abstraction and generalization into Genera, Species, and Individuals.

3. *From their differences in number* we learn to reverse the foregoing process, and do analytically what in gradation we had done synthetically, resolving genera and species into the individual units of which they are composed.

This distribution of the Noun into genera, species, and individuals, is at the root of Logic; and from a study of it we see how the forms of language correspond with the universal forms of reasoning. In inductive reasoning we ascend from particulars to generals, from individuals, through species, to genera. In syllogistic reasoning we proceed from generals to particulars. The keystone of the whole art is contained in the simple but comprehensive dictum of Aristotle, the *dictum de omni et nullo* as it has been called, that whatever can be predicated (affirmed or denied) of a class universally can be predicated of anything contained in that class. The genus is an idea including the species, and the species is an idea including the individual. The two first classes are expressed in grammar by common nouns, while the individual is either expressed by a proper name or by a common name, individualized by the aid of an article or pronoun.

Time will not permit me to analyze in the same way Gender and Case in Nouns, or to show the relation between the fundamental laws of thought and the adjectives with their degrees of comparison. Let me only instance the Verb, that part of speech which first affirms conscious existence either in action or passion,—to be—to do—to suffer.

If this consciousness of simple existence be, as many maintain, the source and necessary condition of all other powers, you will see how the Verb To Be is at the root of all Mental Philosophy. To Be implies conscious existence; To Do introduces us to the active forms of the Will, which chooses or refuses, accepts or rejects—the manifestations of which are the dispositions, affections, and passions; To Suffer indicates the state in which the mind is supposed to be the passive recipient of external impressions, which refers us back to sensation and those avenues by which external things and their qualities reach our mind.

What the foregoing instances illustrate holds good throughout the whole of grammatical study, which is at once a science and an art. "If grammar," remarks an authority before quoted,[1] is not a true science, if rules are not the 'media axiomata' which give form and consistency to its principles, if it does not teach order and logical method as well as words and names, it is high time that the system of education which has been based on the scientific and logical study of grammar should be, not reformed, but utterly swept away. The nations of Europe, the great nations of the ancient world are involved, with ourselves, in the same condemnation. The whole intellectual training of civilisation has been founded on a delusion! . . . . I am going to assume that grammar is a science, that its subject-matter is language, that the ordinary generalizations of nominative and verb, of optative moods and conditional clauses, are, in the main, as true to facts as the generalizations of other sciences; that the laws of language which it investigates are not arbitrary creatures of the imagination, but express realities existing in language; and that there are rules deducible from these laws of which it is no nearer the truth to say, as Mr Lowe does, 'that you never know whether they apply or not,' than it is to make the same assertion of the rules of applied botany, or applied geology, or of any other science, except those which are purely mathematical and exact. And I am also going to assume that grammar is not only a science, but is also an art, and that it is because the teaching of an art which we must use ought to go, and, to a certain extent, must go hand in hand with the teaching of a science, that grammar holds a place in our educational course which cannot be occupied by any science whose subject-matter is something external to the human intellect."

Contrast with this opinion of a practical educator that of the right honourable gentleman, the inventor of the ponderation system as applied to knowledge, and of the "result system" as applied to education. Mr Lowe's idea of grammar was thus delivered to his Edinburgh audience:—"If language had been made on a set of general principles—if it had been laid down by the wise men of all generations that the nominative should always agree with the verb, and a verb should always govern the accusative—and language had been made like Euclid, every one of these rules which had been tied we could untie, and a language having been put together in that way, we could analyze it into rules. But, gentlemen, language was not so made. Language grew, we know not how, like a tree or a plant; it was not made under general rules, and therefore when you are trying to form general rules for it, you are sowing the sand; you will never attain what you want," etc. (page 19). It is impossible that Mr Lowe can really be so ignorant as this memorable sentence would make him. He must know something of Comparative Philology; he must have studied general grammar; he must know that there are other sciences besides the exact, and would have been strangely at a loss to have named any one science which had been framed according to his ideal: a congress of learned men of all nations, and almost necessarily of all times, having made general rules, the science was then constructed to fit the rigid requirements of a bony skeleton thus provided. "Language grew, like a tree or a plant," and therefore there can be no scientific rules applicable to it; as if we had no botany, and no vegetable physiology to explain the laws of growth of plants that "grow we know not how;" just as we have rules of grammar not laid down beforehand for grammar to be founded on them, but deduced from the actual existing state of grammar, and founded on the adaptations of that science to the felt necessities of the mental faculties.

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[1] Almond, *op. cit.* p. 9.

But enough has been said to vindicate the use of grammar as one of the first educators of human intellect. Some special grammar must, however, be selected; and we think the nearly universal testimony of all teachers will endorse the opinion of the Schools Inquiry Commission just published. The Commissioners are clear in their preference for Latin, partly on account of the beauty of the language in itself, but mainly for the fulness and precision of its accidence, in which no modern language can rival it.

A veteran professor, who spent a long and laborious life in communicating instruction, has left on record his opinion, "that in the study of language when properly simplified and explained we find for our pupils a constant exercise in practical logic, which brings into play the powers of memory, of judgment, of abstraction, and combination of ideas, and of reflection on the subjects of our own consciousness, which induces habits of quiet and sustained attention, facility in sifting and comparing evidence, and promptitude in deciding; which produces, in short, a general acuteness and activity of the intellectual powers."[1]

But Mr Lowe and his science of "ponderation" come in here and meet us with this observation, which must be answered in passing,—"I think it is a poor and imperfect conception of education that should limit it to the learning of any language whatever; but surely if we are to make language the whole or a part of education, it should be the language we are most concerned with; and I must be permitted to say that, in my science of ponderation, I think English has a prior claim over Latin and Greek."

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[1] Pillans's Objects and Method of Education, p. 35.

Mr Pillans has dealt with this argument, which, of course, comes to the surface on all those periodical occasions when an anti-classical fever becomes epidemic. His opinion is that, "with English authors, that mental culture which it is the object of the grammatical study of a language to confer, could not be given at all." I have not time to quote at length all his arguments, a sentence or two must suffice:—

"The ancient are languages of flexion and conjugation, expressing the relation of things to one another, and the variations of the verb in time, person, number, mood, and voice, by changes in the termination of the words, all or nearly all of which we (English) express by separate small particles and monosyllables, which, to prevent ambiguity and confusion, have their places fixed, and must stand in juxtaposition to the words they are intended to affect. Hence, two results: one, that our English sentences admit of very slight and rare deviations from a precise definite arrangement of words; and the other, that modern and more especially English composition is necessarily overrun with monosyllables, most of which, in our language at least, terminate in consonants. The ancient languages, on the contrary, from the circumstances of their incorporating the expression of various relations among objects and ideas into the words themselves, derive two advantages,—first, by avoiding a crowd of such little words as encumber our diction they acquire a pomp, sonorousness, and condensation of meaning, 'a long resounding march and energy divine,' which we cannot look for in our modern dialects; and, secondly, they admit a variety in the collocation of words, and a freedom of transposition which materially contributes, in the hands of an accomplished writer, both to mould his periods into the most perfect music and melody to the ear, and, what is of more consequence still, to present them in the most striking forms to the understanding and imagination of the reader."[1]

But the study of the classics has a twofold operation. We are made familiar both with the meaning and the expression of the ancient authors. The study of their meaning is, as has been shown, an intellectual exercise of the greatest value. Acquaintance with the way in which they expressed their thoughts is important for training the taste, by the use of models which are universally appealed to as a standard measure, if not of perfection at least of excellence. Even if, in pursuing classical studies, the student should never acquire the power of reading ancient authors with ease, yet the time devoted to the pursuit cannot be considered as absolutely lost. If a student has been enabled, even with pain and difficulty, to obtain a full perception of the author's meaning, he has been subjected to a most valuable intellectual exercise. If he has acquired the ability to express that meaning in another language, he has been put through a most useful and necessary practical discipline. And even the most utilitarian educator will be forced to admit, that a knowledge of English will be most easily and most correctly acquired from a knowledge of Latin.

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[1] *Op. cit.* p. 57, *et seq.*

The Schools Inquiry Commissioners remark that "nothing teaches English grammar so easily and so well as Latin grammar." Professor Blackie characteristically observes,—"The ancient languages are certainly far away from us in point of time, so is that of Kamtschatka in point of space; but the distance has nothing to do with the matter. The decisive point is, where does the vital connexion lie? The vital connexion of a tree is with its root, and the root of our modern English culture lies much more in Greece or Rome than in Berlin or Paris. In this way Greek and Latin are more to us, and more necessary than French and German."[1]

If the ordinary Englishman can scarcely understand his own language without a knowledge of the classics, what are we to say of those who would discard them as necessary requirements from those who aspire to be members of a learned profession, or argue, as some have not hesitated to do, that a knowledge of them is an unnecessary acquirement for scientific men? I am far from saying that many modern sciences, chemistry, botany, and the like, may not be studied profitably without a knowledge of the classics. One thing however is certain, they will require, as far as scientific terms and scientific conceptions are concerned, to be studied much more painfully; and their professor, however conversant with his subject, will perpetually run the risk of offending the ears of educated men by false quantities and other mispronunciations.

Those of us who have acted on boards for examining entrants to our profession, are well aware of the ignorance in this respect of many of our candidates, notwithstanding that we profess to exact a sufficient preliminary examination. I remember being annoyed long by one flippant student, who at length, when I asked him a question about Angīna pectŏris, repeated the words after me, Angĭna pectōris, as if to show his superior knowledge to that of his examiner. Tortured beyond further endurance, I exclaimed, "Oh! do learn to pronounce your words with some regard to their derivation." "I assure you, Sir," said he complacently, maintaining his assumed superiority, "we always pronounce them that way in London." This was, of course, as he intended it should be—unanswerable.

If, however, we regard education from Mr Lowe's point of view, and propose as its end the acquisition of a kind of knowledge which shall be immediately convertible into coin, the god chiefly worshipped in this money-making age, then am I free to confess that a full and philosophical study of language, and of grammar as a stepping stone to it, would be time misspent; if, however, we are to judge of the effect of education by the extent to which the mind has been exercised and developed, by the powers which have been strengthened for contests in the battle of life, by the amount of thought which such training has called into active operation, you will admit that the *literæ humaniores* must take the foremost place as educers of the intellect.

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[1] On Education, p. 37.

Add to this what Mr Mill has so well stated, not only in his Rectorial Address, but also in his earlier writings. One sentence only from the former I will quote:—"In cultivating, therefore, the ancient languages as our best literary education, we are all the while laying an admirable foundation for ethical and philosophical culture. In purely literary excellence—in perfection of form—the pre-eminence of the ancients is not disputed. In every department which they attempted, and they attempted almost all, their composition, like their sculpture, has been to the greatest modern artists an example to be looked up to with hopeless admiration, but of inappreciable value as a light on high guiding their own endeavours. In prose and in poetry, in epic, lyric, and dramatic, as in historical, philosophical, and oratorical art, the pinnacle on which they stand is equally eminent."[1]

It does appear to me, that however you may expel the classics from the education of the mere tradesman, shopkeeper, or lower rank of merchant, who does not aspire to associate with educated men, you cannot dispense with them in the case of any who purpose to enter one of the learned professions or to exercise an influence on or mould the opinions of his age. "Where can we acquire such lessons in the first principles of politics and social science as from a study of the causes which led to the rise and fall of the ancient commonwealths, and a familiarity with the philosophical system of law framed by the Romans? Where can the inquiring intellect learn such lessons in the investigation of truth as are taught both by precept and example in the dialectics of the ancients? Who would think of studying philosophy without sitting at the feet of Plato and Aristotle? or who aspire to a cultivated taste that has not been formed on the models of ancient Greece and Rome, for all our great philosophers, poets, and orators confess to having been nourished with the spirit of antiquity?"

> "Hither as to their fountain other stars
> Repairing, in their golden urns draw light."

While I thus insist on the necessity of the Latin language as the best fitted to train and draw out the mental powers, I am free to admit that the mode in which it is taught in many schools, especially in England, is susceptible of great improvement, and that it never ought to be made the *sole* means of education.

I would of course expect that no one would commence his medical studies without a thorough knowledge of his own language, and without proving his competence to write it grammatically. The lamentable instances of defective grammar and defective spelling which come before our Examining Boards, when testing the professional knowledge of the candidate, abundantly show the low share of mental culture which is supposed to be sufficient to qualify a man to pursue one of the most philosophical sciences and to practise one of the most difficult arts. A thorough knowledge of English should be required, and a failure in this should exclude the candidate from enrolling himself as a student of Medicine.

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[1] Rectorial Address, p. 17.

The Medical Council, after a keen debate, and by a narrow majority, have determined to make a knowledge of Greek essential before any student can commence his professional studies. I doubt the wisdom of this. With all my anxiety to see a high standard of general education, I would rather see less demanded and its possession more thoroughly tested. For the higher grades of the profession, for the Doctor of Medicine, who conjoins the testamur of the university to his scholarship as well as to his professional skill, a knowledge of Greek is indispensable. Wherever high mental culture is prized, the language and literature of Greece will ever hold a pre-eminent place. But to demand a knowledge of that language from every village surgeon, every "rough and ready practitioner," is a thing which will not be obtained, and will therefore be a mockery and a delusion. If I can read the signs of the times, while the study of Latin will be retained and even extended in our schools, that of Greek will become less general,—a prediction which seems borne out by the Report of the Schools Inquiry Commission :—"It seems to be generally agreed that, except for education of the first grade, Greek cannot be usefully taught, and even in that grade it seems to be the prevailing opinion that Greek should not be considered absolutely essential."

While thus advocating the necessity for the thorough mastering of the Latin language, and vindicating the necessity of efficient instruction in the English language, the claims of other modern languages must not be forgotten. Latin has been well called "the common gateway to French, Italian, and Spanish," and if it has been thoroughly mastered the acquisition of any of these will be easy. As a means of mental training, the French is second only to the Latin grammar. A knowledge of it, too, is of marketable value, which will please the "result school," while a living, being always less likely to be forgotten than a dead language, its acquisition is probably a more permanent possession. Add to this, too, the fact that the French are among the most diligent cultivators of medical science, and that many of their professional treatises are of great value, and we have ample reason for considering a knowledge of the French language as almost indispensable to the student of Medicine.

Of German I will scarcely speak: while the ability to read with ease the many profound and scientific treatises which the country has produced will ever be an acquisition greatly to be prized by any student, I do not suppose that any one would think of requiring its possession as an essential preliminary to entering on classical study, while all would regard its possession as an accomplishment of very great value.

In the early universities the great preliminary branches of knowledge were divided into the *trivium*, containing Grammar, Logic, and Rhetoric, and the *quadrivium*, comprehending Arithmetic, Geometry, Astronomy, Music; the two together constituting the *liberæ artes*.[1] I take now, therefore, from the quadrivium our second class of subjects, Mathematics.

The very name given to this branch of learning, which has no reference to its subject-matter, but only to its supposed effect on the mind, shows the high place which the ancients assigned to it as a trainer of the mind. The Greek *μάθησις* is translated by the Latin Disciplina, and, divorced from its original meaning, which the Latin equivalent word has retained, is now applied to all knowledge regarding space and number, which the ancients seemed to have considered as *the* discipline for enlarging the mind,—a circumstance in itself sufficient to lead us carefully to consider the value of these branches of study as means for this end. Even Mr Lowe forgets his "ponderation system" when speaking of Mathematics, and regards them "as a most admirable study, and calculated to train the mind to strict habits of reasoning, and habits of close and sustained attention," as if the ponderation system took training into account at all, and did not profess to limit itself to considerations of practical utility. His meed of praise is, however, confined to synthetical mathematics, analytical he unequivocally condemns, for the somewhat peculiar reason, that "it approaches the subject analytically, which is not a good way of reasoning." It seems scarcely necessary to defend from such attacks the method which conducted Leverrier and Adams to the discovery of Neptune,—a practical application, by the way, which should not have been forgotten in the "science of ponderation."[2]

Whether we have regard to the precision of the definitions from which it starts, the caution which it exercises in admitting nothing which is not or cannot be made most evidently true, the severe reasoning by which its conclusions are arrived at, the certainty of its results, and the different ways in which they may be verified, the concentration of mind which its pursuit demands, we cannot fail to see how fitted the study of Mathematics is to give correct habits of judgment, to sharpen the powers of investigation, to discipline the reasoning faculty in matters from which self-interest, preconceived opinions, and all other disturbing elements are carefully excluded.

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[1] Huber, The English Universities, translated from the German, by T. Newman, vol. i. p. 4.

[2] See Almond, *op. cit.*, p. 7.

The main branches of mathematical science are Arithmetic, or its operative process of Algebra, and Geometry, springing out of the simple notions of number and space. It is not, however, only as a means of disciplining the mind that these studies are valuable, they form the only gateway through which many of the sciences can be properly approached.

Of the importance of Arithmetic and Algebra as branches of elementary education, there can be no doubt, and the Schools Inquiry Commission record that most of the witnesses "seemed to look upon arithmetic as simply indispensable" (Report, pages 22 and 29). "Both for its utility and its educational power, nothing else can stand in its place. It has not, of course, the breadth which belongs to the study of language. But it has still greater power of exercising the reasoning faculties, and it is the gateway not only to all natural science but to a very large part of men's dealings with each other" (Report, page 29). It is probably for this last or utilitarian reason that the science of ponderation gives it a high place in its list, and that it finds favour even with the cynical Mr Lowe, who says—"I think it more important that a man should be able to work out a sum in arithmetic, than that he should be acquainted with all the abstract principles of Aristotle's logic; and that the moods of a syllogism are not so important as the rule of three, practice, and keeping accounts."[1]

Algebra is more nearly based upon definition than either arithmetic or geometry: the results of its fundamental definitions are deduced from symbols, which are used in a prescribed manner, and imply operations on magnitude. It is a science of operation or process of an abstract character, being utterly apart from any of our ordinary notions. It is a powerful trainer of the mind, and now forms part of the education of every gentleman.

The College of Surgeons of Edinburgh, which seems, of all the licensing bodies in the country, to have been the earliest to awaken to a sense of the necessity of enlarged general education, has for many years past prescribed Mathematics (in its restricted sense) as one of the indispensable branches of study for its license. Perhaps in so doing it had regard as much to the bearing of the study on that of Anatomy and Surgery as to any power which it possesses as an educator. "How can the student compass the rigid detail of descriptive anatomy without a more than vulgar acquaintance with mathematical nomenclature? How can the exquisite contrivances of the skeleton be otherwise than a conundrum to him unless he have prepared himself with a study of the simple mechanical appliances?"[2] Be this as it may, the example thus set is not the less laudable.

The Schools Inquiry Commissioners are of opinion that "no one can doubt the value of geometry as an exercise in severe reasoning. . . . . There were a few indeed who would put mathematics distinctly above all other subjects as a means of education, but on the whole there appeared to be no general desire to push the cultivation of mathematics very far." The Commissioners ascribe this in some degree to the fact, that the teaching of mathematics in English schools is rarely satisfactory: the Scotch burgh schools were found much superior to the average English schools in mathematics, which is ascribed to the practical turn given to their mathematical teaching.[1] On the whole, it may be said, that while there is no difference of opinion as to the value of mathematics as a branch of general education, its right position in the scheme is the great difficulty. There is no doubt that its precise and rigid demonstrations cannot be approached with advantage until the reasoning powers are tolerably developed, which they seldom are when the study of Mathematics is commenced. Professor Airy thinks that the amount of Geometry which the mind of a boy of sixteen or seventeen is capable of comprehending is very small indeed, and Mr Almond, concurring in this, is of opinion "that a great deal of time is often wasted by boys beginning to learn it before their reasoning faculties are sufficiently developed, is the experience of most practical educators."[2]

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[1] Address, page 15.

[2] Sir John Simon's Observations on Medical Education.

Without presuming to dictate on a subject of such difficulty and importance, I would venture to suggest that the study of Mathematics would be greatly facilitated if the student brought to it some previous knowledge of the art of reasoning, and some familiarity with the nature of Assertion, Denial, and Deduction; such a study at an earlier period than it usually takes place would enable pupils to approach every subsequent branch of study, and especially Mathematics, with a more limited and defined idea of the precise meaning and extent of words and phrases than is to be learned in the ordinary use of language. The study both of Arithmetic and Geometry should also, in the earlier lessons at least, be made strictly demonstrative, every assumption should be explicitly stated, and every inference logically deduced. Arithmetic almost universally, and Geometry very generally, are in this country taught by dogmatic rules, of the reason of which the student has not the slightest conception, so that his memory rather than his judgment is exercised, and these studies are thus often made to miss their mark as intellectual factors.

The place which Geometry should occupy in the educational course is of importance, not only on its own account, but in reference to a question much agitated at present—the introduction of some branch of natural science into a proper scheme of primary education.

The study of Natural Science as a branch of Primary Education must be limited by three considerations—

1. The amount of time which can be allowed for it.

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[1] Report, pp. 30, 31.

[2] Almond, *op. cit.*, p. 7.

2. The previous mental acquirements of the pupils.
3. The object which is proposed in teaching it.

If there is any truth in the preceding arguments, it is plain—

1*st*, That those who regard the mental training which it confers as the chief object of education will not allow of the displacement of any of our existing intellectual factors to make room for one the value of which is at least not superior to them.

2*d*, That, as the exact sciences cannot be efficiently taught without a certain knowledge of Mathematics, and as we have shown that Mathematics cannot be studied with success until the mind has attained a certain stage of development, the time when instruction in these sciences can be commenced with advantage is necessarily not very early in the preliminary course.

3*d*, That, as in accordance with what has been already shown, the perceptive faculties are the first to unfold themselves in the mind, those sciences which demand observation should be first cultivated, the pupil being encouraged to acquire the facts in the first instance, the classification, arrangement, and study of the inferences drawn from these facts being left to a later period.

4*th*, It should be distinctly understood that the observation of these facts, and the retaining of them in the memory, is prescribed as an intellectual exercise rather than on account of any information they may give, however valuable that information might prove in after life,—for facts so acquired, unless cemented in the mind by the philosophy of the subject, would be forgotten more surely, more entirely, and with less possibility of recovery than the Greek or Latin languages, the rapid effacing of which from the memory is so feelingly deplored by Mr Lowe.

While some of the more ardent advocates of the change dwell upon the extension of the sciences as a strong argument in its favour, I am free to confess that this has always appeared to me to be one of the chief difficulties. At the stage of mental development with which we have had to do, it must be chiefly the facts and objects of the science, and a very few general principles, which can be taught. But the mass of facts of any one science is now so great as to render it impossible to teach them as part of general education, and we are forced to leave them to be acquired as part of their professional education by those whose professional duties require their mastery.

There is a twofold evil to be guarded against, *first*, the risk of cramming the mind with facts, however valuable as information, which will only overload and choke rather than develope and educate the mental powers; *second*, the risk of merging the general in the professional education; and there could scarcely be a greater evil than that the general education should cease to be such as will train the mind by thorough and systematic study in such branches of learning as will best develope its powers, and form a foundation for, rather than an introduction to, all future professional or special acquirement.

With these cautions, there is no doubt that there are certain powers of the mind which can be better exercised by the study of the physical sciences than by any other means, for

"All natural objects have
An echo in the heart. This flesh doth thrill
And has connexion by some unseen chain
With its original source and kindred substance:
The mighty forest,—the proud tides of ocean,
Sky-cleaving hills, and in the vast of air
The starry constellations, and the Sun,
Parent of life exhaustless—these maintain
With the mysterious and breathing mould
A co-existence and community."

In the study of the natural sciences, an eloquent author has well remarked, "Curiosity is ever excited, attention rivetted, and memory bribed by perpetual novelty, variety, and beauty; the comparing power is ever kept alive by an endless succession of similitudes and contrasts, that now sustain the interest by inducing the pupil to note the like in the different, and the different in the like, and now reawaken the flagging attention by renewed excitement and gratification of the senses; and the reasoning power is finally evolved in order to trace and explain the varying adaptation of means to proximate ends, displayed in instincts which anticipatively rehearse the functions of that family, which, when enlightened by reason, and directed to ultimate ends, becomes human understanding. Thus as the student watches the ascension of nature into mind, he shall learn that up the whole ascent nature is a prophetic hymn, heralding the advent of man, and proclaiming the wisdom and goodness of the Creator."[1]

On the whole, I am inclined to think that the part of the Report of the Schools Inquiry Commission which deals with this important subject is not the least valuable.[2] It shows how the claims of natural science as a branch of primary instruction have been much pressed by high authority; how the evidence of witnesses who were not schoolmasters was favourable to the introduction of some branch of natural science as a part of primary education; how the evidence of the schoolmasters shows great division of opinion on the subject; how there are three great difficulties in introducing natural science as a branch of education, the want of suitable teachers, the want of suitable text-books, the want of suitable rooms and apparatus. The conclusion the Commissioners arrive at is as follows:—"We think it established, that the study of natural science developes better than any other studies the observing faculties, disciplines the intellect by teaching induction as well as deduction, supplies a useful balance to the studies of language and mathematics, and provides much instruction of great value for the occupations of after life." The Report recommends Physical

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[1] Green's Mental Dynamics, p. 4.

[2] Pp. 32, *et seq.*

Geography as the best starting point. This should, in the opinion of the Commissioners, be followed by some branch of science which would not tax more than the powers of simple observation. "But when the pupils reach the age to be taught natural science with scientific precision, the subjects best adapted for teaching above all others are experimental physics and chemistry, inasmuch as they constitute the platform of all the rest."

When we find that the Commissioners recommend this by way of addition, not substitution, to the existing range of education, we remember how forcibly the extent of science contrasts with the brevity of life.

The whole business of the physician in his daily avocation is to discover and ascertain truth. The symptoms which he discovers by observation he either analyzes that he may get them in their purest and most simple form, or he synthetically combines them according to their mutual connexions and affinities. In either case they are made the material on which his reason operates, whether his object be to determine the true nature of the disease, the true state of the internal organ affected, the probable character of the change which it has induced in other organs or tissues, the cause of such change, or how a deviation from the normal state of one organ should be followed by a deviation in another; the precise action of each remedy which he employs, and so on throughout the wide range of inquiries which the philosophical exercise of his profession embraces.

It is scarcely necessary to say, that our conclusions from any of these premises are liable, like all other conclusions, to be vitiated by any fallacy which may occur, either when listening to nature in observation, interrogating her in experiment, or endeavouring to ascertain the reciprocal relations of cause and effect on which the medical art is grounded. There is, perhaps, no art, the whole processes of which are so liable to the intrusion of fallacies as that of Medicine. Looking back on its past history, it is impossible not to see how much its scientific progress has been retarded by the operation of such fallacies. Some have arisen from imperfection in the ancillary sciences, some from the tendency to theorize, and the neglect of the true principles of inductive science; some from the ambiguity of language, some from the fallacy of testimony, some from the fluctuating character of the phenomena with which we have to deal, and the impossibility of finding two human beings perfectly alike; so that, while in the objects of inanimate nature it may generally be assumed that what has a given effect on one substance will have a similar effect on another of the same kind, there is no such uniformity when the animal bodies, with the various actions and reactions peculiar to life, are the subject of observation.

He who aspires scientifically to practise his profession ought therefore to be skilled in judging correctly of evidence, and able to use his reasoning powers. In this, after all, will be found to consist the difference between the ability and skill of one physician and that of another.

"The intellectual part of our education has nothing more important to do than to correct and mitigate this almost universal infirmity (inability to estimate evidence)—this summary and substance of nearly all purely intellectual weakness. To do this with effect needs all the resources which the most perfect system of intellectual training can command. Those resources, as every teacher knows, are but of three kinds: first, models; secondly, rules; thirdly, appropriate practice. The models of the art of estimating evidence are furnished by science, the rules are suggested by science, and the study of science is the most fundamental portion of the practice."[1]

In what has been already said, we have shown that in the study of Grammar we get our first ideas of reasoning; that in Geometry we learn, first, clearly to state our premises, secondly, to make sure of each step in the reasoning before we ascend to the next, and it is only by thus clearly verifying each step in the process, and thus accurately defining each premiss which we reason from, that we can make sure of the validity of the conclusion at which we arrive. There is, however, another form in which we investigate truth. The one just described is illustrated in Mathematics and the exact sciences; in what are called the experimental sciences, the process of reasoning, though different in form, is essentially the same in character. We start now from a premiss rigidly determined and fixed; we perform an experiment, the results of that experiment are the premises from which we argue to the conclusion at which we arrive. This mode of reasoning is peculiarly applicable to Medicine and to all arts which profess to act upon or draw inferences from experience. Just as a person from habit and association may talk and write grammatically without learning a single rule of grammar, so it is quite possible that a person may reason correctly without a knowledge of the laws of logic. It is the province, however, of logic to lay down the rules which all correct reasoners must follow, whether they know it or not. In a science and art such as Medicine, so beset with fallacies, it is peculiarly necessary that we should have a facility for detecting them, and this will be most easily given by familiarity with those rules which, whether intended to guide us in pure reason or in the pursuit of truth by induction, are the simplest means of detecting fallacies, and of preserving us from error in our intellectual operations. "When I consider," says Mr Mill, "how very simple the theory of reasoning is, and how short a time is sufficient for acquiring a thorough knowledge of its principles and rules, and even considerable experience in applying them, I can find no excuse for omission to study it on the part of any one who aspires to succeed in any intellectual pursuit. Logic is the great disperser of hazy and confined thinking. It clears up the fogs which hide from us our own ignorance, and make us believe we understand a subject when we do not."[1]

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[1] Mill's Inaugural Address, page 23.

If these views be correct, it may well excite surprise that the Medical Council have not added Logic to the branches required for the general education of the physician. Individual members may, and I know have, contended for this addition, but hitherto without success. It is fortunate, however, that it does form part of the curriculum in Arts required of medical students by our Universities, so that it is probable that all the higher grade of physicians will know something at least of the laws of reasoning.

One other subject must engage our attention. Schiller has truly said, "A physician whose horizon is bounded by an historical knowledge of the human machine, and who can only distinguish terminologically and locally the coarser wheels of this intellectual clockwork, may perhaps be idolized by the mob, but will never raise the Hippocratic art above the narrow sphere of a bread-earning craft."[2] The great extension of the domain of Physiology of late years by the empirical mode of inquiry—the microscope, chemical reagents, etc.,—has led our students to a too exclusive attention to what is perceptible to the senses. What the eye can see, the ear hear, the tongue taste, or the hand touch, is believed in; but there is a profound scepticism, or rather crass ignorance, in regard to powerful but hidden agencies which are continually at work. If we have trained our students to understand something of the laws of the physical world, or have at least given them that culture which will enable them to study these laws with advantage themselves, we must next teach them that the Somatic portion is not all man's nature, that there is a Psychical element of which he ought to know something: we must lead them to the boundary where physical observation terminates: but we must cross that boundary and conduct them into another domain (*μετα τα φυσικὰ*) where the study of mind asserts itself.

In the introductory remarks which I made on the various powers of the mind, I attempted to show that the knowledge of facts on which these powers act, was supplied either by Sensation and Perception, which gave the mind the information supplied by the senses, or by the Consciousness which informs us of the operations of our mind. If, then, it is one object of general education to train us to the best means of comprehending and elucidating the phenomena of sense, it is another to make us comprehend the facts which have been ascertained, and the laws which have been discovered in regard to the manifestation of the phenomena of mind. But by the physician such a study is imperatively required. He has to do with matter and spirit united to form body and mind, and a very brief experience in his profession is sufficient to teach him how constantly the mind acts both etiologically and therapeutically on the body, and the body on the mind, and how essential it is that he should understand the operations of both.

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[1] *Op. cit.*, p. 27.

[2] Inaugural Dissertation on the connexion of the Animal Nature of Man with Intellectual. Quoted by Baron von Feuchtersleben.

The same arguments that support Logic apply to Metaphysics, and, as Professor Blackie observes,—"The prejudices of the vulgar English mind against metaphysics must not be allowed to infect our Universities."[1] "Almost everything which has been contributed from these islands toward the advancement of psychology since Locke and Berkeley has, until very lately, and much of it even in the present generation, proceeded from Scottish authors and Scottish professors. Psychology, in truth, is simply the knowledge of the laws of human nature. . . . There are certain observed laws of our thoughts and of our feelings which rest upon experimental evidence, and, once seized, are a clue to the interpretation of much that we are conscious of in ourselves and observe in one another. Such, for example, are the laws of association. Psychology, so far as it consists of such laws,—I speak of the laws themselves, not of their disputed applications,—is as positive and certain a science as chemistry, and fit to be taught as such. . . . There is hardly to be found any discipline comparable to that which these metaphysical controversies afford,—for they are essentially questions about the estimation of evidence; about the ultimate grounds of belief; the conditions required to qualify our most familiar and intimate convictions, and the real meaning and import of words and phrases which we have used from infancy, as if we understood all about them, which are ever at the foundation of human language, yet of which no one except a metaphysician has rendered to himself any account."[2]

To such testimony nothing can be added.

And now, in conclusion, I would merely observe, that it will become those who aspire to be true philosophers not to be deterred from following those methods of education, the value of which has been proved by experience, by Mr Lowe's sneer, "I do not think you will find anywhere in the study of antiquity that which is in everybody's mouth—the idea of progress."[3]

No doubt, Progress is a watchword which has shaped the thoughts and efforts of multitudes, and which each and all of contending sects and parties in religion, in philosophy, and in politics have claimed as their own. It is a rallying cry, too, which has an echo in every human heart oppressed with the sense of want and imperfection in every department of activity, and yearning for truth beyond what it has already obtained.

But that such a yearning may be satisfied,—if satisfied it ever can be in a world where perfect knowledge is unattainable,—we must not regard man's conquests over matter as the signs of his highest advancement. We may harness the steam to our chariot, and, like one of the genii of the Arabian Nights, make it our slave and bid it do our will; we may make the lightning our messenger, and flash our thoughts from continent to continent with an accuracy and rapidity, to which only the will, acting through the nerves of the animal body, seems a fitting comparison; we may bid the Sun be our artist, and instantaneously produce and record a copy of the image, dead or alive, presented to him.

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[1] On Education, p. 35.

[2] Mill's Inaugural Address, pp. 31 and 32.

[3] *Op. cit.*, p. 22.

All this shows us the superiority of man over matter, or rather the superiority of that spiritual force of which he is conscious in the depths of his personality,—a force which the eye cannot see nor the hand touch, which the scalpel of the anatomist cannot demonstrate, or the skill of the physiologist unravel, which has no local habitation even in the body which it inhabits and controls, but which makes itself known and asserts its dominion in the movements of the muscles which the will commands, in the pulsation of his heart which he cannot control, in each molecule of his tissue which it fills with its life.

The real progress is the progress of man intellectually, separated and isolated from all that lies outside of him; and such progress does not consist of the increase of one mental power to the prejudice or neglect of others. We must not regard the book of human knowledge as a ledger in which, by the "science of ponderation," we shall register each acquisition, and insert in the money-column after it its precise commercial value. The highest work of education is not the accumulation of facts, but the education of faculties, the discipline and development of intellect; and while intellect, so cultivated and refined, will ever be the best means of advancement in the acquisition of knowledge, and in its application to the business of life, it will satisfy every requirement of true progress; for while all that is material in us shall pass away, that which belongs to our immaterial nature, if well and wisely directed, will be quickened into new energies, and find at last, in another sphere of being, its aspirations after ideal perfection fully satisfied.

Part Second.

REVIEWS.

Review of the History of Medicine. By THOMAS A. WISE, M.D., Fellow of the Royal College of Physicians, Edinburgh, late Bengal Medical Service; Secretary to the Committee of Public Instruction, Bengal; Principal of the Hooghly and Dacca Colleges, etc. 2 vols. 8vo. London: 1867.

THE name of Dr Wise is already well known to those who are interested in, and are at all familiar with, the progress of medical science in ancient times and among primitive nations. His learned work, entitled "Commentary on the Hindu System of Medicine," which appeared at Calcutta so long ago as 1845, and of which a new issue has been published by Trübner of London in 1860, is at once a monument of its author's profound erudition, and a most instructive and interesting guide to all inquirers in a similar direction. It is with justice that Haeser, the learned German historian of medicine, in his "Lehrbuch der Geschichte der Medicin," has styled the earlier treatise of Dr Wise, "Eine sehr gediegene Arbeit."

The present volumes, which are entirely occupied with the consideration of medicine among the Asiatics, are the first instalment of a much larger work, one, indeed, which is to treat of the progress of medicine during all the centuries which have passed, and among every nation. The materials for such a "Review of the History of Medicine," Dr Wise informs us in his Introduction, "have been slowly collected as an amusement during the course of my studies, and during a long residence in Asia, where my occupation, inclination, and curiosity brought me into frequent contact with the native practitioners." If our author succeeds in producing a complete history of medicine which shall equal in interest and solid instruction the matter which is contained in the present volumes, he will have achieved a very great result, and conferred a signal benefit on his profession.

Dr Wise proposes to consider the progress of medicine under five periods; and these, as affording a proper view of the scope of his work, we shall place before our readers, before proceeding to indicate very briefly the nature of the contents of the early volumes now in our hands.

"1. The *primitive oriental period* was that in which the Aryan race, a sober, reasoning people, employed their intellectual vigour and natural sagacity in the investigation of useful truths, developing their powers under a happy combination of circumstances. At that early period the Hindu Brahmins arranged laws, regulated the calendar, promulgated distinguished scientific and literary efforts, and practised the healing art in all its branches. The mysteries of theology were combined with medicine and astrology by this remarkable people, which was modified by sectarian Buddhists.

"2. The second, or western branch of the Aryan race, form the *ancient period*, and will include the Grecian and Roman systems of medicine, from the time of Thales and Pythagoras to the time of Sextus Empiricus, towards the end of the second century. During this period, there were stores of information accumulated by great men, which, combined, adorned, and reflected by Grecian men of genius, conferred such glory on their country, and imperishable renown upon themselves. Lycurgus, Solon, Pythagoras, Plato, Herodotus, and Hippocrates derived the first principles of their investigations of mental and practical philosophy from an eastern origin. The duties of legislator, natural philosopher, religious teacher and physician were combined in the same individual, or class of individuals, at this early age, and knowledge was strangely confounded and entangled. Greece became the seat of philosophy, and carried the sciences, including medicine, to a state of very considerable perfection. The Romans, so illustrious in arms and wisdom, were formed from the Etrurian, or Turanian, and Celtic races, which created the Roman civilisation, and that of the south of Europe. The mixture of races, the climate, etc., modified the character of the inhabitants and the productions of the country. The Romans succeeded the Greeks, though they did not attain their intellectual greatness. They were their scholars; they learned their language, and imitating the works of some of their sages, they formed a civilisation of their own. The study of ancient literature in Europe was interrupted by the invasion of rude and hardy races from the north, who subverted the Roman empire, and destroyed what was noble with relentless barbarism, preserving only what harmonized with their rude manners, or administered to their sensual pleasures. Persecuted by such ignorant masters, the learning of the West sought refuge in Egypt and the East, and Europe remained for ages in a state of lethargy.

"3. The *transition period* will contain a sketch of the Egyptian and Jewish systems, and of the decay of learning in Europe. In this period, under the patronage of the Caliphs of Bagdad, medicine was cultivated with diligence, and with a certain measure of success, both in Asia and Europe. This temporary greatness of the Mohammedan Arabs was not lasting, and their literary zeal, though energetic, was short-lived. The translations and compilations, which they prepared from eastern and western works of science, were for a time studied in the East as well as the West; but amidst the political revolutions and changes in Asia, they fell into a state of neglect.

"4. In the *restorative* or *copying period*, when learning began again to flourish in the congenial soil of Europe, medicine slowly revived.

Security for life and possessions being obtained, convenience and enjoyment were the next objects of attention, and learning was found to constitute the highest gratification of the mind. The arts and sciences were first studied in the gloom of the monasteries; but the Mohammedan schools in Spain and other parts of Europe were generally resorted to for studying the Arabian translations of the ancient classical authors, which would otherwise have been lost. Individuals and nations vied with each other in honouring the pursuit of learning; and the study of medicine exercised the genius of many great men, with much advantage to suffering humanity.

"5. The fifth, or *philosophical period*, extends from the revival of literature and medicine in Europe in the fifteenth, to the beginning of the nineteenth century. In this more mature period of the history of medicine, the science was constructed on anatomy, observation, and reasoning, which led to a more correct classification and treatment of disease. Medical science being thus settled on its true basis, anatomy and physiology advanced rapidly. New facts being a foundation for those which are to follow, and not being confined to one state, or existing among one race, the advancement of the science resulted from the labours of able men in many lands. Though impeded at times by ignorance and prejudice, as learning advanced, the stream of knowledge flowed clearer, purer, and broader; the wisdom of to-day was eclipsed by the science of to-morrow. The more knowledge men acquired, the greater were the number of aspirants and the more ardent their thirst for advancement in every branch of intellectual undertaking."

The foregoing quotation, while indicating, as we have already stated, the scope of the author's undertaking, exhibits also the philosophical spirit in which he enters upon it, and justifies the opinion which we are glad to express, that, for its satisfactory execution, he possesses every needful requirement.

In the description of the ancient Hindu system of Medicine, Dr Wise is the more particular from the circumstance, that it is so little known in Europe, and because it is the prototype of so many other systems. There is a very interesting chapter on *the profession in ancient Hindostan*, including a consideration of the education and duties of the physician. From the latter we extract the following admirable piece of advice:—"The physician should always be dignified in his deportment, correct in his manners and habits, gentle and kind, amiable, cheerful and collected. His language should be mild, candid and encouraging, rather like that of a friend than an acquaintance, and he should be always ready to assist the sick. His heart should be always pure and charitable, and he should carefully follow the instructions of his Guru and of his predecessors. Such a physician should possess a character for strict veracity, be of calm temper, and of the greatest sobriety and chastity. He should be a man of sense and benevolence, and his constant thought should be how he is to do good. A person may be afraid of his father and mother, friends and master, but not of his physician; so the physician should be more kind and considerate to the sick than a father, a mother, a friend, or a master." —p. 69, vol. i. How scrupulously exact the rules affecting modesty were in the Hindu code may be gathered from this precept:—"The practitioner should avoid frivolous or improper language, particularly with females; he should not sit down upon the same bed with them, and the only present he should receive from them is food." In the Hindu system there was also a proper estimate of the remuneration of the physician, for we find it stated that, should a patient not pay his physician a price equivalent to the value of his soul, then all his holy virtues, and good acts, which he has performed during his lifetime will belong to the physician. That price will vary with the rank and condition of the patient. Money will be the recompense bestowed by the rich; friendship, reputation, increase of virtue, prayers, and gratitude will be that of the poor." Again, "Physicians have often been blamed for not attending to their recompense, which embraces love, riches, religion, and happiness, and they lose these benefits by accepting an insignificant sum of money. Hence they say, 'they exchanged a bag of gold for a mass of dross.'"—p. 89, vol. i.

The remainder of the first volume and the larger portion of the second are devoted to the examination of the physiology, anatomy, pharmacy, hygiene, surgery, midwifery, and practice of medicine among the ancient Hindus. These portions correspond very closely, in many parts exactly, with the information contained in Dr Wise's earlier treatise. Towards the termination of the second volume, there is a very interesting review of the Buddhist system of medicine, and the last hundred pages are devoted to a consideration of the *Chinese* system of medicine.

We shall be delighted to meet Dr Wise at a future time, and recommending these volumes of his to the study of our readers as being replete with curious—but not on that account the less interesting—information, we shall terminate this notice by making one other quotation. Alluding to the preservation of health and warding off of death, the Hindu writer has thus expressed himself: —"Man is like a coachman driving his own carriage; if this be well made, and if he continue to drive cautiously, it will go for a long time; but if he drives it upon bad roads, the wheels will get injured, and the carriage will be soon worn out. Should he indulge too much in the gratification of the senses, he will die like a deer, which is supposed to be deluded to its destruction by the sweet sounds of the lute which the hunters use; should he indulge in lust, like the elephant; in sight, like the butterfly in approaching the lamp; in smell, like the bee which is enclosed and crushed in the flower which has attracted it by its smell; and in taste, like the fish by the fisherman's hook. So beware of indulging too much in any of the pleasures of sense, else it will lead, in like manner, to your destruction."

A Practical Treatise on the Diseases of Women. By T. GAILLARD THOMAS, M.D., etc. Philadelphia: 1868.

WITHIN the last few years the American press has sent forth a comparatively large number of books on Gynekology. That to which we now direct attention is certainly not the least deserving of notice. It aims at being a systematic treatise, and it is well done on the whole, from the author's point of view. It is in no sense an original work, unless we regard its comparative completeness as original. Everywhere it rests on authority; scarcely anywhere on original observation. No doubt the author could not write with the correctness and authority he shows, unless he had himself had considerable experience in the subjects treated, and had carefully studied them. Yet the book has not that appearance of authority founded upon thorough study and mental digestion of a subject, which a systematic treatise should have; and this is quite in accordance with the circumstance that the author is young and little known. On this side of the Atlantic, such a book as Thomas's is almost never published, without the author having previously laid his credentials before the profession in the form of original contributions to the science or art of which he intends to treat systematically. And we regard this as a good plan; for it can very rarely happen that an author should, in our day, begin with a systematic treatise. Extensive and profound study must have revealed to him observations which he should have published, both as a duty to his brethren and a source of pleasure, if not profit, to himself, before he commenced a general view of the subject which is meant to be "A Practical Treatise on the Diseases of Women." We say all this from no feeling of disrespect to the very creditable work now before us.

Dr Thomas has written a valuable book, which will be useful to students, especially those before whom he professes. They will get a fair and not one-sided view of the subjects treated, and one which is a very fair reflection of the gynekological days in which it is written.

But what of those days? Should a teacher not soar above the little fleeting ingenuities of the hour, destined some of them to oblivion before his ink is dry? Should he not everywhere carefully distinguish between what is fact and what is not? With this view, should he not give the pathology carefully, or the morbid (not transcendental) anatomy? Should he not, when facts are scarce and theories must prevail, as in therapeutical topics, give what has been confirmed by time and authority, and omit what is merely recent and merely novel? We think he should; and these things Dr Thomas has not sufficiently attended to. Dr Thomas is, as we have said, to a great extent a reflexion of his day, and a deal of his practice—founded as it is neither on sufficient authority nor sufficient facts—will read in a few years like the practice of the ancients, as credulous and as useless. A delightful example we may give, from the treatment of chronic cervical endometritis:—

"Another excellent plan (says Thomas) of treating this affection, is by the use of medicated tents of sponge. For the past six months (!) I have employed it very generally, and now prefer it to any other mode of treatment. . . . . Sponge, cut into proper shape, is saturated for a length of time in solutions of zinc, copper, iron, iodine, carbolic acid, or lead, ... alteratives of most reliable character" (!) Instead of this, cervical suppositories may be used. "Into these cervical suppositories may be introduced zinc, copper, iron, lead, or bismuth, with opium, conium, or hyoscyamus."

We have called this a delightful specimen of practical advice. It is like its day on both sides of the ocean, and it is delightful only because amusing. What good can come of it to students? What humbug or evil may come of it to patients? We have said that such therapeutics is not unlike that of the ancients, and may in proof point out that, in the treatment of pelvic abscess, Archigenes is said to have recommended sitz-baths of penny-royal, horehound, laurel, sage, mugwort, dittany, centaury for drawing; and under other circumstances, cerates of rose-oil or butter, containing dross of furnaces, antimony, plumbago, or litharge of silver, with some milk from the human female!!

Where is the difference between Thomas and Archigenes? The difference lies in this, that Thomas ought to know better than Archigenes; that a modern American should not have such credulity —or at least not show it—a credulity that is beautiful in the nursery, but disgraceful to modern gynekology. What should we say did we meet with such therapeutics in the works on Practice of Medicine where conjunctivitis, or tonsillitis, or bronchitis, or cystitis are discussed? We fear that gynekological therapeutics are in a very backward state.

These remarks do not apply merely to Thomas's work. They apply to much of the writing of our day. In regard more particularly to the book before us, we have pleasure in pointing out that it has a good introductory historical chapter.

The author presents us with scarcely required additional evidence of the growing corruption of female virtue in the United States.

"On my table," says he, "at this moment lies one of the most popular, respectable, and best edited daily journals of New York—one which finds its way into the first circles of society, and into the hands of maidens and matrons throughout the land. In its columns I count fifteen advertisements well known as being those of professional abortionists—men and women who make a business of infantile murder."

Dr Thomas participates in the erroneous but common views as to the hardiness of savage or degraded women. "The Indian squaw," says he, "or Southern freed-woman, may go half-naked while menstruating, carry heavy burdens from morning till night, or rise to labour or to travel in a day or two after parturition, and yet no evil will result." To this we only say, *Omne ignotum pro magnifico!* We could say much more. Let the author consult practitioners among Indian squaws and Southern freed-women, and he will find his error.

Here is a good and true remark, and withal curious. "There is always danger in dilating the cervix by tents, though it is by no means so great as make one hesitate in employing them. In a case which I saw, in consultation with Dr Edward Parsons, I employed two tents in succession, and, in about twenty-four hours after removal of the second, tetanus developed itself, which proved fatal. In one case, in the practice of another physician, I have seen death result from peritonitis after their use; and in three others have known periuterine cellulitis thus produced."

And now we end with some too true and sage remarks, and with thanks to Dr Thomas for his valuable volume.

"It is not rare to meet with instances in which physicians have, for months, treated cases of uterine disease, concerning the nature of which they not only did not have a correct theory, but had no theory at all. Under these circumstances, the most general practice is to pass, about once a week, a solid stick of nitrate of silver up to the os internum, not to cure cervical endometritis—for that has never been suspected—but to do the best one can in the way of treatment when he does not know the nature of the disease which he treats. I have no inclination to attribute this to any intentional laxity of morals, but rather to indecision and aversion to creating a disagreeable issue with the patient. It is, however, impossible to deny the fact, that such a course will sometimes be pursued by those who, in the case of a diseased eye or inflamed knee-joint, would not hesitate to confess, with the utmost frankness, their uncertainty and need of assistance. With uterine, as with all other diseases, the diagnosis must be properly made before treatment can prove curative."

Handbook of the Sphygmograph, being a Guide to its Use in Clinical Research; to which is appended a Lecture on the Mode and Duration of the Contraction of the Heart in Health and Disease. By J. BURDON SANDERSON, M.D., F.R.S., Physician to the Hospital for Consumption, Assistant Physician to Middlesex Hospital, and Joint Lecturer on Physiology in the Middlesex Hospital Medical College. Pp. 83. London: Robert Hardwicke: 1867.

THIS little work consists of three chapters. The first describes the sphygmograph and the mode of applying it; the second enters into an examination of the different characters of the pulse; and the third contains some important remarks on the various disorders of the circulation found in the different forms of heart disease. In illustration of the physiological part of the subject, the author has appended (what is equivalent to a fourth chapter) the lecture on the phenomena of the contraction of the heart in relation to the arterial pulse, which he delivered at the Royal College of Physicians of London in March 1867. The handbook accordingly comprises a sketch of nearly the whole information necessary for investigations with the sphygmograph.

In his account of the instrument, Dr Sanderson describes a new method of fixing it, which he has devised in place of the bandages employed by Marey for that purpose. This plan consists "in adjusting to the end of the instrument a rectangular block of brass, by the under surface of which it rests on the tendon of the *flexor longus pollicis*, and on the space between that tendon and the spine of the radius; the block being kept closely applied to the surface by means of a strong elastic band which encircles the wrist." The forearm should be, at the same time, supported and controlled in its movements by means of a pad, consisting of a board nine inches long and three and a half broad, the upper surface of which is well padded, with the padding thickened to form a cushion at one end. The author likewise gives directions for regulating the pressure of the spring, and applying it to the point of the artery most favourable for exhibiting its expansile movements, and he recommends instead of paper the use of smoked glass, the tracings of which may be preserved by varnishing them. These and similar contrivances facilitate and render more certain the application of the instrument, which, however, under all circumstances, require some patience and dexterity.

We shall not follow the author into his examination of the phenomena of the pulse and the circulation. It is well known that he has made a special experimental study of these subjects, and the results of his inquiries have not as yet been completely published. We trust, therefore, to have before long a more complete exposition of his views.

In his appreciation of the value of the sphygmograph for clinical purposes, Dr Sanderson insists very properly on the indirect advantages that will result from it in the more accurate study of the pulse at the bedside. The older physicians possessed a discriminative *tactus eruditus* which their successors have failed to cultivate. The finger can recognise the different forms of pulse as well, probably better than the sphygmograph can show them; but the latter has the advantage of manifesting them to the eye of the beginner, and preserves a permanent record, the various details of which can be accurately explained. If the eye in its turn instruct the hand, the use of the sphygmograph may serve to educate a tactile sense sufficiently precise to be substituted for it on ordinary occasions. That there was great need for more accuracy in the definition and nomenclature of pulses is well brought out in the author's excellent second chapter.

When Marey first introduced his sphygmograph, it presented such a marvellous superiority over the similar instruments which had preceded it, and the phenomena it presented were so lucidly explained in his treatise, that sanguine persons entertained the hopes of seeing auscultation and percussion superseded, and the diagnosis of cardiac and arterial disease reduced to a process of self-registration. These lightly-formed expectations are as yet very far from being realized, as Dr Sanderson's candid estimate of the actual and probable results of this method of investigation show.

> "The sphygmograph is not to be regarded, like the laryngoscope or the ophthalmoscope, as an aid in the discovery and discrimination of organic diseases, for affections the most diverse communicate to the pulse the same graphical characters. Its use is to enable the physician to investigate the state of the circulation and circulatory organs in diseases of which the general nature is already recognised, with reference to (1) the mode and duration of the contraction of the heart; (2) the soundness of the arteries; and (3) the relative quantity of blood contained in the arteries and veins, or in other words, the balance of pressure between the venous and arterial systems. If it can be shown that the sphygmograph affords a reliable means of determining these most important conditions of disease, it is quite enough, without forcing it to applications of which it is from its very nature incapable."

In the present state of the inquiry, the sphygmograph can only be used with advantage by those who have specially studied it in connexion with the more recent investigations into the physiology of the circulation. The results already obtained by means of it have been sufficient to prove that it is of great value as an instrument of clinical research, and from the number of able and zealous investigators who are engaged with the subject, still further progress may confidently be anticipated.

Part Third.

MEETINGS OF SOCIETIES.

EDINBURGH MEDICO-CHIRURGICAL SOCIETY.

SESSION XXVII.—MEETING VI.

18*th March* 1868.—Dr OMOND, *President*, in the Chair.

I. EXCISION OF OS CALCIS.

Mr Annandale introduced a patient in whom the entire os calcis had been removed, as much periosteum as possible being left. The result had been most satisfactory; the patient being able to walk with the foot operated on.

II. CYSTIC EXPANSION OF BONE.

Mr Annandale showed a preparation of a cystic expansion of bone at the lower end of the radius, which had been the result of a vascular tumour. The dried specimen presented the appearance of *spina ventosa.*

III. CALCULI.

Mr Annandale exhibited a calculus which he had removed by lithotrity, and explained that an enlarged prostate had rendered the operation unusually difficult.

Mr Annandale also showed a urethral calculus which he had removed from a young child, in whom it had caused retention of urine.

Mr Annandale also showed a calculous body which lay quite loose under the muscles of the upper eyelid, and which he had removed.

IV. PECULIAR FIBROUS POLYPUS UTERI.

Dr Matthews Duncan exhibited a polypus the size of a walnut. It had caused extreme anæmia by producing hæmorrhage. It was not a fibrous tumour, but a continuous outgrowth. It had three lacunæ filled with chocolate-coloured blood. Two of these communicated with one another by a small aperture. It was covered by a layer of mucous membrane. Dr Grainger Stewart had examined the polypus. The lacunæ were lined with columnar epithelium.

V. PECULIAR ABORTION.

Dr Matthews Duncan showed an early abortion which came away entire, that is, the ovulum was involved in its decidua vera and reflexa. The ovulum was about the size of a small grape. It was, as covered by reflexa, connected with the vera by a part constructed like a neck, having less diameter considerably than the ovule.

VI. CASE OF AORTIC ANEURISM, WITH LESIONS OF THE CILIO-SPINAL CENTRES AND OF THE PHRENIC NERVE, OCCURRING IN PROFESSOR LAYCOCK'S CLINIC. REPORTED BY DR HADDON.

Alexander M'Nab, æt. 38, labourer, admitted January 22, 1868.—The patient states that he enjoyed good health until about eight years ago. At that time, being in the Crimea as a soldier, after exposure to cold and wet, he had an attack of rheumatic fever. During this illness nearly all his joints were swollen and painful, and he was confined to the house for fourteen weeks. After his recovery he occasionally had pains in his joints, especially during changeable weather. So far as he is aware, his heart was not affected by the disease. After his return from the Crimea he was employed in a boiler manufactory, where his chief occupation consisted in using large heavy hammers. Twelve months ago he began to suffer from breathlessness after working for any length of time; and shortly after this he was troubled with fits of coughing and palpitation, which obliged him to abandon his employment. Six months ago he observed that he became giddy on the least over-exertion, as running or lifting a heavy weight. Within the last three months he has had occasional attacks of hæmoptysis, to the extent of several mouthfuls at a time. Feeling himself becoming worse, he entered the Infirmary. *On admission.—Respiratory System.*—He was troubled with a cough occurring in paroxysms, and of a spasmodic character. The paroxysms began with a difficulty in breathing, during which his countenance assumed an anxious expression, his eyes were slightly protruded, while his face and neck became congested. The cough then supervened. It consisted of a series of expirations, which, at first of moderate length, gradually became shorter, until he was left sitting motionless, his face and neck greatly congested, and his jugular veins standing out as thick as one's little finger. His eyelids were closed. He then took a deep inspiration, and immediately fell back on his pillow quite exhausted. A second attack generally followed, of a milder nature, and the paroxysm invariably terminated in the expectoration of a mouthful of frothy mucus. *On percussion*, there was comparative dulness over the upper part of left lung, as far down as the third rib. *On auscultation*, the respiratory sounds were normal. *Circulatory system.*—Pulse 92; small and weak. There was visible pulsation in arteries, at the root of the neck. The apex beat in its normal position. The cardiac dulness was 2½ inches. *On palpation*, the heart's action could be felt more distinctly than usual. *On auscultation*, a long blowing murmur of a musical character could be heard all over the cardiac region. It was loudest over the third rib, about an inch to the inside of the left nipple. It was very distinctly heard from the third to between the first and second ribs on the left side. Over the right side it was much less distinctly heard. *Nervous system.*—He complained of pain over the cardiac region, extending through to the back, and down the left arm as far as the elbow. He also had pain in the lower part of the chest, on the right side, when coughing; and percussion over the hepatic organ was painful. After the paroxysms of coughing he was troubled with giddiness and palpitation. The digestive, genito-urinary, and integumentary systems were normal.

Progress of the Case.—On the 23d, five drops of the nitrate of amyl were given by inhalation. It relieved the anginal pain for about six hours, but increased the severity of the paroxysmal cough. On the 24th dry-cupping over the cardiac region relieved the pain for more than a day. The murmur gradually lost its musical character, and on 5th February it was of a harsh blowing character, of maximum intensity, about 2 inches above and to the inside of the left nipple. The anginal pain and spasmodic cough continued. He was ordered ℞ atropiæ gr. $\frac{1}{4}$; aquae ℥viij. Sig. two tablespoonfuls night and morning. While taking this he felt himself better, the pain in the cardiac region being less severe. On the 8th there was a trace of albumen in the urine. On 10th February he was ordered Parrish's syrup of the phosphates. On the 12th the albumen had disappeared from the urine, and he continued much in the same condition. On the 24th it was noticed that the cough was becoming more frequent and anginal pain more severe. There was now one-sixth of albumen in urine. He continued in this state till the 11th March, on the morning of which day he had a violent attack of coughing, attended with great pain in the right hypochondrium. About 1 o'clock P.M. he had a similar attack. After it he lay on his left side, moaning and very much exhausted; his pulse was almost imperceptible; his lips were pale, and the surface of his body cold. At 8 P.M. he had a third attack, from which he never rallied, but, gradually sinking, died at 11 P.M.

Dr Laycock diagnosed from the paroxysmal attacks, the physical signs, and the spinal nerve pains, an aneurism of the sinuses of Valsalva, and a lesion at some point of its course of the phrenic nerve.

Post-mortem.—When the thorax was opened, a careful dissection of the phrenic nerves was made, with the heart and lungs *in situ*. On tracing the *left* phrenic nerve downwards, it was found adherent to, and partially implicated in a small tumour, of the size of a filbert, situated in part of root of left lung, and composed of a bronchial gland enlarged, infiltrated with carbonaceous matter. On microscopic examination, the nerve tubes were found continuous at the affected part of the nerve; the substance was slightly granular. The right phrenic nerve was natural. The vagi and recurrent nerves on both sides natural. Pericardium was adherent throughout, very loosely behind, but firmly on upper part of anterior surface over an aneurismal swelling, to be presently described. The heart was of natural size. Ventricles were somewhat hypertrophied. Right auricle somewhat dilated. Tricuspid orifice widened. The valves on both sides were thickened at the margins, but appeared competent. Half an inch above the attachment of the aortic valve there was a round opening on the arterial wall of the aorta, leading to the aneurism. The opening measured about half an inch in diameter. The aneurism was of the size of a small orange, contained numerous layers of clot, the older ones partially decolorized. It projected forwards somewhat to the right side, and encroached on the muscular substance of the outer and anterior wall of right ventricle. It obstructed the entrance of the finger into the orifice of pulmonary artery, and one segment of the valve of pulmonary artery was adherent to the rounded surface of the tumour. The pulmonary artery beyond this point was natural. The aorta was atheromatous throughout the arch and for some distance down. No important vein was compressed; abdominal viscera presented no important lesion.

Comment by Dr Laycock.—This case is of special interest, as illustrating the complications of nerve-lesion in thoracic aneurisms. The nervous symptoms were both motor and sensory, very distressing, and ultimately so associated with the condition leading to death, that it is probable the patient died of the lesion of the phrenic. The nerve chiefly affected in aortic aneurisms of the arch is the recurrent branch of the vagus, and the symptoms are laryngeal. In this case, as there was no affection of the larynx, there was at least no aneurism of the arch sufficiently large to interfere with the recurrent nerve. But the physical signs pointed rather to an aneurism of the ascending portion. The patient had moreover pains such as are felt in angina pectoris, viz., over the cardiac region, extending through to the back, and down the left arm as far as the elbow. The nerves thus involved were therefore branches of the cervical plexus, and chiefly derived from the seventh cervical and first dorsal pair; and since these pains are chiefly felt in men in cases of angina with degeneration of the coronary arteries, it seemed to me probable that the probable aneurism indicated by the peculiar roaring tone of the murmur was within the pericardium, and occupied one of the sinuses of Valsalva. There remained for explanation the singularly distressing paroxysms of cough and dyspnœa, with pain in the right hypochondrium, and slight but distinct exophthalmos, ending in suspension of respiration and drooping closure of the eyelids. It was one of a succession of these paroxysms which proved fatal. At first the extreme anguish manifested, and the upright position of the patient led to the conclusion that the aneurism had burst into the pericardium, but this was at once set aside by the fact that the intense pain was referred not to the region of the heart, but to the right hypochondrium; and further, instead of leaning forward in the sitting posture, as is the usual position when there is rupture into the pericardium, the patient lay on his right side. The motor symptoms indicated a lesion of both the cardiac and respiratory motor nerves; but which of the latter? That it did not involve the respiratory centre, seemed clear from the localization of the pain in the right hypochondrium and the affection of the eyelids, which are under the influence of the motor fibrils of the seventh cervical and first dorsal, as Claude Bernard has demonstrated. It seemed probable, then, that a nerve-trunk was involved, and that this might be either the left or right phrenic nerve, if we assumed that the pain referred to the right hypochondrium was in the diaphragm. On careful examination, it did not seem to be hepatic. As the *left* phrenic nerve was found to be exclusively involved, the explanation of the pain in the right hypochondrium implies crossed action of at least the sensory portion of the nerve. The whole subject of the cardiac neuroses requires, however, reconsideration. All, perhaps, that we can certainly conclude in a case like the preceding is, that the nerve-lesions are essentially spinal, and involve the cilio-spinal axis, and that the phrenic is involved secondarily.

Dr Sanders remarked that, in a case under his care about a year ago, an aneurism springing from the concavity of the arch of the aorta, near the junction of the ascending with the transverse portion, and compressing the pulmonary artery just before its division, had produced symptoms of disordered circulation very similar to those detailed in Professor Laycock's case. In Dr Sanders's case, however, the phrenic nerve was not involved, as in the one just detailed.

VII. *Mr Annandale* read a paper recording a

CASE OF LOOSE CARTILAGE IN A BURSA.

With reference to this paper, *Dr Chiene* showed a preparation of a cyst or bursa in connexion with the popliteus tendon at the back of the knee-joint, which contained calcareous loose bodies in its interior.

VIII. *Dr Thomas R. Fraser* read a paper

ON THE CONNEXION BETWEEN CHEMICAL CONSTITUTION AND PHYSIOLOGICAL ACTION; WITH SPECIAL REFERENCE TO THE PHYSIOLOGICAL ACTION OF THE SALTS OF THE AMMONIUM BASES DERIVED FROM CERTAIN VEGETABLE ALKALOIDS.

Dr Fraser explained that his paper contained some results of a joint investigation undertaken by Dr Crum Brown for the chemical, and himself for the therapeutical part of the inquiry. An account of these researches had been read to the Royal Society of Edinburgh.

Dr Matthews Duncan remarked on the great interest and importance of the paper. Scientific therapeutics was a branch of inquiry far too seldom brought before medical societies; he did not remember a paper on the subject for many years.

Dr John Duncan had already tried the sulphate of methyl-morphia with advantage to procure sleep, without the irritant action of opium or morphia simply.

XI. *Dr Chiene* read the description of

A CASE OF INTRA-ABDOMINAL HERNIA.

This interesting case was a rare example of an intra-peritonæal hernia, arising from congenital malformation.

SESSION XXVII.—MEETING VII.

1*st April* 1868.—Dr Omond, *President*, in the Chair.

I. THORACIC ANEURISM.

Dr George W. Balfour introduced a patient, a man of middle age, who had been under his charge for a considerable time labouring under a thoracic aneurism, situated in the upper and left front of the chest. Under the long-continued use of iodide of potassium the aneurism had latterly diminished in prominence, so that the walls of the chest presented the natural level; the tumour also had become solidified, the dyspnœa and dysphagia formerly complained of had disappeared, and pulsation could only be obscurely felt. Dr Balfour regarded the case as a very satisfactory example of the benefits to be derived from this treatment.

Dr Warburton Begbie, who had seen the case before it came under Dr Balfour's care, noticed the great change which had taken place in the tumour, in confirmation of Dr Balfour's account. He referred to the successful results which had been obtained by Bouillaud and others in the treatment of aneurism by the iodide of potassium. He had also met with encouraging results in his own practice; and even when the iodide did not cure the aneurism, he had always found it efficacious in relieving pain.

Dr Sanders remarked on the satisfactory progress of the case, but considered that longer time was required to establish the evidence of a complete cure, as aneurism not unfrequently, by changing in the direction in which it extended, produced apparent amendment on the parts of the tumour obvious to sight and touch. The case exhibited, however, as far as could be seen, was very encouraging.

Dr Balfour stated that he would bring the subject of the treatment of aneurism by iodide of potassium fully before the Society on a future occasion.

II. ANEURISM OF THE HEART.

Dr Grainger Stewart exhibited a specimen of aneurism at the apex of the heart from atrophy of the muscular substance, the peri- and endo- cardium being in contact.

III. *Dr Stewart* also showed a preparation of an aneurism of the left ventricle, formed by the giving way of the endocardium and muscular substance; ultimately rupture of the heart took place, and the patient died a few minutes after the rupture had been completed.

IV. *Dr Stewart* exhibited specimens of cancer of the lungs and heart, taken from a recent case.

V. *Dr Stewart* also showed the lungs in a case of tubercular disease, in which the lower lobes were specially affected.

VI. *Dr Joseph Bell* read the account of a very remarkable

CASE OF COMPOUND FRACTURE OF CRANIUM WITH HERNIA CEREBRI, AND COMPOUND FRACTURE OF PELVIS, FOLLOWED BY RECOVERY,

which will appear in next number.

Dr Maclaren read notes of a somewhat similar case from his own practice; and *Dr Argyll Robertson* remarked on the symptoms of eye affection in Dr M'Laren's case.

VII.—*Dr John Duncan* read a paper on a

CASE OF EPILEPSY CONNECTED WITH VESICAL CALCULUS, CURED BY LITHOTOMY,

which will appear in next number.

VIII.—*Dr Sanders* read a communication on

THE VARIATIONS OF ORGANIC CARDIAC MURMURS,

which will appear in an early number.

Dr Halliday Douglas considered that the explanation of the variations of organic cardiac murmurs was a subject of considerable difficulty and obscurity, and one which required to be investigated. In acute endocardial inflammations, both the position and character of murmurs frequently underwent alterations. Many years ago, in a case of contracted mitral valve, he had observed the murmur disappear, and remain absent for many years before the patient's death.

Dr Warburton Begbie confirmed the not unfrequent occurrence of variation in organic cardiac murmurs, and more especially with respect to the presystolic murmur.

Dr Grove had recently observed a case of bronchorrhœa, attended with great dyspnœa, in which a loud murmur, audible at the apex, had subsequently disappeared.

Addendum.—The following was inadvertently omitted from the report of the Meeting of 5th February :—

CANCER OF BLADDER, UTERUS, AND RECTUM.

Dr Millar showed a preparation of firm medullary cancer, involving the rectum, uterus, with the upper part of vagina and posterior wall of bladder, which occurred in the case of a female, aged 65, who presented all the well-marked characteristics of the cancerous cachexia. The history as well as symptoms of the case were very obscure, so that it was only at the post-mortem examination that the full extent of the disease could be accurately determined. During life the urine was thick, sometimes tinged with blood, and contained well-marked cancer cells, which were also observed microscopically in the examination of the pathological preparation.

PROCEEDINGS OF THE EDINBURGH OBSTETRICAL SOCIETY.

SESSION XXVII.—MEETING VIII.

11*th March* 1868.—DR BURN, *President*, in the Chair.

Dr Aitchison was admitted as a Corresponding Fellow, and Mr Brotherston, Alloa, as an Ordinary Fellow of the Society.

I. CASE OF SUPERFŒTATION. BY DR AITCHISON. COMMUNICATED BY DR BRYCE.

During the hot season of 1866, I was called to visit a case nearly two miles from Umritsur. On arrival, found it to be a case of premature confinement. Mrs M., age about thirty years, a small but well-formed woman, who had been born and brought up in India, had born two children at full period of gestation; and then followed two miscarriages; lately been suffering a good deal from fever, for which she had been attended upon by myself; her general health not good at present; system much relaxed from debility, caused chiefly by the prevalent great heat of the season. On examination, I found that after a short and easy labour the child was born, and the placenta discharged.

The uterus was fairly contracted. On examining the child, found it to be a case of premature birth at the seventh month, which corresponded with the mother's statements. The child did not survive a few hours.

I then examined the placenta, to which, much to my astonishment, was attached a large sack. This, on closer inspection, proved to be another set of membranes in an entire state, containing another fœtus. The placenta of this last being incorporated into that of the first-born fœtus, much smaller, but with a distinct mark of union still existing. At one point the membranes of both the fœti were inseparably united, and those of the smaller fœtus were much more dense in structure, and less transparent than those of the larger fœtus.

The fœtus contained in the sack (which was not opened) seemed from its size and development to have completed nearly its fourth month of utero-gestation.

The large placenta, with its attachments, I carried off from the house, without informing any one of the second fœtus; otherwise a valuable acquisition to the Lahore College Museum might have been lost.

On cross-questioning the mother a few days after relative to what she considered as having been the cause of having produced the premature labour, she stated that for about ten days back (before the birth of the child), she had been suffering from motions resembling those of the child in utero, but higher up in her abdomen, and which were accompanied by pain, and, at the same time, that these movements caused the movements of the (first) child to come on with violence. That position relieved her, and only upon first lying down did they come on, and that gradually the motion in the upper part of the abdomen got worse, and that to the combined motion and pain she attributed the miscarriage.

It must be here remembered that, up to this time (1868), the mother does not know of the second fœtus.

Therefore I would beg to ask the opinion of the Society whether they would consider the *quickening* of the second fœtus to have been the cause of this premature labour, taking into consideration the relaxed condition and general unhealthy state of the mother, the advanced state of the fœtus first expelled, and the apparent previous liability of the mother to premature confinement.

Supposing the case to have been one of twins, where one fœtus was less developed than the other, might not the want of room for further development have created an excess of motion in the smaller child, and again thus have led to a like result, viz., premature labour. But I myself consider the case to have been one where one ovum was impregnated previous to the other, and therefore of true superfœtation.

Dr Wilson was inclined to think that the case just related was one of twins, in which one of the children had died, and not a case of superfœtation. He thought that the account of the second fœtus was defective.

Dr Sidey was of the same opinion.

Dr Keiller took the same view of the case. Many cases of a similar kind had been recorded. It often happened that one child died in utero, and was retained, while the other went on to the full time. When the liquor amnii is retained, the dead child may be kept in perfect preservation for a very long time. He had a number of preparations illustrative of blighted twin cases in his museum. The blighted fœtus usually presents a wizened or squashed appearance. He remembered a case which occurred in the Maternity Hospital, where the blighted fœtus was not discovered for a day or two after the delivery of the other child. On examining the placenta, a small clotted mass was observed attached, and, on raising the membranes, the blighted twin was found. Most of the so-called cases of superfœtation were of this nature. In true superfœtation, both children are at the full time, and the one is expelled a long time before the other. Such cases are very rare, but they may occur. Dr Matthews Duncan, in his paper on the subject, shows that the spermatozoa may reach the ovule for three months after impregnation has taken place. The decidua does not obstruct the passage of spermatozoa in the early months. The old notion was that the mucus plug prevented their passage. Another explanation of superfœtation is the existence of a double uterus. The chief difficulty in the matter, however, is that during pregnancy there are no ovules to impregnate. Scanzoni at one time held that ovulation went on after impregnation had taken place, but he has now changed his opinion.

Dr Cuthbert had met with a case similar to that related by Dr Aitchison, in which one of the children had died and the other was born healthy. The dead fœtus was withered and shrivelled.

II. CASE OF OVARIAN CYSTIC TUMOUR. BY DR JOHN MILLAR.

This case will be reported in a future number of the Journal.

Dr Macdonald said he would be glad to believe, if he possibly could, that the bromide had achieved this cure, but his own experience, and that of others in the use of the drug, tended to show that its deobstruant properties were not such as to cause the absorption of an ovarian tumour. Its removal, he thought, was more likely to have been effected through some inflammatory action in the tumour.

Dr David Gordon said the general opinion was that solid fibrous tumours might be absorbed; and, if that was the case, why should not fluid tumours also disappear?

Dr James Young said that just lately he had met with a curious case, in which about a week after confinement a large swelling of the size of a child's head made its appearance in the region of the stomach, but towards the left side. He could not make out its nature; and Dr Thomas Keith, who was asked to see the patient, formed an unfavourable opinion of the case. He thought it was connected with the spleen. The bromide was given, and in five or six weeks the swelling began to diminish, and, in the course of other four weeks, all trace of it had disappeared.

Dr Sidey alluded to the great difficulty of finding out the true action of remedies, and mentioned a case of fibrous tumour of the size of a child's head, which disappeared entirely after two years without any treatment whatever.

Dr Keiller agreed with Dr Sidey's remark on the difficulty of arriving at satisfactory results as to the action of medicines. He thought the bromide an admirable remedy in many cases, such as epileptiform convulsions, but could not believe that it could cause the absorption of an ovarian cyst. He had known cases in which as great a time had elapsed after tapping before the fluid re-accumulated as in Dr Millar's case.

Dr Burn had a case of fibro-cystic tumour, in which it was proposed to inject iodine, but inflammation seized the tumour, and it all but disappeared.

Dr Bryce thought the remarkable thing about the case just related was the fact, that when the bromide was stopped, the tumour increased in size, and *vice versa.* He thought the bromide was an uncertain remedy, but he had often seen benefit from its use in spasmodic asthma, epilepsy, etc.

SESSION XXVII.—MEETING IX.

25*th March* 1868.—Dr BURN, *President*, in the Chair.

Dr M'Laren, Lasswade, was admitted an Ordinary Fellow of the Society.

I. ON RETROVERSION OF THE GRAVID UTERUS. BY DR MILNE.

This paper will appear in a future number of the Journal.

Dr Keiller remarked that Dr Milne took the view now generally held as to the cause of retroversion. It was formerly believed that distention of the bladder was the cause of the displacement, but now it was known that the distention of that viscus was usually the result of the retroversion. An over-distended bladder does, however, sometimes cause displacement of the uterus. It is not usual to have retroversion in first pregnancies. The view held by Dr Tyler Smith is that retroversion does not come on after impregnation, but has previously existed. Retroversion of the gravid uterus is most likely to occur where the pelvis is large, and sometimes comes on suddenly as the result of straining or over-exertion. The cases which had come under Dr K's. observation all occurred in women who had previously given birth to children. The first occurred some ten years ago, in a female who had had one child. She was suffering from retention of urine, and did not believe herself to be pregnant. Dr K. found, on examination, a tumour in the vagina, pressing down. He stethoscopized the abdomen, but failed to discover any sound, and it was this case that first led him to think of using the intra-vaginal stethoscope. By introducing an ordinary stethoscope into the vagina, and applying it to the uterus, he discovered a murmur. He replaced the womb, and the woman went to the full time. Another case he had seen with Dr Bruce, in which he got the thumb into the cervix, and with the fingers behind pressed up the fundus. In a third, the woman was brought to him in a cab, suffering greatly from retention of urine, and in her case the displacement was remedied by placing the patient on her hands and knees, and she was able to walk home. Retroversion of the gravid uterus is nevertheless sometimes attended with great danger, and death may result from rupture of the bladder and peritonitis.

Dr Messer was of opinion that retroversion was much more common than most practitioners supposed. He had himself met with three instances. In one, the pregnancy had advanced to about the fourth month; there was no difficulty in making water, but the patient complained of much pain in the back. In this case the fundus of the uterus went up with a jerk, and did not come down again. In the other two cases the displacement got right without any interference. He thought the most common cause of retroversion was a lax state of the vagina and ligaments; and, as regards treatment, he was of opinion that the less one interfered the better.

Dr Cappie said he had at present under his care a case in which retroversion had occurred for the third time. He did not interfere, and the patient had gone to the full time. All he did in the way of treatment was to regulate the bowels and restrict exercise.

Dr Burn stated that he had met with cases of retroversion, but they did not require much interference, quiet and rest being all that was necessary. He had never met with any cases of retroversion in the unimpregnated state.

Dr David Gordon remarked that since retroversion caused so little disturbance usually in the pregnant state, how much less inconvenience must such displacement cause in the unimpregnated uterus!

II. *Dr Fraser* read "Note of a Primitive Mode of Aiding Labour," from Carver's Travels in North America, published in 1807. The method alluded to consisted in the application of a handkerchief round the patient's neck till strangulation was nearly produced, and this was believed to increase the expulsive power of the pains.

Dr Keiller thought the whole secret of the success of this treatment lay in forcing the woman to close the glottis, a practice which was had recourse to every day.

Dr Macdonald believed the mode alluded to was analogous to the custom of giving snuff to women in labour, which was common among the lower classes.

Dr Burn said that patients themselves often imitated this mode of hastening labour by burying their faces in the pillow.

Part Fourth.

MEDICAL NEWS.

ROYAL INFIRMARY OF EDINBURGH.—FUND FOR REBUILDING THE MEDICAL HOSPITAL.

The Medical House of the Royal Infirmary dates from 128 years ago. It is a substantial edifice, and was built according to the most approved principles of hospital construction known at that period. For some time past the building has exhibited many signs of decay, requiring annually extensive repairs; and even admirers of the old house could not fail to perceive that the size of the wards, the ventilation, and the internal arrangements generally, were far below the level of the requirements of the present day, and were certainly not suited for a metropolitan Hospital connected with a great Medical School and University. Accordingly, by the advice of their architect, and with the entire concurrence of the Medical Officers and the Contributors, the Managers determined that the old House should be pulled down and an entirely new Medical Hospital, with all the modern improvements, should be constructed. After considerable discussion, it was decided to build the new Hospital on the present site; but in order to obtain more space and ventilation, the houses situated between the Infirmary and the front of the University are to be bought up and removed. The estimated cost of these changes amounts to £100,000, and as the Infirmary is entirely supported by voluntary contributions, it has been necessary to appeal to public benevolence. Fortunately the managers had already, by prudent arrangements, accumulated a building fund of £40,000, leaving a sum of £60,000 to be derived from public subscription. On the 17th April last, an influential public meeting was held in the Music Hall, under the presidence of the Lord Provost, at which the claims of the Infirmary for liberal support were brought forward and most warmly responded to. Owing to the enlightened and energetic action of the Building Committee, and especially of their Convener, Mr T. J. Boyd, subscriptions to the large amount of £25,000 were announced at the meeting, comprising eleven donations of £1000 each. Before the close of the meeting this sum was raised to £27,000, making with the £40,000 in the Managers' hands, previously mentioned, a total of £67,000. Subscriptions are still being actively collected by the Committee, and there can be no doubt that if all the friends of the Institution are proportionately liberal, the entire sum required will soon be contributed.

It is very gratifying to find that the claims of a charitable Institution, noble and catholic in its character, and, in a medical point of view, of national importance, has received such generous recognition; and we doubt not that subscriptions will continue to flow in not only at home, but also from our colonies. Wherever Scotchmen are found, they look with gratitude upon benefits which the Infirmary has bestowed on the community, and with pride upon the celebrity it has acquired as an Institution for the promotion of medical science.

NAVAL MEDICAL DEPARTMENT.

Verb. sap.

We commend the subjoined leading article, from the Army and Navy Gazette of the 11th ult., to the thoughtful consideration of the profession; and more especially would urge its attentive perusal upon the students of those schools that have of late furnished, rumour says, only too many sorry recruits to the Royal Navy. Although deeply regretting that the cause should still exist, we have been glad to know that, except in three or four instances, Scotch students have been sufficiently wise and high-spirited to keep away from Whitehall and Somerset House, and that too despite the witching tongue of the semi-officially inspired Purser, whose patriotic labour of love ("Everything about them") Messrs Edmonston and Douglas brought out in their usual neat style some eighteen months ago.

We certainly hoped for better things, for a more far-seeing policy, if not for more justice or generosity, from the present Board of Admiralty; and now only look forward to the day, not far distant, when a reformed Admiralty will end the reign of misrule that has continued so long at Whitehall.

"From what we hear in naval medical circles, where the affair is spoken of with much indignation, the case of Dr John Stirling, late surgeon of the naval hospital at the Cape of Good Hope, who has been summarily dismissed from his office by Commodore Randolph, is very hard, and it may be fraught with incalculable evil to the service, if it be allowed to stand as a precedent. It seems that Dr Stirling ventured to assert it 'to be his place to judge of the necessity of more frequent or rarer visits to his patients at a distance from the hospital,' and was, in consequence, deprived of his post on the following day by the Commodore. We understood that medical officers sent by the Admiralty to occupy posts abroad are always selected for long service and tried ability; and that the Admiralty are jealous of any unnecessary interference with them in their proper places. They have on that account withheld from commanders-in-chief and senior naval officers the power to disturb such arrangements without special authority from the Board, and in each case retained the patronage of these appointments, even when death vacancies happen. We see here a case, however, in which a surgeon is summarily dismissed, and discharged to the shore in England. This case demands searching investigation, and due inquiry should be made into the conduct of an officer who seems to have dispensed altogether with the legal forms of restraint instituted by his superiors. It is certain that if Dr Stirling had been a junior assistant-surgeon of the Seringapatam, which bears Commodore Randolph's broad pennant, he could not have been treated in this manner by the greatest martinet in the service. A court-martial, at which he would have been defended before sentence, would have been his undoubted right. That being the case, we ask, in the name of common sense, if it be right, if it be legal, or if it be just, to the service, that an officer, trusted by the Admiralty, in a position of high responsibility, should have been thus subjected to the arbitrary caprice of his superior officer without the right of appeal, or, at least, of such an appeal as might ensure speedy justice to him? There is a gnawing at the core throughout most of the so-called civilian branch of the Navy, because they believe they never receive fair play in any collisions with the executive departments. Statements of the most prejudicial nature are, it is well known, transmitted in darkness to the Board of Admiralty which the civilians have no chance of disproving or of refuting. Thus they suffer without the hope of redress—all inquiry being stifled in order to maintain the discipline of the service by screening the executive branch from the civil consequences of its acts. The time has come, we think, for an alteration, and we shall be glad to see the day when, as a matter of regulation, all statements shall be made as public and above board as if the officers complained against had enjoyed the naval right to court-martial before condemnation. We are pained to see the grievances of medical officers becoming again the topic of the day in the medical press and schools, but we trust that the Admiralty and War Office will be warned in time, for we cannot but think that the cases of Drs Stewart, Mouat, and Stirling will have their weight in preventing the entry of gentlemen into the medical services. The Admiralty have abolished 'gagging in the Navy,' as soon as a death through it shocked the public mind, and we hope that, with equal alacrity, they have reprobated the conduct of Commodore Randolph, that they are prepared to remove the gag from officers as well as men, by giving publicity to the whole circumstances in cases like this of Dr Stirling; and it is not too much to hope that some member of the House of Commons will ask for the production of all the documents relating to this remarkable case, and of the instructions under which Commodore Randolph has acted, in summarily superseding Dr Stirling from the appointment in which the Admiralty had placed him. In the meantime, their Lordships cannot fail to be seriously annoyed at the existence of such a scandal as a wrangle between officers of such rank as Commodore Randolph and Dr Stirling is admitted to be, and which might have been easily avoided, if the senior officer had taken the wise precaution of sleeping over the matter."

INTERNATIONAL MARITIME MEDICAL CONGRESS AT HAVRE.

During the International Naval Exhibition, which is to take place at Havre in 1868, under the patronage of H. M. the Emperor of the French and H. R. H. the Prince Imperial, it is proposed to hold a maritime medical congress, in reference to which we have received the following communication, which we translate:—

" *To the Editor of the Edinburgh Medical Journal.*

" Havre, 47 Rue Royale, 26th March 1868.

" Sir,—Being charged by the Committee of Organization to make preparations for the meeting at Havre of an International Maritime Medical Congress, I beg you will grant me the assistance of your journal in order to request such of your readers as take an interest in Naval Hygiene, 1*st*, That they will send in their names as members of the Congress; and, 2*d*, That they will intimate to me the questions which in their opinion ought to find a place in the programme of the proceedings. I have the honour to be, etc.

" Dr Auguste Durand."

We cordially recommend the Congress to the attention of our readers. The questions of the health of sailors, both in the Royal and in the Mercantile Navy, have been carefully studied by our naval medical officers, and much attention has been recently directed to the subject by the Reports of Dr Walter Dickson and others. We trust therefore that the profession in this country will take an active part in the proposed International Congress.

APPOINTMENT.

G. H. Philipson, M.A., M.D., has been elected one of the Physicians to the Newcastle-upon-Tyne Infirmary, vice D. B. White, M.D., deceased.

An Invitation to the Doctors by the New York Medical Society.—Doctores! Ducum nex mundi nitu Panes; tritucum at ait. Expectu meta fumen tu te & (*sic*) eta beta pi. Super attento uno Dux, hamor clam pati; sum parates, homine, ices, jam etc. No Sideror Hoc. *Festo resonan floas sole.*—Nov. Ebor, Jan. 20.

PUBLICATIONS RECEIVED.

Charles Bader,—On the Human Eye. London, 1868.

Edward Ballard, M.D.,—On Vaccination. London, 1868.

Frederic Bateman, M.D.,—On Aphasia. Lewes, 1868.

G. S. Bedford, M.D.—On the Principles and Practice of Obstetrics. New York, 1868.

R. Beveridge, M.D.,—On the Statistics of Typhus in Aberdeen. London, 1868.

E. L. Birkett, M.D.,—Thomson's Conspectus. London, 1868.

W. J. Collins, M.D.—On Vaccination. London, 1868.

Austin Flint, M.D.,—United States Sanitary Commission Memoirs. New York, 1867.

Dr F. Frankenhaeuser,—Die Nerven der Gebaermutter. Jena, 1867.

History of Anæsthetics from an American Point of View. Edinburgh, 1868.

Messrs J. W. Ogle and T. Holmes,—Catalogue of the Pathological Museum of St George's Hospital. London, 1866.

David Prince, M.D.,—Plastic Surgery. Philadelphia, 1868.

Report on Leprosy by the Royal College of Physicians. London, 1867.

T. G. Thomas, M.D.,—Practical Treatise on the Diseases of Women. Philadel., 1868.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*An Account of Three Cases of Congenital Cataract in One Family, cured by Operation.* By BENJAMIN BELL, F.R.C.S.E.

THE following cases seem worthy of being recorded, both because it is not common to meet with so many in a single household, and because in one of the individuals there was a good opportunity for observing the phenomena which follow the operation when there has been no previous perception either of external objects or of colours.

Isabella, William, and Charles King, the children of a respectable labouring man in the parish of Lochmaben, were sent to me by a benevolent lady in their neighbourhood, to have the operation for cataract performed upon their eyes. I was informed at the same time, that their mother, now dead, had been operated on at the age of fourteen for the same complaint with considerable benefit, and that an older daughter living at home, and twenty-five years old, has very defective vision from the same cause with the rest of the family.

The eldest of these three young persons was born with defective sight, but could see tolerably well until she was six years old, when the symptoms became greatly aggravated. She is now seventeen, and, although evidently not very robust, enjoys good health. She can find her way at home pretty well; but is quite incapable of following any employment. The right eye, which is the more serviceable of the two, has a large anterior chamber, indicating absorption of the lens; and the pupil, which dilates freely under *atropia*, is occupied by the remains of a capsulo-lenticular cataract, reflecting little light, and not very readily observed. The left pupil also dilates freely, and is occupied by a large soft cataract of milky appearance. As this eye seemed more favourable for surgical interference than the other, I resolved to operate upon it, and to reserve the right eye, if necessary, for a future occasion.

I accordingly penetrated the *cornea* with a needle having a fine edge, and cut across the front of the *capsule*. A considerable portion of the *lens*, which was fluid, passed into the anterior chamber. In addition to the horizontal incision already made, I then divided the capsule vertically. These two incisions formed a considerable opening; and the patient said that she now saw much more light than previously.

The eye was closed, and a piece of cotton wool applied over the lids, retained by a bandage.

William, a lad of fifteen, could see pretty well for several years after his birth; but the cataracts became complete even at an earlier period than in the case of his sister. For a number of years he has been unable to distinguish objects. The pupils respond to variations of light. They are occupied by cataracts, which, being very white and flat on the surface, appear to be mainly capsular, the lenticular substance probably in great measure absorbed.

The same operation was performed on both eyes as in the preceding case. The diagnosis turned out to have been correct. The opaque capsules were less tough and intractable than they often are, and yielded readily to the knife, so that a good-sized aperture was formed in the centre of both.

Charles, a smart boy of thirteen, has never perceived *any object, nor any colour, even the brightest.* Both pupils, which dilate readily under atropia without any improvement to his vision, are occupied by white cataracts more distinctly capsular than in the case of his brother. In the middle of the right capsule, there is a round portion about the size of a small pin-head, of a yellow hue, and suggesting the idea of its being cretaceous.

The same operations were performed as in the case of William, and with a similar result.

All the three young persons were operated upon the same day, December 19, 1867, and no untoward event occurred to any of them.

I removed the bandages four days afterwards (23d), and made the following note with regard to the several cases.

The girl can tell the hour on a watch with her left eye, on which the operation was performed. There is a good central opening of the capsule, although some flocculent portions of the lens are still to be seen in the lower part of the anterior chamber.

The elder boy, on being taken to the window, named several objects on the street,—such as a man, cart and horse, with correctness.

The younger boy was very carefully examined, from the circumstance already mentioned, that he had never seen any object or colour before the operation.

On presenting a red leather spectacle-case, and asking him what it was, he replied that he did not know what it was, but that the colour was red. This seemed almost inexplicable. It was ascertained, however, that before my visit and the formal taking off of the bandage, his sister had shown him his own *neck-comforter*, which consisted of alternate portions of white and red worsted, and, being a smart little fellow, he knew the red colour when he saw it again. When I presented various objects before him and desired him to lay hold of them, he did so without hesitation. As this seemed remarkable at so early a period, I was at pains to learn from his sister the exact circumstances which occurred when an object was first placed before him. She said that, holding up a book, she asked him what it was; that he replied—"I see it, but don't know what it is;" that she desired him to take hold of it, which he did by putting forth his hand like any other person. I asked—"Did he not put his hand to his eyes?" She answered —"No!" and the boy gave the same account of the matter, being obviously a little affronted at my supposing him capable of doing anything so foolish. I may mention that, before he came to town, he had been learning to read with his fingers, according to Mr Moon's system.

Five days (28th) after the foregoing observations, as the openings in the capsules were little more than narrow chinks,—although he was evidently making rapid progress, looking at and handling all manner of things during the intervals of my visits,—I made another attempt to enlarge the aperture in the right eye; and on the 30th the same was done upon the left. These operations were both satisfactory in their result, and there seems to be no occasion for farther interference.

January 3, 1868.—I took him to the window and asked him to describe what he saw. He said that the ground was white, and that he could see the snow-flakes falling. There was a shower falling at the moment.

I am happy to say that all these young persons have returned home possessing useful vision for most occasions, without the aid of glasses; and that, by means of convex lenses, they are able to distinguish ordinary print, such as that of the *Scotsman* newspaper. The younger boy has yet to learn the art of reading; but I have no doubt that this will be speedily accomplished.

Before concluding, a few observations may be expected upon the peculiarly rapid manner in which Charles King acquired visual perception.

There can be no doubt, I think, as to the soundness of the common doctrine expounded by metaphysicians, that visual perception takes cognizance of *extension* and *colour*, and is in fact principally conversant with these phenomena. But it would be an error, I imagine, to assert on the other hand, as some have done, that the sense of touch or tactual perception is altogether incompetent by itself to impart the conception of extension and space. An infant, who possesses both senses in a normal degree, attains very gradually the power of laying hold of objects with precision and promptitude; and it can hardly be doubted that, in these circumstances, the knowledge of extension, and space, and form, *implied in the act of prehension*, depends upon both sight and touch mutually aiding and correcting each other.

When the sense of sight is in abeyance, is the idea of extension or of space impossible to the individual so unfortunately placed? We see no sufficient reason for believing this. Numerous instances will occur to every one of persons blind from birth, who were constantly giving proofs that the ideas of extension, of space, and of form, had been realized by them. Charles King, for example, besides being an active boy in moving about, had learned to read with his fingers by means of raised characters; and Mr Nunneley, of Leeds, mentions a similar case, a boy of nine, who, before the operation for congenital cataract, if he happened to be walking along the middle of the road, when he heard any object approaching, at once stopped, groped his way to the side of the road, and remained perfectly still until it had passed.

But, admitting that the ideas of extension, space, and form, may be acquired through the sense of touch, although less completely than through the sense of sight, it is still undoubtedly true that, if the power of seeing be acquired for the first time by one who has passed the period of childhood, there will be some hesitancy in rightly interpreting the visual perceptions, so as to harmonize them with those previously communicated by the organs of touch. The interval of hesitancy will doubtless vary in length, according to the mental activity and intelligence of the individual, the success of the surgical operation, and the sensibility of the *retina*.

The case so well described by Mr Cheselden, in 1728, and so often quoted by the metaphysicians in confirmation of their doctrines; other subsequent cases; and particularly the one already referred to, which occurred to Mr Nunneley, show that, as a general rule, a *temporary difficulty* is experienced. In the case of my patient, Charles King, this difficulty must have been overcome more rapidly than usual, and might indeed be almost ignored entirely, were it not that its existence is rendered highly probable, both by the history of previous cases, and by *à priori* reasoning.

I am inclined to find an explanation of this specialty, in his intelligence and mental activity, which are certainly above the average. In confirmation of this view, and before concluding this short paper, I adduce the following case, in which the individual, as will be seen, was comparatively stupid and dull of apprehension. The case occurred in the practice of my late uncle, Mr George Bell, of this city, and is taken *verbatim* from his note-book.

"In the year 1802, the late Countess of Leven placed under my care a young woman, æt. 19, from the town of Leven. She had cataract in both eyes; could distinguish no objects—only light from darkness. She had no headache. Her eyes looked in other respects perfectly well. The *iris* contracted and dilated as in health, and she was otherwise well, so that she appeared a fit subject for an operation. Being extremely anxious to verify the account given by Mr Cheselden in the Transactions of the Royal Society, I mentioned the subject to the late Professor Dugald

Stewart, Professor Playfair, and my friend Dr Thomas Brown, who visited her two or three times, and were present at the operation, when the cataract was couched in the left eye, in January 1802. Having, however, found upon the previous visits, that the girl had formed no idea from the organs of sight, excepting that she was able to distinguish a bright light from total darkness, and that she was *unable to return distinct answers to the questions which were put to her*, these gentlemen did not think it necessary or worth their while to repeat their visits after the operation. The very circumstance, however, of the *want of acumen* upon her part, which appeared to all of us to be so unfortunate as to prevent any information being got from her, proved to be in her case the most fortunate for the verification of Cheselden's experiment.

"Upon taking her to the window, at the end of the fifth day, as I was unwilling to expose her to a strong light until that time, the eye seemed in a natural state. The lens had been soft, and was either absorbed or quite out of sight. She could distinguish light from darkness, or any opaque body moving between her and the light. She could not, however, distinguish either colours or separate bodies distinctly, so as to give them a name; but upon being desired to take hold of a watch placed at about the distance of a yard from her eye, and which she said she saw moving between her and the light, she made several attempts before she could seize it, first taking hold of her own nose, and on the fifth or sixth attempt laid hold of it. She thus proved, in my humble opinion, without any leading questions being put to her, that she conceived a body which she saw touched her eye,—which she might have been induced to have done had any leading questions been put to her in that direction, without any intention on her part to deceive, but from not understanding their import."

ARTICLE II.—*Case in which Fifteen Intestinal Concretions were passed, with Complete and Permanent Relief, with Remarks.* By ROBERT PATERSON, M.D., F.R.C.P.E., etc., etc.

FRANCIS LAWSON, æt. 16, has been an invalid, and more or less under my care for the last eight years. His ailments began with an attack of scarlatina, eight years ago, previous to which he had been a very robust, healthy boy. The attack of scarlatina was severe, and was followed by dropsy, which was unusually obstinate. He had hardly thoroughly recovered from his dropsical attack when he was seized with ileus, a disease not easily accounted for in him, as he was a child who never suffered from constipation, and who was dieted with the utmost care. He was treated perseveringly with opium, and the disease ultimately yielded to this treatment. But his sufferings still continued, for repeated attacks of ileus, or iliac colic, closely followed upon the first, and, upon an average, it may be said that an attack occurred every fourteen days. These attacks were sometimes accompanied with complete obstruction, sometimes not. The abdomen was frequently and carefully examined at this time, but no hardness of any kind could be detected. Thus five years of misery, characterized by repeated attacks of colic, by want of appetite, by languor, by sleeplessness, and by various other kinds of suffering, passed over this child, and yet no change occurred. He had grown, but was still under size; he was emaciated, dull, and wretched. About this time, on examining the abdomen, a doughy hardness in the region of the caput cæcum could be detected; it was of no size, but, when pressed upon, felt tender. Three years ago, and not long after the detection of this tumour, he was seized with a more severe attack than usual of colicky pains of the abdomen; his sufferings were intense, not being confined to the seat of the tumour alone, but extending all over the abdomen. During these agonies, he was seized with epileptic convulsions, recurring with great force, and lasting for twelve hours.

Simultaneously with the passing away of the convulsions, relief to the symptoms of ileus occurred, and little time elapsed ere he passed an ordinary sized plum-stone.[1] It was naturally concluded that the passage of this foreign body, the impaction of which had caused such prolonged suffering and danger, would at once rid him of those continually recurring attacks of colic or ileus; and it did so for a time, during which he was removed to the country to try and recruit his emaciated and enfeebled frame. It was while residing in the country, and while he was comparatively free from suffering, that some friend of the family strongly urged upon his mother, to resort to the use of enemata of oatmeal gruel and cod-liver oil. The gruel was made stronger than ordinary, and to a pint of it two tablespoonfuls of cod-liver oil was added. These enemata were repeated every night and every morning, and they generally remained twelve hours without any action. The relief which these enemata afforded was great, and the boy began to enjoy good health, to be out, take exercise in the open air, and to put on fat. These favourable symptoms continued for nearly four months, during which time he had these enemata morning and evening; but again unfavourable symptoms of the old type appeared, and attacks of pure ileus and colic set in as before. These now and again assumed a characteristic form, from which any one not familiar with the course of the case would have prognosticated a fatal issue. Meantime the tumour in the region of the caput cæcum had greatly increased in size, and become much harder and more tender to the touch; yet it was not from this that the colicky pains emanated; they seemed often to begin at the stomach, and with much flatulent noise to spread over the bowels, occasioned in their progress great distress. During the latter part of his illness, these symptoms were almost continuous, lasting often for twenty-four hours without the slightest relief.

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[1] The convulsions in this case, were most probably attributable to the state of the kidneys, the urine having continued more or less albuminous ever since his attack of desquamative nephritis; and it was remarked by his parents at the time that no plum-stone could possibly have been swallowed by him for a twelvemonth.

On Christmas eve, 1867, he suddenly obtained relief by the passage of four calculi; these were of a brown colour, with a soft, velvety coat, and indented as if there had been more from whence they came. I saw the boy the same evening, and carefully examined his abdomen; the tumour of the cæcum was still there, but on manipulating it carefully, the feeling of a number of hard bodies rolling and rubbing on each other was so distinct as to lead me to tell the parents that there were more calculi to come away.

The relief which the passage of the first four occasioned was not of long duration, and was succeeded by a repetition of the same symptoms of even a more severe character; but his final relief was at hand, for, on the 1st of April, 1868, he passed eleven calculi of the same character as the former, but, generally speaking, all of larger size. The relief has been perfect and enduring; he now sleeps more, eats well, and is again taking exercise. The tumour in the right iliac region has entirely disappeared, and there is no tenderness even to be felt. The boy says, that while formerly he could never stretch himself quite straight without being caught with a pain or uneasiness there, he no sooner passed the calculi than he could stretch himself out without any pain whatever.

This case was seen during various stages of its progress by Dr Christison, Dr Begbie, and more than once by Dr Warburton Begbie.

The calculi, fifteen in number, had all the same brown, velvety exterior, with flattened facettes, as if they had been pressed upon by other calculi; still their velvety surface had not been removed, and there was no polishing by rubbing or otherwise. They weighed from 9·07 grains to 30 grains and 81 grains; and the largest weighed 150 grains. These calculi, as we shall find in the report of Dr Gamgee annexed, are lighter than water; their bulk, therefore, is much greater than might be inferred from their corresponding weight. Thus the largest calculus weighs 150 grains; it is a large stone, like a small hen's egg, and measures $3\frac{3}{4}$ inches in circumference.

The report of their chemical constitution by Dr Gamgee, addressed to Dr W. Begbie, I shall now quote:—"The calculi were of a brown colour, and exhaled a decidedly fæcal smell; their density was found to be only 0·709 (water being 1), so that they floated readily when placed in water. The calculi could be pretty readily cut with a knife, and their section was of a brown colour, and presented a hairy appearance. There was no appearance of a central nucleus. On a microscopic examination, it was found that the calculi consisted essentially of interwoven fibres, and some amorphous brown colouring matter. The fibres were unmistakably those of the caryopsis of the oat, and the cells and spiral vessels were derived from the envelopes of the oat. These points were made out by actual comparison with the oat. The calculi were almost unaffected by boiling with water, dilute hydrochloric acid, or alcohol. Ether removed, however, 5·37 per cent. of fatty matter. The brown colouring matter could only be removed by treating the calculi first with hydrochloric acid and then with chloroform; a brownish solution was then obtained. The colouring matter was obviously of the same nature as that often found in combination with lime, in certain biliary calculi. The concretions contained in addition a very small quantity of earthy phosphates and silica.

(Signed) "ARTHUR GAMGEE."

Remarks.—There can be little doubt, after the detailed analysis of Dr Gamgee, that the origin of these concretions was the use of oatmeal, in some way or other introduced into the alimentary canal. In this case it was introduced in two ways, in the shape of oatmeal porridge, of which he always partook of a small saucerful every morning, and of enemata of oatmeal gruel, which he had administered to him for four consecutive months, night and morning, and retained generally for twelve hours. Now, from which of these sources did the concretions take their origin? It appears to me that we must decide unequivocally for the latter. It will be observed, in perusing the case, that symptoms of ileus occurred long before the enemata of oatmeal gruel had been administered; these symptoms were repeated, and severe, and terminated for a time, after a more than usually painful attack, by the passage of a plum-stone, after which he had comparative relief. It may be observed, regarding this plum-stone, that it was passed without anything covering it, of which it might be said to be the nucleus. In fact, the moment it was passed, it was recognised by the patient's parents as unmistakably a plum-stone. I consider from this, that had the origin of these concretions come from the oatmeal introduced into the stomach, we should naturally have had this plum-stone encrusted with the material of which these concretions were composed; but it was otherwise; and looking to the history of the case, the relief after the passage of the plum-stone, and the gradually increasing sufferings of a similar kind to those he had experienced before in his attack of ileus and colic, I think we must undoubtedly refer his second set of iliac symptoms to the concretions which were formed from the repeated enemata of oatmeal gruel and cod-liver oil. Being curious to know the effects of mixing these two ingredients together, and allowing them to rest at a moderate temperature, I have experimented in several ways. I have kept the mixed fluids completely at rest, and I have kept them in partial motion; and although in an experiment out of the body it is impossible to imitate the steady peristaltic movement of the intestines, still my experiments sufficiently proved to me the ready manner in which this combined material, and especially when coming in contact with the fæcal matter of the larger intestines, would deposit, and form a mass of concretions. Thus when thick oat gruel and cod-liver oil are shaken up together, the oil takes up a great proportion of the fibres of the oat, which, although allowed to stand for a long time at rest, do not leave the oil but remain combined with it, and thus we can readily understand how this combination of oil and the fibres of the caryopsis of the oat, especially if mixed with the fæcal matters (biliary and phosphatic compounds) which are to be found in this quarter, would readily concrete and fill the cells of the colon, or caput cæcum itself. Concretions found in the bowels, and passed through them, are familiar to many practitioners in Scotland, and have been described by Mr Torbet and Dr Duncan, by Dr Monro, tertius, and also by Dr Watson in the last number of the *Edinburgh Medical Journal.* They arise from the use of oatmeal as an article of food; "The beard and fibres of the husks of the oat (says Dr Copland) resisting digestion, and collecting together so as to form concretions or nuclei around which saline matter with accessions of these fibres collect. The external layers of the calculus formed from this source are generally solid, compact, and soft to the touch, and composed of saline matter. In other instances, the outer layer has a velvety appearance, and consists of very fine fibres, closely united."[1] "Dr Marcet found these concretions to consist of compact layers of fibrous substances, and of phosphates. The following is his analysis:—In 100 parts, 25·20 was animal matter, 3·90 resin, 5·16 ammonio-magnesian phosphates, 45·34 phosphate of lime, and 20·30 vegetable fibres. The vegetable fibres were cemented together by deposits of earthy matter and the animal matter."

It will be observed that there is a large difference between the composition of these concretions and those I show you, although their external characters were alike; for while, in Dr Marcet's analysis, the phosphates amounted to 50·50 per cent.; in Dr Gamgee's analysis of those before us, he only announces the discovery of "a very small quantity of earthy phosphates and silica."

The large proportion of the calculi in this case was made up of layer upon layer of vegetable fibres, and it is important to remember that there was 5·37 per cent. of fatty matter also. If I am right in my conjecture, that these concretions arose from the enemata of oatmeal gruel and oil, so perseveringly administered to the boy, this important difference in the analysis of Dr Marcet and Dr Gamgee is also satisfactorily accounted for.

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[1] Copland's Dictionary, vol. i., p. 398.

ARTICLE III.—*On Umbilical Hæmorrhage and Tumour.* By R. PEEL RITCHIE, M.D.

(*Read before the Obstetrical Society of Edinburgh*, 27*th November* 1867.)

LAST Session I submitted to the Society a paper on Infantile Hepatic Abscess, and at some length narrated a case uncomplicated with hæmorrhage. At more than one meeting reference was made to umbilical hæmorrhage; and at the last of those, I alluded to a case then under treatment, in which the arrested hæmorrhage was followed by a vascular tumour at the umbilicus. The President expressed a desire that the case should be again brought before the Society, and I am now able to give a favourable report of the treatment and its result. The case was one in which the hæmorrhage had not occurred at the usual time, when the cord separates, but when the infant had nearly reached the age of five months. A peculiarity in the case, too, was that the arrest of the discharge was followed by the growth of a tumour. In this communication I propose to offer some observations on umbilical hæmorrhage and umbilical tumour.

I shall first, then, state with regard to hæmorrhage, that one would have supposed an occurrence of so serious a nature could not fail to attract the notice of medical writers; but it is somewhat remarkable, that previous to the commencement of this century, the allusions to the accident do not appear to be numerous. The result of my inquiries leads me, however, to conclude that hæmorrhage from the umbilicus had been noted before the time of Underwood, to whom the credit of first alluding to it has hitherto been assigned.

Previously to the time of Ambroise Paré, I have been unable to find any reference to the subject. "Les Œuvres de Chirurgie" were published at Paris in 1575. He alludes to the danger which may arise from the cord being improperly tied, and in the "Livre de la Génération," the 74th chapter is entitled, "De la Relaxation et Enfleure du Nombril qui se fait aux Enfans;" but although he refers to tumour and abscess, and the danger of opening these, he does not recognise umbilical hæmorrhage nor vascular tumour following the separation of the cord.

The earliest special work on diseases of children to which I have had access is that of Omnibonus Ferrarius of Verona, "De Arte Medica Infantium," in four books. It was published in Germany for the first time at Leipsic in 1605. The third chapter of the second book is headed, "De Incisione Umbilici." He directs that the "umbilicus" be cut with a sharp lancet four fingers from the belly. In the fourth book, the fourteenth chapter is, "De Inflammatione Umbilici," which he states may arise from ulceration. The fifteenth chapter is, "De Tumore seu eminentia Umbilici," which he considers may happen without inflammation, and is called ὄμφαλου ὄιδημα. He does not mention either hæmorrhage or vascular tumour of the part.

François Mauriceau published the first edition of his "Traité des Maladies des Femmes Grosses," etc., in 1668, and a second edition appeared in 1675. It is evident to me that Mauriceau was aware of the danger of bleeding, not only from a cord improperly tied, but also from the hæmorrhage after its separation;—"car il s'est vû quelquefois des enfans perdre misérablement la vie avec tout leur sang, avant qu'on s'en appercût, pour ne leur avoir pas bien noüé l'ombilic." He observes that the cord is separated ordinarily on the sixth or seventh day, and rarely longer than the eighth or ninth. I think the following case should be rather imputed to the kind of hæmorrhage under consideration than to being merely due to a badly tied cord. He says:—"Car il se rencontre quelquefois que ce cordon est si gros à certains enfans, que bien qu'il ait esté lié fort serré dans le premier abord, néanmoins venant après à se flêtrir et à se dessécher, la ligature en est renduë plus lâche, au moyen de quoy le sang ne laisse pas de s'écouler ensuite, si on n'y prend garde. Cet accident arriva dernièrement à un pauvre enfant, qui mourut le deuxième jour par un tel flux de sang, quoique la Sage-femme m'eût protesté qu'elle luy avoit bien exactement lié les vaisseaux; et s'étonnant comme cela s'étoit pû faire, elle me dît qu'il falloit bien assurément (ce qui en effet étoit vray) que la ligature s'en fût relâchée à mesure que l'ombilic s'étoit flétry; c'est pourquoy afin de n'estre pas cause d'un tel malheur, il faudra le serrer encore d'un nouveau nœud la première fois qu'on remuëra l'enfant, si on juge qu'il en soit besoin."

Upon this case it is almost unnecessary to observe, that the cord withers in all cases, and that this accident, as explained by the loosening of the knot in proportion to the withering of the string, would necessarily be a very frequent occurrence, whilst it is not so. I cannot but conclude, therefore, that this was one of the cases of hæmorrhage from the umbilicus itself. That he was aware, however, of this form of hæmorrhage, is placed beyond doubt by this observation in his twenty-ninth chapter:—"De l'inflammation et ulcération et de l'éminence du nombril des enfans nouveau-nés." "Mais cette inflammation vient pour l'ordinaire (ainsi que j'ay dit) de ce que l'enfant ressentant de grandes douleurs et trenchées du ventre, fait continuellement des cris par lesquels l'ombilic est empêché de se réunir; elle peut aussi estre causée par une violente et frequente toux, d'autant que par ses efforts le sang est poussé dans le bout restant de la veine umbilicale, qu'il tient toujours dilatée, et se corrompant par le séjour qu'il y fait, il ne manque pas de causer inflammation au nombril; et ce qui a esté lié venant à tomber avant que la réünion soit faite, *il y demeure un ulcère très-fâcheux, auquel survient parfois une grande perte de sang et mesme la mort.*"

So much for Mauriceau, to whom, so far as I have been able to trace, the credit must be given of distinctly recording umbilical hæmorrhage and its danger.

The next authority I have been able to lay my hands on is Gaulterus Harris, M.D., Celeberrimi Collegii Medicorum Londinensium Socius atque Censor. He published his treatise "De Morbis Acutis Infantum" in 1689. He alludes to petechial fever and its treatment amongst the Turks by lemon-juice, but makes no allusion either to umbilical hæmorrhage or tumour.

In 1722, Theodorus Zuingerus of Basil gave to "Europe and the Provinces" the "Pædoiatreia practica curationem plerorumque Morborum Puerilium per meras observationes in Praxi quotidianâ factas, clare et distincte exponens." His 112th observation is on "Umbilici Hæmorrhagia, Nabel-Bluten." He says, "Hæmorrhagiam vocant umbilicalem, quâ aliquando tanta sanguinis quantitas profunditur, ut tenelli infantes hinc maximè imbecilles evadant, morituri profecto brevi, nisi convenientibus medicamentis vires mature restituantur, debitoque alimento robur partium amissum revocetur." In treating of the cause of this hæmorrhage, after observing that for the most part it proceeds from a too loose ligature, and before the vessels have closed, he continues,—"vel quando umbilicus utut bene ligatus, vel per se ante dictam coalescentiam delabitur, vel per aliqualem incuriam et vim illatam ablatus, vel ab aliis etiam causis læsus fuit."

The next author, whose work on children I have seen, is Dr W. Cadogan, F.R.C.P. In 1749, he wrote "An Essay upon Nursing and the Management of Children from their birth to three years of age." It is in the form of a letter to a governor of the Foundling Hospital, and was so much thought of that nine editions of it were published. One reason, possibly, why so long a period elapsed without any notice of this disease being taken, in this country at least, may be found in this quotation:—"It is with great pleasure I see at last the preservation of children become the care of men of sense; it is certainly a matter that well deserves their attention, and I doubt not the public will soon find the good and great effects of it. . . . . In my opinion this business (nursing) has been too long fatally left to the management of women, who cannot be supposed to have proper knowledge to fit them for such a task, notwithstanding they look upon it to be their own province." The letter dwells chiefly on the clothing and feeding of infants, and no reference is made to umbilical disease.

In 1767 appeared "An Essay on the Diseases most fatal to Infants," by George Armstrong. In his opening remarks he echoes the views of Cadogan, stating that the province of medicine, which happens to be of the greatest consequence to society, "that which regards the diseases of infants, has hitherto lain uncultivated, or at least been much neglected." "I know," says he, "there are some of the physical tribe who are not fond of practising amongst infants, and I make no scruple to own that I was of that number myself till within these two or three years, that I may venture to say I have discovered a more successful method of treating their complaints." The diseases we are considering had either not come under the observation and treatment of this writer, or he did not consider they were of the class most fatal to infants, for he makes no reference to them. He remarks their diseases are fewer than those of adults, "for, excepting what the nurses call inward fits, teething, and the rickets, I do not recollect any particular disorder that infants and children are subject to which adults are exempted from."

"An Observation on the Management of Children at the time of birth," was published anonymously in London in 1775; it refers to the division of the navel-string; but although the author commences thus—"It hath been a matter of the most serious consideration to those who have had the care of very young children to see so great a number born dead, or die after an imperfect existence of a few hours or days,"—he does not allude to hæmorrhage after the falling off of the cord as a cause of death or danger.

The authority I shall next quote is Christopher Frid. Daniel. In 1780, he published at Halla his "Commentatio de Infantum nuper natorum Umbilico et Pulmonibus." About this time it seems to have been questioned whether the umbilical cord should or should not be tied. In this controversy Cl. Fischer appears to have occupied a prominent part, and to refute his opinions is the object of Daniel's commentation. He incidentally alludes to the subject we have under consideration in his ninth section—"Respond. ad Fischeri argum. ii. Ex morbose constituto sanguine hæmorrhagiam sequi per funiculum umbilicalem non deligatum quo modo probat v. cl.? Quod aliae etiam observentur hæmorrhagiæ in pueris atque adultis! Observationes adductæ ita sunt comparatae ut nullam omnino vim habeant. Namque si tardius secuta est ex umbilico hæmorrhagia ut in exemplo Bartholini, non spectat ad fetum jam natum, si post annum tumor in umbilico ortus, ut in exemplo, a Mauriceau adducto aut tumor ortus est die septimo, ut in Wagneri exemplo, aut frictione die septimo producta est hæmorrhagia ut in Krameri exemplo, tum aliunde de vita infantis et mortis caussa constat, atque tumor in cadavere exstabit."

I have been unable to find the passage referred to either in Bartholinus's works or in Mauriceau, but I hold that, by the passage I have already quoted from Mauriceau, it is sufficiently evident that death, as the result of umbilical hæmorrhage after the separation of the cord, was recognised by him as at least an occasional cause of death.

Turning next to the "Aphorisms composed for a Text to Practical Lectures on the Constitution and Diseases of Children," by Dr Andrew Wilson, F.R.C.P.E., published in 1783, I find the idea of Cadogan repeated, in other words, that it has been a long-rooted general opinion that has too often been countenanced by even men of character in the profession of Medicine, "that physicians can know little of the maladies of infants, and that nurses are their best doctors." He states that "the first diseases that affect them are, (1.) an unnatural retention of the meconium ; (2.) the gum ; (3.) an appearance of the jaundice ; (4.) aphthæ." But he makes no reference either to umbilical hæmorrhage or tumours.

Dr Michael Underwood, who follows next in order, has a section, in his "Treatise on the Diseases of Children," on "Hæmorrhage from the Navel." I have been unable to see an earlier edition of his work that the fifth, published in 1805.[1] He does not, however, appear to have been aware of that form of hæmorrhage which, in the present day, is regarded as almost invariably fatal. Thus, he observes (vol. ii. p. 121), "the navel of new born infants is liable to several disorders, some of which are of considerable importance ; but I speak in this chapter only of the slighter ones,—of these, one is an oozing of blood from the part after an unkindly separation of the cord, and is owing to the shooting up of a soft fungus which prevents the skin from covering the divided vessels in the manner it otherwise does." From this statement he would appear to have been cognisant of the tumour, but regards it as preceding rather than succeeding to the hæmorrhage. From the facility with which such cases are treated successfully, one cannot regard the "soft fungus" as identical with those tumours I have alluded to. He subsequently observes, "There is, indeed, another kind of hæmorrhage of more importance, but this seems to be sympathetic, and is attendant upon infants who are in an ill state of health during the month, and is perhaps a bad sign. It takes place where the cord has been apparently well healed, but the skin afterwards gives way, and the bleeding is much more considerable than in the former. It requires, however, nothing more than the application of common styptics with proper compress and bandage." This form of hæmorrhage appears to be analogous to that which occurred in the case I shall describe. In the subsequent chapter, he treats of "Soreness or Ulceration of the Navel," and writes as if familiar with it—"A more troublesome case is that of the part becoming sore, often some weeks, after it has appeared to be healed, and as far as I have seen, ... has taken place only where the skin of the belly has extended an unusual way on the cord." "This soreness is likewise attended with much thin discharge, which disappears and returns irregularly, together with a raw appearance of the part, which is not many days in the same state." Under the treatment he suggests—"I have always found it get well, but sometimes not in less than five or six weeks."

In Dr Alexander Hamilton's "Treatise on the Management of Female Complaints and of Children in Early Infancy," 1792, no mention is made either of umbilical hæmorrhage or tumour.

In Dr James Hamilton's "Hints for the Treatment of the Principal Diseases of Infancy and Childhood," which is founded on the work of his father, and first published as a separate volume in 1809, under the heading of the Management of the Navel, no reference is made to hæmorrhage; but he remarks, "Sometimes, whatever precautions be used, a rawness round the edges or degree of ulceration remains, and proves very difficult of cure. This arises in some cases from a small excrescence, not much larger than a garden pea, sprouting up from the root of the cord, and in other instances from an erysipelatous inflammation." As his book was intended for popular use, he observes, "As the most unfortunate consequences have often been occasioned by neglect in such cases, and as a great variety of treatment is necessary under different circumstances, a practitioner should always be consulted."

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[1] In the first edition, 1784, he briefly alludes to it.

In the sixth edition of Burns' "Principles of Midwifery," 1824, both hæmorrhage and tumour are treated of. He observes, the vein "is supposed to bleed oftener than the arteries." He seems to consider there is no safer nor better plan of treatment than the sustained pressure by "the point of the finger."

The "Traité des Maladies des Enfans nouveaux-nés et à la Mammelle," by C. Billard, was published at Paris in 1828. He has a chapter on the "Chute du Cordon ombilical;" but his observations on hæmorrhage or tumour, following it, are vague and limited. At one place he remarks,—"La ligature est, à cette époque, tout-à-fait, inutile, et l'on ne doit plus craindre d'hémorrhagies par le bout du cordon;" but this has reference to hæmorrhage whilst the remains of the cord are still attached. At another part of the same chapter I find,—"On observe aussi, au centre de l'ombilic, une sorte du tubercule mollasse, et plus ou moins rouge, formé par les extrémités vasculaires reúnies, et qui disparait et s'enfonce peu à peu dans l'abdomen. Quelquefois ce tubercule s'enflamme, devient fongueux, et forme au centre du bourrelet cutané une excroissance qu'on est obligé de cautériser." These are the only remarks I find in this original author which have any bearing on the subject of which I am treating.

The *Edinburgh Medical and Surgical Journal* of July 1832 contains an article by Mr Thomas Radford, of Manchester,—"Remarks on Umbilical Hæmorrhage." It is an excellent, though short paper, and, contrary to the favourable statements of some of those I have quoted, he observes,—"Hence, when the funis comes away, or in a short space of time after, a hæmorrhage commences from the navel, which generally destroys the infant." He is, I believe, the first writer on the subject who draws a distinction as to the result between hæmorrhage from the umbilical arteries or vein,—"The result of umbilical bleeding in my practice has been invariably fatal, with the exception of the case just related; and, from an extensive inquiry amongst my professional friends, I have ascertained this to be the usual termination." "Venous hæmorrhage from the umbilicus occurs frequently, and, in general, terminates fatally. Arterial hæmorrhage, on the contrary, in this situation, is more rare, and more fortunate in the result."

Subsequently to Mr Radford, various communications on this form of hæmorrhage appear in the journals,—not the least important of these being that of Dr A. D. Campbell to the *Northern Journal of Medicine*, August 1844, in which he indicates that jaundice occasionally complicates these cases.

In 1849, Mr Edward Ray submitted a communication "On Hæmorrhage from the Umbilicus after the separation of the Funis," to the South London Medical Society, in which he states,—"Of the 16 cases *distinctly* mentioned in this paper, 9 *are reported as having occurred* in males,—in the remaining 7 cases, no allusion is made to the sex by the reporters; of the 16 cases, 14 proved fatal."

Dr H. I. Bowditch published a paper in the *American Journal of Science*, for January 1850, "On Hæmorrhage from the Umbilicus in New-born Children, with Cases." He instances 2 cases of fatal hæmorrhage occurring in the family of the same female. One of these was a female, the other a male. After giving a list of the names of authors he has consulted, but which from its inaccuracy I do not quote, he gives the credit of first distinctly noticing the disease to Underwood, who, he says, "alone makes two classes." I hope I have made it clear that the malady which we now dread to meet with was known more than a century before the time of the last named author. Dr Bowditch states the records of 12 cases that occurred in Boston, but does not state the sex of those affected. He divides these cases into "five classes:"—

1*st*, Bleeding from the funis itself.

2*d*, From forcible removal of the cord.

Neither of these classes requires observation at this time.

3*d*, Hæmorrhage from "an imperfect closure of the vessels from non-coagulation of the blood."

4*th*, "The largest class of serious bleeding." "In those cases it is very common to observe an alteration in the functions and structure of the liver, the dejections being non-bilious, and at the post-mortem examinations disease of the hepatic structure or of the ducts being observed."

5*th*, "The really hæmorrhagic tendency." "The blood in these cases oozes from the gums, intestines, under the skin, etc."

With this division I do not altogether agree; and in the treatment he himself observes,—"It will be difficult to decide whether the bleeding is owing to the open mouths of deep-seated vessels, or to an original hæmorrhagic tendency." In the treatment of the third class cases, he follows that advised by Dr Radford (but previously suggested by Burns), to cut down and tie the bleeding vessel.

In the fourth class, when the liver is at fault, "very little hope may be anticipated of affording relief." "Some have advised internal remedies in these cases, with the idea of correcting the fluidity of the blood. For that purpose I used sulph. of soda, as recommended in the *Brit. and For. Med. Review*, ix. 247."

In the fifth class, he regards the actual cautery as the best styptic; but in his cases "the cautery did not in fact check the hæmorrhage one hour."

Soon after Dr Bowditch's article was published, Dr John Manley read a paper before the Abernethian Society, which appeared in the *Medical Gazette* of May 3, 1850. It is "On Hæmorrhage from the Umbilicus, after the separation of the Funis." In this communication he commences by describing a case which occurred in his own practice, which terminated fatally in spite of all his efforts to arrest it; and he quotes cases which had occurred both in home and foreign practice. He alludes to the frequency of these cases happening more frequently in males, and observes,—"The frequent occurrence of jaundice, of petechiæ, and of extravasations of blood under the integument, or into the gastro-intestinal canal, are not the least interesting features of this singular affection." "The physical qualities of the blood are also worthy of notice: it is frequently mentioned as being of a lighter colour than natural, and presenting no tendency to coagulate." In the treatment, Dr Manley has no additional light to throw on these cases; and, from their short duration, does not think we are "warranted in placing much dependence on constitutional remedies alone." He is in favour of the ligature *en masse*, as recommended by M. Paul Dubois and also mentioned, but less favourably, by Burns.

In Dr J. M. Coley's "Practical Treatise on the Diseases of Children," 1846, no reference is made to hæmorrhage; but on "Tumours at the Navel" he writes,—"When cicatrization is interrupted after the separation of the funis, fungous flesh is apt to appear, which either assumes a mulberry appearance, being of a dark red colour and irregular on its surface—or, being covered by a fine cuticle, resembles a polypous tumour."

Dr Churchill's "Diseases of Children" was published in 1850. The only statement bearing on our subject is at page 17,—"However long it (the separation of the funis) may be, no efforts should be made to detach it, as the risk of fatal hæmorrhage would be very great."

In Bird's edition of "Bouchut's Practical Treatise," 1855, he alludes to hæmorrhage, and points out the frequency of its occurring with signs of purpura. With regard to treatment, he says the ligature *en masse* "is the only method which has, up to the present time, succeeded;" and he does not mention internal treatment, nor does he refer to umbilical growth.

Dr West (4th ed. 1859) alludes to umbilical hæmorrhage being the cause of death in cases of congenital hepatic malformations. He also states that the blood "is almost completely destitute of the power of coagulation." He, too, is in favour of treatment by the ligature *en masse*. He does not take notice of umbilical tumour.

In Dr Tanner's "Diseases of Children," 1858, special reference is made to fatal hæmorrhage.

Mr J. Cooper Forster, in his excellent work on "The Surgical Diseases of Children," 1860, observes that the chief diseases of the umbilicus in early life "are ulceration, either of a specific or simple character, vascular growths, and fæcal fistula." The first "only occurs in children that are neglected;" and, with regard to the second, he has seen "in three or four instances, a small polypoid growth from the umbilicus in children from one to eighteen months old." "This growth appears at first like a granulation of unusual size, but I have seen it attain a length of three quarters of an inch." "It is of a red colour, and pours forth a constant purulent secretion; occasionally it bleeds, but gives rise to no pain or other inconvenience. I have not been able satisfactorily to trace the history of these cases, but I presume they result from imperfect separation of the umbilical cord."

I find also that Mr T. Bryant, at p. 143 of his "Lectures on the Surgical Diseases of Children," 1863, states that the "fleshy tubercle at the umbilicus is an affection which occasionally comes under notice." He considers it to be composed of exuberant granulations, as it always follows the separation of the cord. He has treated five cases successfully by the ligature. The oldest infant was fifteen months.

I have now traced the literature of the subject of my communication from what, so far as I have been able to ascertain, is its earliest notice; and I consider it is to Mauriceau, and not to Underwood, that the credit is to be assigned of having first put on record hæmorrhage from the umbilicus after the separation of the funis. The former observed the fatality attending its occurrence, whilst the latter treats of it as being of small importance. To Zuingerus as preceding Underwood, merit must also be given for his decided allusion to the hæmorrhage and his true appreciation of its danger.

In my perusal of the various writers on the subject, I have been impressed with this, that two forms especially have been observed, —the first, and most hopeless, associated with hepatic disease or malformation; the second not being so associated. As regards the first, the prognosis appears always to be of the most gloomy; but, as regards the second, a more hopeful result may be possible. Of the first I have only to observe, that no treatment in any case appears to have been successful, nor would it be expected to be so, if malformation existed; and the instances of hæmorrhage with jaundice without malformation, seem to be limited; but cases have occurred where both jaundice and hæmorrhage were present, and the infant has survived.

The treatment of the second class is more hopeful. It may be subdivided into those cases in which there is a hæmorrhagic diathesis, and those in which it is not known to exist. I am not quite inclined, however, to recognise this distinction, for I do not hold that these cases can be assigned entirely to this hereditary peculiarity. Mr Ray, who supported this view, seems to found his opinion principally on this, that male children rather than female are the subjects of it; and in 16 cases, as already stated, 9 were males; the sex of the other 7 was not reported. The frequency of more than one case occurring in the same family is presumed to give weight to the hereditary influence. Instances of more than one member of a family dying from umbilical hæmorrhage have been given by Maxwell Adams, Bowditch, Ray, and Dr Smith of New York; but these are capable of explanation, on the view of the blood being in a purpuric condition, and the evidence of transmission of the hæmorrhagic diathesis is deficient; for although it is clear enough that two brothers, or a brother and sister, have both been the subjects of umbilical bleeding, it has not been made equally clear that the ancestors or immediate progenitors or relatives had been the subjects of this peculiar diathesis in any number of instances. Dr Manley cites one instance where the mother, grandmother, and aunts were subject to epistaxis, and the second infant died on the third day in consequence of hæmorrhage from the umbilicus and the gums. Mr Ray's cases do not well support the theory. That the state of health of the mother affects the condition of the blood of the fœtus is well illustrated, were illustration necessary, by a case detailed by M. Stoltz, in the 15th vol. of the *Arch. Gen. de Med.* The pregnant mother died with petechiæ on various parts of the body, and the blood was found to be liquid. In the fœtus extravasations were also found, and the blood was in a similar state to that of the mother. I prefer, therefore, to divide the cases into the two classes. The peculiarity of all the cases of the second class is sufficiently constant. It is the fluidity of the blood, and its little tendency to coagulate; the inference being that the blood is in a diseased state, which I consider to be purpuric. This view is sustained by the frequency of hæmorrhage from the bowels, and petechial eruptions or extravasations. The following tables have been prepared to show more distinctly the nature of the cases of this disease, so far as I have been able to consult the authorities. From the *Edinburgh Medical Journal* it is found that, at the meetings of the Obstetrical Society since 1860, 14 cases have been mentioned. Of these, 3 were associated with jaundice; 4 were without this complication; and in the other 7 reference is not made to the state of the child. One of those with jaundice had melæna. The sex is stated in two instances: one being a female, the other a male; the hereditary tendency is not stated. The duration is stated in three cases as 4, 3, and 2 days. The result was that 9 died and five recovered. The treatment of the latter was as follows:—in 3 the ligature *en masse* was used successfully; in 1 it failed, and success attended the use of perchloride of iron; and in 1 the cautery was successful. If to these I may be permitted to add my own case, it was without jaundice: a female: of chronic duration, and treated by internal remedies, with no predisposition other than chronic bronchitis and debility in the mother.

The second table is formed of cases found in the writers consulted. There are 32 cases in all. In 11 hepatic complication existed; in 2 there was none; in 19 reference is not made to the state of the child; in 9 petechiæ or signs of purpura were present; in 2 of these there was hepatic complication. The sex is stated in 18 instances: 2 were females, 16 males. In 14 it is not stated. The hereditary tendency is vague. The duration is stated in 14 cases; it varied from 5 hours to 6½ days. The result was, that 25 died and 7 recovered. Compression was successful in 1 case; plaster of Paris in 1; ligature *en masse* in 2; caustic in 1; perchloride of iron in 1; and in 1 the means is not stated.

I have not been able to see the paper on this subject by Dr Smith of New York. From Dr West's work I learn that he has collected particulars of 79 cases, and that in 26, "other children of the same parent had died from the same untoward accident."

Excluding these cases of Dr Smith, but including Homan's, the other tables give the following combined information:—Total cases, 59. Of these, with hepatic complication, 14; without hepatic complication, 7; not stated in 38; indications of purpura in 18. Sex, 17 males, 3 females; sex not stated in 39. Hereditary tendency vague. Average duration of stated acute cases 64 hours. Result: 42 died; 17 recovered. Treatment of the 17 successful cases: not stated in 5; ligature *en masse*, 5; perchloride of iron after ligature, 1; perchloride of iron alone, 1; cautery, 1; compression, 1; plaster of Paris, 1; caustic, 1; citric acid and iron internally, 1.

TABLE I.

Authority.	No.	Sex.	Duration of Hæmorrhage.	Result.	Treatment.
Dr Keiller, . . .	1*	...	...	Died	Ligature *en masse*.
Sir J. Y. Simpson,	2†	...	...	Do.	Do.
" " .	3†	...	...	Do.	Do.
Dr Hamilton, . .	4	...	...	Cured.	Do.
" " . .	5	...	...	Do.	Do.
Mr Syme, . . .	6	...	...	Do.	Do.
Dr Moir, . . .	7†	...	...	Do.	Do., & perchl. iron.
Dr Burn, . . .	8	...	...	Do.	Cautery.
" " . . .	9	...	...	Died.	Ligature.
Dr Thorald, . .	10* *p.*	F.	4 days	Do.	Perchloride of iron.
Dr Pattison, . .	11	...	...	Do.	
Dr A. Simpson, .	12	...	...	Do.	Styptics and ligature.
Dr Linton, . . .	13†	M.	3 days	Do.	Perch. iron; plaster Paris.
Dr Wilson, . . .	14*	...	2 "	Do.	
Dr Ritchie, . . .	15†	F.	Chronic	Cured.	Internal; citric acid & iron.

Note.—* 3 cases with hepatic complication. † 5 cases without hepatic complication. *p.* 1 case with indications of purpura. No hereditary tendency stated.

TABLE II.

Authority.	No.	Sex.	Duration of Hæmorrhage.	Result.	Treatment.
Mauriceau,	1†	...	...	Died, ag. 12 d.	
Prout, . .	2	M.	2 days	Do. „ 10 „	
„ . .	3	...	...	Do. ...	
„ . .	4	...	...	Do. ...	
Radford, .	5	...	1 day	Do. ...	Compression.
„ . .	6	...	...	Cured, ...	Do.
Campbell, .	7*	F.	2 days	Died, ag. 10 d.	Styptics.
M. Adams,	8*	M.	4 „	Do. „ 11 „	
„	9*	M.	4 „	Do. „ 11 „	Styptics, iron, alum, &c.
Ray, . .	10*	M.	...	Do. „ 19 „	
„ . . .	11*	M.	...	Do. „ 11 „	
„ . . .	12* *p.*	M.	2 days	Do. „ 12 „	Styptics and needles.
Bowditch, .	13† *p.*	F.	6½ „	Do. „ 20 „	„ and ligature.
„ .	14* *p.*	M.	4 „	Do. „ 14 „	„ caut., & sulph. soda.
Marsh, . .	15*	...	...	Do. ...	
„ . .	16*	...	...	Do. ...	
Cook, . .	17*	...	...	Do. ...	
Manley, .	18*	M.	4 or more d.	Do. ag. 16 d.	Styptics and cautery.
Key & Babbington, .	19 *p.*	M.	...	Do. ...	
„ .	20 *p.*	M.	...	Do. ...	
„ .	21 *p.*	M.	...	Do. ...	
„ .	22 *p.*	M.	...	Do. ...	
Hill, . .	23	...	5 hours	Cured, ...	Plaster of Paris.
Steny, . .	24	...	...	Do. ...	
E. Dubois,	25 *p.*	M.	1 day	Do. ...	Ligature *en masse.*
„ .	26	M.	1 „	Do. ...	Do.
Thoré, . .	27 *p.*	M.	6 days	Do. ...	Caustic.
West, . .	28	...	1½ „	Died, ...	Ligature *en masse.*
Churchill, .	29	...	...	Do. ...	
„ . .	30	...	...	Do. ...	
Keiller, .	31	M.	...	Do. ...	Ligature.
Tanner, .	32	...	...	Cured, ...	Perchloride of iron.

Note.—* 11 cases with hepatic complication. † 2 cases without hepatic complication. *p.* 9 cases with indications of purpura. No. 27 subsequently died of purpura. No hereditary tendency stated.

In a third table it was intended to have grouped the cases collected by Homan of Boston, quoted by Bowditch, but the particulars are so vaguely stated that no satisfactory information farther than the statement of the cases, which may be thus arranged, can be obtained. The cases were 12 in number; the condition of the liver is not mentioned; 9 of them showed indications of purpura; the sex is not stated, and 1 case appears to have had hereditary tendency. The duration of 1 case was a few hours. The average duration of 8 other cases was 3 days 18½ hours; 4 of the cases were cured, and 8 died.

From these figures we may gather that, 1*st*, Hepatic complication is not necessarily associated with umbilical hæmorrhage.

2*d*, That purpuric indications are frequently met with. 3*d*, That there is an apparent greater frequency of its occurrence in males;—but on this point the information is very defective, as the sex is not stated in 38 of the 59 cases. 4*th*, That the hereditary tendency requires further elucidation. 5*th*, That the average duration of the cases was nearly 3 days; and 6*th*, That the recoveries were nearly 1 out of 3.

So much for the results of an investigation into this matter; and, in drawing this communication to a close, it may be observed that the case which led to this investigation appears to me to form one of the second class. It seems to be peculiar in the tumour following the hæmorrhage.

On the 3d March 1867, I was requested by the gentleman in attendance to visit Agnes M., æt. 7 months. He had been endeavouring, but unsuccessfully, to arrest a hæmorrhage of some standing from the umbilicus. The hæmorrhage had increased whilst the child was under his care. I found the discharge coming from the left side of the umbilicus, from a sulcus in the centre of a small ulcerated spot. The blood was dark in colour, watery, and showed no tendency to coagulate. I contented myself with recommending external styptic applications, for the child, though pale, was lively, and took the breast greedily. The mother had long been in delicate health, and could give little information regarding the state of the child when the funis separated, and could state nothing more regarding the hæmorrhage than that it had increased slowly. So far as I could ascertain, there was not, nor had there been, any purpuric eruption. The treatment was partially successful. The amount of hæmorrhage decreased, but the daily tendency to it still continued. In a month my attention was again called to the child, and I visited her on 14th April. The character of the discharge, its little coagulability, and the absence of any apparent cause for the hæmorrhage, led me to regard it as being probably due to the dyscratic condition of the blood. The child was now pale and exsanguine, and had sadly lost the plumpness of health. I failed to detect any extravasations or petechial spots. I, however, regarded the condition of the blood as being purpuric; and, as before my first visit, attempts had been made to alter the feeding of the child, I recommended citric acid with citrate of iron, internally, with the application of sulphate of copper lotion to the ulcerated surface. In a few days there was a marked improvement, and when I saw it ten days after (24th) there was merely a very slight oozing, but the blood showed increasing coagulability. On one subsequent occasion there was a return of the hæmorrhage, but it did not continue; and as the ulcerated surface healed, a vascular growth formed, which, in its early stage, had much resemblance in size and colour to a red currant. There was now no hæmorrhage, and, as the child improved in appearance, the citric acid and iron were continued; the tumour steadily continued to increase, and by the

11th July it had almost attained the size and appearance of a Morello cherry. It was attached by a broad base, was not pedunculated, and in consistence was firm, elastic, and incompressible, and appeared to be covered by a fine cuticle. Without much difficulty a ligature was placed round the base, and the tumour immediately became darker in colour; the necessary manipulations occasioned some venous blood to flow, and during the afternoon there was a slight return of hæmorrhage. July 13, With the exception of pain and constipation, the case has gone on well; to relieve the latter, phosphate of soda was ordered. July 16, Bowels more regular; the tumour has decreased in size, and has separated this evening without bleeding. There is an ulcerated surface of some size. The citric acid was continued for a short time. There was no return of hæmorrhage; the ulcerated surface healed, and the case was lost sight of till to-day (November 27), when I find there has been no return of hæmorrhage; the umbilicus is less depressed than it generally is, and the upper part is hard.

The growth of this tumour is a matter of some interest. In those cases to which allusion has been made, the authorities have observed them when the cord separates, and the opinion of some of them is, that they are due to an exuberant growth of granulations. A peculiarity, it may be, in the case now detailed was, that the cord appears to have fallen off naturally without ulceration, but that subsequent ulceration occurred, and the growth did not form until the ulcerated surface apparently healed. Whilst the ulcer was largest, it did not exceed three lines in length by one in breadth, and did not show any tendency to the granulation being exuberant,—the tumour from the first was globular.

In the absence of any agreed treatment of the cases of the second class, I would venture to recommend a trial of citric acid. The case narrated, it may be said, can scarcely be taken as an example of the more serious form of umbilical hæmorrhage, and I would not feel justified in offering an opinion from it alone. But it is observed that purpuric dyscrasia is not uncommon amongst very young children; and I am inclined to believe it is more common than is generally allowed. The rapid improvement which I have sometimes seen to follow the administration of citric acid in these cases, and the benefit which attended its use in the case detailed, have led me to suggest a trial of it by those who may meet with suitable cases. But whilst doing so, it is not to the exclusion of those local means which are judged the most suitable in such cases, but chiefly to aid them; nor is it substituted for any other remedial agent.

When I commenced referring to authorities on the subject of this paper, I found that the knowledge of the writers was defective as to what had been written on it. I also regretted to find that the recorded cases were in many points defective, and could not be accurately compared. In bringing it before the Society, I have endeavoured to supply a want, but my communication is not so perfect, from the cause stated, as I could wish it to be. In conclusion, I would suggest that in recording future cases of this form of hæmorrhage, attention be paid not only to the various points having reference to the child, which are noted in the tables, but also to the state of health of the mother, which hitherto has not been done.

ARTICLE IV.—*Case of Fracture of the Skull, with Hernia Cerebri, and Compound Fracture of Pelvis, followed by Recovery.* By JOSEPH BELL, F.R.C.S., Lecturer on Surgery, Assistant-Surgeon, Clinical Wards, Royal Infirmary.

(*Read before the Medico-Chirurgical Society*, 1st *April* 1868.)

ON the afternoon of the 13th of April 1867, Thomas Raisbeck, æt. 25, was driving the engine of which he had charge on the North British line. He had taken a train up an incline, and was returning with only a goods van attached to the engine down the hill again. He was in front of an express train from the South, and with his light burden was going at the rate of at least 40 miles an hour. He discovered that something was wrong with the supply pipe of the engine.

This is, he tells me, a pipe which passes from the tender to the engine, below the footboard on which the driver and the stoker stand, and can be reached by them only by leaning over the side of this footboard and reaching below the engine. He was very unwilling to stop, and thus delay the express which was just behind him, so endeavoured to remedy the defect by trying to reach it without slackening his speed.

He forgot that he was approaching a station, and was still leaning over when the engine swept past Tynehead, the platform of which station at once knocked him off his step, and crushing him between the platform and the step, a distance of five or six inches at most, left him lying on the line. Most fortunately he fell clear of the wheels of the tender and van. The stoker at once stopped his engine, returned for him, picked him up quite insensible, and brought him into town as rapidly as possible.

He was at once conveyed to the Infirmary, and admitted into Mr Syme's wards, by whose permission I treated him, and now report the case. On arriving, I found him very restless, and quite unable to articulate, though apparently he could recognise his wife and his mother who had come to see him. He was bleeding from two flesh wounds on the back of his head. One, extending obliquely backwards from the right parietal towards the occiput, was about three inches in length, and led to a comminuted and depressed fracture of the skull about two inches in length and one broad. The bone was shattered, the dura mater wounded, and one small piece of bone was driven through the dura mater into the substance of the brain. The wound of the dura mater was large enough to admit a goose-quill. Another flesh wound, parallel with the first and higher up, was unconnected with the bone.

The pelvis also was very severely injured—the left ala of the ilium being knocked off the basin of the pelvis, and quite movable. A small wound, close to the anterior superior spinous process of the ilium, rendered the fracture compound. There was very great swelling of the pelvis and thigh.

He was put under chloroform, and I removed all the loose fragments of bone from the fracture of the skull, carefully preserving the periosteum. I found the inner table broken to a greater extent than the outer, but easily enough got the splintered fragments away. The injured leg was very stiff, and any movement produced loud crepitus; but, as there was no fracture of thigh, the injury of the pelvis seemed sufficient to account for the crepitus and stiffness. A catheter was passed, and drew off a quantity of urine untinged with blood. His pulse was strong and firm, about 78. There was no evidence of any internal rupture. A bandage was placed round his pelvis, and the wounds of the head dressed with dry lint, as from his restlessness no other dressing would stay on.

On coming out of the chloroform, he seemed much more sensible, and appeared to recognise his wife.

April 14*th.*—Patient had a very restless night, and became so noisy that he had to be sent to the special ward for such cases, and to be strapped to his bed. Still it was possible to rouse him, and, when roused, he was pretty sensible and quite good-natured. Put out his tongue when asked. Pulse 84. Pupils natural.

He was ordered a full dose of castor-oil.

I now report the progress of the case as briefly as possible, by a few quotations from my notes of the case taken at the time.

15*th.*—Another very restless night. Bowels freely acted on. Takes beef-tea and milk easily—no solid food. Skin hot—face flushed. Pulse 92. To have a mixture containing aq. acet. ammoniæ, and tr. hyoscy. in camphor mixture.

16*th*, 17*th.*—Still very restless and delirious; can be roused by being spoken to.

18*th.*—More rational, but very thirsty and feverish. Milk diet only.

19*th.*—Weak, wandering, very low. Brain protrudes through the wound of dura mater.

20*th.*—Pulse 120. Very free discharge from compound fracture of pelvis. Extensive erysipelatous redness of side and flank.

21*st.*—Hernia cerebri increasing, now as large as a walnut.

29*th.*—During the last week all the inflammatory symptoms became more marked—possibly partly owing to his removal from No. 10 ward to the Surgical Hospital, which was advisable for various reasons, and took place on 23d. To-day he is again wildly delirious.

Pulse 125, very hard; skin hot. Hernia cerebri as large as the half of a small apple. Head is now completely shaved, cold applied, and 1-12th of a grain of tartarized antimony given every hour.

30th.—Much better. Pulse down to 110.

May 9th.—Since last report, his general condition has much improved. The hernia cerebri has now dried up and disappeared. The wound in dura mater is apparently closed. The compound fracture of pelvis has ceased to suppurate, and now, four weeks after the accident, the fractured basin of the pelvis may be supposed to be nearly firm. Attention could now be directed to the condition of the thigh, which is shortened, and the head of the femur, which seems to be lying somewhere about where the upper margin of the acetabulum ought to be.

He was put under chloroform, and manipulation very cautiously used under Mr Syme's direction. It was found that, with the most perfect ease, the bone could be shifted from the position above the acetabulum into the foramen ovale; but, on endeavouring to place it on the acetabulum, tremendous crepitus was elicited, and the head immediately slipped out of the broken cavity. The previous diagnosis of fracture of the pelvis involving the acetabulum was thus confirmed, and no further attempt was made; the limbs were fastened together at knee and ankle, and the thigh was kept with the head as near as possible to its proper position. This interference with the parts was, however, not without bad results, though well-intentioned, for, without going into detail, his pulse steadily rose, till on the 12th it was again at 120, and on the 15th a large abscess had formed under the adductor muscles, which was evacuated low down in the thigh to his great relief.

23d.—Steady improvement since abscess was opened. Pulse down again to 104.

27th.—Discharge very profuse and fetid. Pulse up again, and rising. Skin hot.

June 1st.—Patient apparently sinking from hectic, discharge extremely profuse, new abscesses forming round hip, appetite gone, occasional rigors. Pulse 148. Bed-sores formed on sacrum and nates from the contact with the fetid discharge, which rapidly rots the bed clothes and irritates the skin.

Whisky and wine *ad libitum*, with strong beef-tea.

4th.—A slight improvement; discharge sensibly less. Pulse down to 116.

From this date the progress of the case was one of almost uninterrupted recovery. He gained appetite, flesh, and strength. The sinuses gradually ceased to discharge. The bed-sores healed, and he was dismissed Sept. 16, walking on crutches.

Present State.—General health remarkably good, thigh and leg well nourished, no shortening between head of femur and patella, but very great change in position of head of femur. It is not in the seat of either of the four well-known dislocations, but is displaced more directly upwards, approximating closely to the anterior superior spinous process of the ilium, and involving a shortening of about $2\frac{1}{2}$ inches. The motion of the joint is free enough in a limited area, *i.e.*, the head of the bone apparently plays in a cup lined by synovial membrane and cartilage, without creaking or roughness, but the flexion is limited apparently by abnormal bony processes. There seems little doubt that this new seat is the old acetabulum, limited in size, and displaced bodily upwards by the complicated fracture of the pelvis. A very similar case is recorded in the *Medical Times and Gazette* for Saturday, March 28, 1868, as existing in the Dupuytren Museum.

I have endeavoured to give some idea of the exceedingly dangerous nature of the poor lad's accident and subsequent illness; but it is difficult for any one to realize, who had not seen him, the intense prostration and apparently absolutely hopeless condition in which he was, first from the acute head affection, and secondly from the hectic from his profuse discharge. He is now working in the engine-sheds at St Margaret's seven hours a day, and his general health is excellent; although the limb is shortened, and he needs a high-heeled boot, he walks rapidly with a stick only.

With regard to the dietetic treatment pursued, his chief nourishment consisted of beef-tea and milk, in large quantity and at short intervals. The quantity of alcoholic stimulants was much smaller than is usually given in such cases in the present day. But the advantage of limiting their use was very evident in the beginning of June, when he was apparently sinking from hectic; for then the use of a few ounces of whisky daily improved his appetite, and seemed greatly to aid in his recovery.

ARTICLE V.—*Contributions to Dermatology: No. 2. On the Pathology of the so-called Sycosis Menti.* By Dr M'CALL ANDERSON, Lecturer on Practice of Medicine in Anderson's University, Physician to the Dispensary for Skin Diseases, etc., Glasgow.

BEFORE entering upon the pathology of Sycosis, it is necessary for the reader to know exactly what is meant by the expression, for there can be no doubt that many eruptions which are termed sycosis are not cases of it at all, but of the so-called impetigo menti (Eczema impetiginodes of the hairy portions of the face).

In typical cases, the disease commences by the appearance of small erythematous spots, which have a tendency to heal in the centre, and to increase at the circumference, thus leaving elevated rosy circles or segments of circles, covered with furfuraceous desquamation. As the irritation is not very great, and as the eruption is concealed in part by the hair, the patient does not apply for relief, and hence the disease is not usually seen in this stage. But as the process advances, the hairs become affected and can be pulled out with great ease, while papules and pustules form at the orifices of many of the follicles. The deeper structures too become involved, and small indurations occur, surmounted by pustules resembling those of acne. Finally, as the cellular tissue becomes more and more implicated, large tubercles, the size of a nut or larger, make their appearance: these are frequently covered with crusts, on removing which large fleshy-looking indurations are exposed to view, the surfaces of which are not unlike the pulp of a fig (hence the name sycosis from συκον, a fig), and which are very characteristic of the advanced stage of the disease.

It becomes more and more evident as the eruption increases, that the hair follicles are the parts specially affected, for many of them are obliterated, and thus permanently bald patches are left if the disease is unchecked. The functions, too, of most of those which remain are perverted; hence the hairs which grow from them have lost their shining appearance; their colour is altered, and they have become very brittle, and have a tendency to break off on a level with the skin, or within a couple of lines of it; hence the blackish points with which the tubercles are often studded. These hairs can be pulled out with the utmost ease, and have neither capsule nor root.

In some cases large segments of circles identical in appearance with those of ringworm (*Tinea circinata*) extend round the front of the neck, beneath the beard, from ear to ear. Similar patches, too, are not uncommon over the top of the sternum, and on the wrist.

The disease very rarely, if ever, terminates spontaneously unless after destruction of the hair follicles of the affected part: it is often aggravated by injudicous applications, and may last for many years when suitable treatment is not adopted.

The writer has met with cases in which all the above symptoms were present; only in exceptional instances however, for when the eruption comes under the observation of the physician, few, if any, of the circles of eruption may be detected; but even then its tubercular character is a striking and typical feature.

It is usually met with upon, or in the vicinity of, the chin (hence the term sycosis menti), although it may attack any hairy part of the body, except perhaps the head; but cases have been met with in which the head was the seat of patches of Tinea tonsurans (ringworm of head).

The disease with which it is often confounded is the so-called impetigo menti (Eczema impetiginodes of the hairy portions of the face). The latter is one of the most frequent of the varieties of eczema, whereas sycosis is comparatively rare. For Hebra tells us that he meets with it twelve times at the most amongst 4000 cases, and my own statistics give only twelve amongst 7342 cases of skin disease; and, if we put these together, we find only twenty-four cases of sycosis out of 11,342 skin diseases.

Eczema impetiginodes of the hairy portions of the face presents certain features in common with sycosis; and yet a careful study of its natural history shows that it is a different affection altogether. It commences by the formation of pustules, each of which is situated at the orifice of a hair-follicle; for a hair passes through the centre of each pustule. These dry up into small yellow crusts, which are difficult of removal owing to their adhesion to the hair as well as to the skin. When many pustules form at the orifices of neighbouring follicles, they have a tendency to run together; and, on drying up, large irregular yellow crusts are left. If these are not removed, successive exudations on the surface of the skin are confined by them, and lead to excoriations, and, owing to their continued pressure, to obliteration of the hair-follicles, and permanent alopecia as a consequence. But if the crusts are regularly removed, there is no permanent loss of hair.

The skin on which the pustules are developed assumes a dusky red tint, and becomes gradually more and more thickened and infiltrated. The patient sometimes complains of itching, oftener of pain or burning heat—a sensation which is principally experienced during the formation of a crop of pustules. The disease is often kept up for months, or even years, owing to the occurrence of successive crops of pustules.

Trusting that these preliminary remarks will enable the reader to separate the spurious from the genuine form of sycosis, it remains for us to elucidate, if possible, the pathology of the latter. In other words, is it due to the presence of a parasite, or is it a mere variety of eczema impetigo acne or the like?

Hebra is very decidedly opposed to the parasitic view. "From the circumstance," says he, "that in microscopic investigations mistakes so readily occur, and considering that in so many books upon skin diseases mistakes do occur, I shall surely not be misjudged if I doubt the existence of a sycosis parasitica so long as I have myself no opportunity of observing a case of the kind . . . . especially as the results of my investigations coincide with those of Bärensprung, Gust Simon, Wedl, and others."[1] And further on he says, that he looks upon "sycosis as a local inflammation having its seat in the hair-follicles, whose origin is altogether unknown" to him.[2] Wilson denies the parasitic nature of this as of all other skin diseases, although he admits it presents "the phytiform kind of degeneration," and tells us that "the cause of sycosis is debility."[3]

"My own observation of the disease," says Nayler, "in so far as it relates to microscopical inquiry, has led me to the belief that, in this country at least, sycosis is seldom attended by a parasite." [1] My friend Mr Jonathan Hutchinson, too, whose opinion on all subjects relating to dermatology is so valuable, has given it as his verdict that sycosis is not cryptogamic. "Will some one of the several British dermatologists," says he, "who advocate its cryptogamic origin, prove the fact by a demonstration before the Pathological Society?" [2]

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[1] Handbuch der Speciellen Pathologie und Therapie, dritter band—4 Lieferung, p. 524. Erlangen: Ferdinand Enke, 1865.

[2] *Loc. cit.*, p. 529.

[3] "On Diseases of the Skin." By Erasmus Wilson, F.R.S. Ed. vi., pp. 769 and 770. London: Churchill, 1867.

On the other hand, we find it recorded that, in the year 1842, Gruby,[3] in investigating the nature of sycosis, discovered a fungous growth to which he gave the name of the *Microsporon mentagrophytes.* The accuracy of this discovery was confirmed by Bazin and other French dermatologists, in so far as it concerned the view that sycosis is dependent upon a parasite, but their investigations led them to the belief that the fungous growth was identical with that of ringworm.

This view does not seem to have met with any supporters in this country until the publication of the results of my own investigations,[4] which tended to confirm in every respect those which had been arrived at by the French School. And a much more extended experience has only had the effect of strengthening me in the view which I then held. For of the twelve cases of sycosis treated at the Dispensary for Skin Diseases, Glasgow, during the last seven years, in almost every case [5] the parasite was looked for and detected without any possibility of doubt. And although an opportunity has not presented itself of proving the parasitic nature of sycosis "by a demonstration before the Pathological Society of London," as Mr Hutchinson suggests, I had the pleasure of doing so at the meeting of the Medico-Chirurgical Society of Glasgow, held January 17, 1868, on which occasion, amongst others, Professor Allen Thomson said there could be no doubt that the hairs exhibited were loaded with a fungous growth.

But lest there should be any scepticism on the part of others who had not the opportunity of seeing these specimens under the microscope, I took some hairs from the same case to my friend Dr Wilson of Glasgow, who kindly made a most accurate and elaborate drawing of one of them with the aid of the camera lucida, from which the illustration which accompanies this paper was taken. I can certify to the accuracy of the delineation, and no one who has any experience of parasitic affections of the hairs can fail to admit the typical nature of the degeneration. I have still in my possession a good many hairs taken from this case, and shall be very glad to give a microscopic demonstration of them to any one who still doubts the parasitic nature of sycosis.

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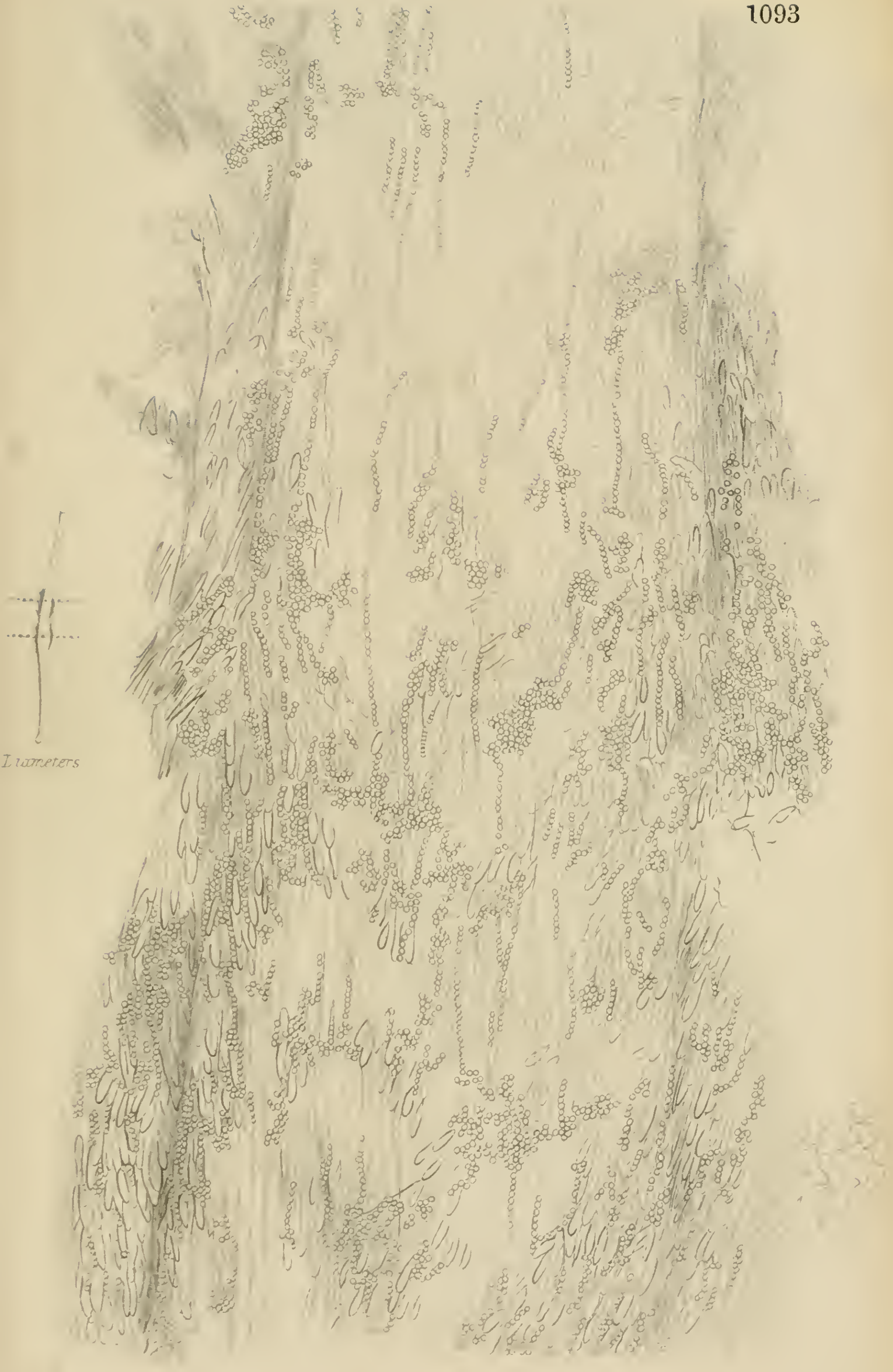
[1] A Practical and Theoretical Treatise on the Diseases of the Skin. By George Nayler, F.R.C.S., p. 194. London: Churchill, 1866.

[2] London Hospital Reports, vol. iii., 1866, p. 389.

[3] Gazette Medicale, 1842.

[4] "Parasitic Affections of the Skin." London: Churchill, 1861.

[5] In *one* or *two* the microscope was not resorted to, either from want of time, or because the features of the complaint were typical.



SYCOSIS PARASITICA.

In conducting investigations as to the nature of this disease, it must be borne in mind that many of the hairs on the diseased surface exhibit no traces of the parasite, and that for two reasons. The one is, that when the parasite implicates a hair-follicle, the inflammation which it sets up is not limited to that spot, but extends to the hair-follicles in the neighbourhood. But this inflammation of the adjoining follicles is a simple inflammation, and accordingly the hairs which grow from them contain no traces of fungous degeneration. The other reason is, that the suppurative inflammation which is set up at many points has a tendency to destroy the parasite, so that after a time there grows from the follicles an imperfectly elaborated hair, but one which is devoid of spores or tubes.

The reasons for the opinion, that the parasite met with in sycosis is identical with that found in cases of ringworm of the head and ringworm of the body, have been so fully stated in my volume on the parasitic affections of the skin, that I need not further allude to them in this place.

ARTICLE VI.—*Operation for Cleft-Palate under Chloroform.* By THOMAS ANNANDALE, F.R.S.E., F.R.C.S.E., Lecturer on Surgery, Edinburgh.

IN the *Edinburgh Medical Journal* for January 1865, I related two cases of congenital fissure of the hard and soft palates successfully closed by a single operation, and advocated the employment of free lateral incisions along the margins of the gums as an important means of obtaining a successful result in such operations. Since that time, I have operated on several occasions with complete success according to this method, but in all, the patients were adults.[1]

Operations for the cure of cleft-palate in infants and children have occasionally been attempted, but it was only recently that the practicability of safely and successfully performing the operation at an early age, and under the influence of chloroform, was proved by Mr Collis.[2] Since then, January 14, 1868, Mr J. Smith read a paper before the Medical Chirurgical Society of London on this subject. In that paper, Mr Smith gave an account of several cases of cleft-palate in infants, on which—with the assistance of chloroform, and an ingenious mouth-gag suggested by himself—he had successfully operated.

By the labours of these gentlemen, a great step in the treatment of this deformity has been gained; and I feel very confident that the successful closure of congenital fissures of the palate at an early age will be followed by much more satisfactory results, as regards the improvement of speech, than have hitherto been obtained.

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[1] In the paper referred to, I stated that in my opinion the division of the muscles connected with the soft palate was an unnecessary complication of the operation. Further experience has confirmed me in this opinion.

[2] See Dublin Quarterly Medical Journal for 1867, vol. xliv.

Having read with interest the account of Mr Smith's cases, and having, through the courtesy of that gentleman, obtained one of his gags, I determined to perform the operation, under the influence of chloroform, on the first suitable case that presented itself. Through the kindness of Mr Syme, this opportunity soon came, and has enabled me to report the following case, which was most successful, both as regards the operation and the result:—

Master B., æt. 12, was recommended to my care by Mr Syme, on account of a congenital fissure, including one-half of the hard and the entire soft palate. The fissure was rather more than a quarter of an inch wide, and the hard palate was narrow and highly arched.

On the 26th of March, the patient having been put completely under the influence of chloroform, and Mr Smith's gag introduced so as thoroughly to expose the whole mouth, I proceeded to operate after my usual method. There was no trouble from the blood passing down the throat. At one time there was slight sickness, which did not, however, interfere with the operation, except to cause a short delay until a little blood had been expelled from the stomach. The entire extent of the fissure was operated on, and the edges brought together by means of seven silver-wire sutures.

April 1st.—The entire wound is healed, with the exception of a small point at the junction of the hard and soft palates, where a stitch has given way.

This loose stitch was removed, and also one from the hard palate.

4th.—Three stiches removed from the soft palate, which is firmly healed. The small opening referred to in the last report is contracting. The patient sat up in the drawing-room yesterday, and was allowed to take solid food for the first time.

6th.—The remaining stitch removed. The wound in both hard and soft palates is firmly healed, and the small opening is rapidly contracting. The patient was out walking to-day.

10th.—Patient returned home to England. The little opening has nearly closed, and his friends already notice an improvement in his speech, especially in the articulation of certain words.

May 8th.—To-day I had an opportunity of seeing the patient, and found that the little opening had still further contracted, and the parts operated on remained perfectly sound.

ARTICLE VII.—*The Katakleistic Treatment of Wounds and Granulating Surfaces.* By A. L. MACKAYE, late Senior Surgeon of the Artillery of the Army of Tennessee, C.S.A., sometime Consulting Surgeon, Officers' Hospital, Griffin, State of Georgia.

MOST surgeons who have had much to do with operations will agree with Mr Adams in a late paper,[1] that "no subject can be of greater interest to the scientific surgeon, or of more direct practical importance, than the principles upon which the treatment of wounds should be based." Under a like impression, I submit the following extracts from my case-book :—In December 1856, I took charge, as acting regimental surgeon, of the Hospital of the 61st B.N.I., then stationed at Jullundhur. I found there a Punjabee private who had been a nuisance to medical officers, colonel, and adjutant almost ever since his enlistment. He suffered from an ulcer similar in shape to, and about the size of, an ordinary teaspoon-bowl, on the lower half of the patella; its apex at the insertion of the extensor tendon, and almost exactly in its narrower pear-shape outline, defining the margins of that structure. There was no thickening of its edge, very little discharge; surface dull, and reddish opaque coloured; not transparently shining as in healthy granulating surface; no pain, no fragmentary debris either adherent or loose on its surface. A believer in strict classification of ulcers would have had no difficulty in pigeon-holing it systematically. I simply describe it. His general health was good, or any little deficiency was clearly due to his confinement in hospital. There was no history of syphilis, immediate or derived; no post-cervical gland enlargement, milky tongue or lip-commissures; skin perfectly free and healthy; no rheumatism, rheumatoid symptoms, or history of such; no penis scars, though carefully looked, *as well as felt for*. In everything but the ulcer, the man was perfect almost. He had been treated as a malingerer of the active sort, and every possible step taken to carry out that view, but without avail. If partially cured, he was scarcely gone from hospital but he was back again, so that it became a question whether or not to discharge him from the service. I found that the man's character was good; that though a young soldier, he knew his drill well (always an important question in speculating on malingerers); had been a smart active man. Conscious from the statement of my native apothecary that he had been made to run the gamut, if not the gauntlet, of all sorts of dressings and drugs, I determined to try the exclusion-of-air method, which I had over and over again used, and not found wanting. The surface of the sore was sprinkled with starch in impalpable powder, sufficient and no more than to absorb the slight moisture on it, and so on during a watch of about two hours on the part of the native apothecary—like most of his countrymen, though by no means all of his class, a very intelligent fellow. When the last sprinkling retained its dryness, a thick solution of gum-mucilage was painted over the artificial skin now made, and starch in powder thickly strewn over it. The limb was kept extended, immovably fixed, and the man confined to bed. Next morning, the gum had contracted so as to lift its edge slightly but undeniably from the surface of the ulcer, still the original coating of artificial skin in starch kept its place intact. Gum was painted round the gap with a camel-hair brush, so as not to introduce air-bells, yet fill it up, the apothecary ordered to watch its drying, and if contracting, to fill up the resulting vacancy with thickened gum. The result was an artificial shell, not unlike a limpet's or a rupia crust; like it from similarity of physical formation, as any who study that remarkable crust, self-telling its history in formation, may satisfy themselves. On the fifth day of its existence, fully formed, the sixth of treatment, it began to curl up at its edge, and sound skin was visible beneath. It was left undisturbed, and on the tenth day, I found it tumbled off in bed, the ulcer cicatrized, the new skin of wonderfully firm and advanced consistence. It by no means gave one the usual impression in such cicatrices of subsufficient "vital force" and tender organization we are all familiar with, and capable of explaining since Bennett's and Paget's researches, with their triumphant restoration of surgery to the place it held in John Hunter's eyes. I kept the patient in hospital a week longer, allowing him to move about, ordered a dose of castor-oil to clear out his bowels, which, for the first five days, had been kept locked up. Compared with the risk of motion to the ulcer, I thought his cæcum might well be used as a cesspool. The case requires no comment. Of a certain sort of ulcer, unhealable, or rather intolerably troublesome, familiar in hospital practice, but not confined to it, it is typical. It is not an ulcer depending on cachexia; mercury, past unskilful treatment, progress to-day, defeated by regress to-morrow; a slip of cicatrix succeeded by a band of ulceration at an opposite point; a central island sprout of granulation passing into the waste of ulceration after a day or two of sickly growth; one or other of the thousand unclassable Protean forms the force of ulceration gives rise to in intractable ulcers, such as all surgeons know. It is the ulceration which not uncommonly follows burns of the skin (superficial), amputation scar of the finger or toe, where there is and can be no question of tightness or stretching of the flaps, and very commonly simple flesh wounds by ball, in such muscular regions as the deltoid, gluteal, or thigh extensor mass. It is a true local life, as thoroughly independent as that of the misletoe. In subjects such as the case of the Sepoy, I venture to say, from some experience of the method I have chosen to call katakleistic[1] (sub-cutaneous not being strictly true, and definiteness always desirable), will succeed when nothing else can, at all events does. It is useless to try it in ulcerations not of the sort, or similar to that I have described. Where there is much "irritability," "angriness," or discharge, these first must be got rid of; and if much discharge still remain, steps taken to allow it to drain away, if shutting out the air be attempted. To allow the products of ulceration to drain off at the same time that air is kept from the sore, is not impossible; treatment, I need not enter into, should always be put in force to reduce the discharge to a minimum. With discharge beyond a certain amount, it is a physical impossibility to make a crust over the ulcer; still, in large ulcers, it is often enough found that the source of the main flow is a very limited space, and the rest may be treated katakleistically—at all events, the attempt, even if not successful, can do no harm; and I know nothing more trying to the patience and wits of the surgeon, though often by no means so to the patient, than forms of intractable ulcer, large or small.

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[1] Medical Times and Gazette, 1858.

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[1] Κατακλειω, to shut up.

In the first week of this year, I was summoned to see a girl, 9 years old, who had been worried by a sheep-dog. The sole feature of interest surgically in her case was a lacerated wound of the left upper eyelid. The cartilage was just pierced by a tooth at its inner upper edge, the skin dragged off so as to show the special structure beneath it, the free margin of the lid uninjured, the continuity of the commissures entire. It was severely torn, but not separated—a rather rude dissection of the cartilage in its place in the upper eyelid had been demonstrated, the skin hanging from it in a torn, ragged flap—an appearance certainly by no means suggestive of union by the first intention, nor criminatory of a surgeon who should not think of attempting it. I washed it gently, to remove what blood there was adherent; and after the oozing of serum had ceased, mopped up as much as possible with fragments of blotting-paper, till the surfaces were comparatively dried. Not having any fine metallic suture, I used a common delicate domestic sewing needle, and firm almost metallically hard fine Paisley thread, to put in a couple of stitches, enlarged by the 1-16th of an inch, with a tenotomy needle, the tooth perforation graze through the mucous membrane as a drainage route, rendering the outer and inner surfaces of the eyelid continuous, shut up the eye by two narrow slips of isinglass plaster, and, waiting till serous exudation showed itself along the lacerated margin, scraped some lint into charpie, and stuck it over and into the oozing. Ordered the dressing on no account to be touched: if pain, discomfort, or swelling ensued, to let me know at once. I saw and heard nothing of the case for weeks, it being some miles off, deep in the mountains, when, on the thirty-first day after, the mother brought her to me. The stitches were still in, the scab-crust had fallen off, and a faint, scarce credible line of cicatrix was perceptible on close and special examination—a line, visible, certainly, but not to ordinary and justifiable examination of the face of a woman, a stranger to us. I snipped out the stitches, *no pus nor fluid* followed. The tiny perforation showed under a pocket lens no sign of ulceration: it exactly resembled, on a minute scale, the hole in the lobe of a child's ear for a jewel.

It proves to me, what I imagine is pretty generally allowed, that metallic sutures act by their non-retention of, and non-soaking with, pus, organic, or organizable fluids. Their relations are purely physical, not so-called vital. We wax our silk sutures, certainly, but the wax melts in the heat of the wound, the by no means thin unwaxed silk becomes spongily distended; a small but effectively injurious mass full of changing matter results; the soaked suture is physically incapable in its capillary attraction of discharging its contents. The first impregnation of matter in them remains during their whole use. A suture *invariably* stinks putrid in the midst of healthy pus. The fine, dense sewing cotton was physically unfitted for soaking, hence the fact that, after many days, it was removed *unchanged* and *unsmelling*. One has not always wire sutures. On that ground, I mention the use of Paisley thread: it may be a useful hint to some of my rural mountain brethren. Indeed, so confident am I in its use, that if I again have a vaginal fistula operation, I mean to use the same thread previously steeped in carbolic acid dilution. I confess that no man who has ever operated for fistulæ in the female generative region doubts the use of wire, or wishes aught better; but every one who has it in his power should do his best to destroy faith in specifics, and the reasoning thereon founded. Iron-wire suture has been upheld before now by believers in specifics and vitalism. Let me quote a third and last case.

I was once called to see an officer's wife who had been wounded by him in an access of furious jealousy, while under the mania due to excessive champagne and rum. He had, in a fortunately wavering blow, sliced the outer half of the areola of the left nipple into the ducts, as she, sharing his bed against my orders, tried to soothe him in his delirium. As she was a singularly pretty young lady, the injury had an ulterior prospect by no means reassuring; and I well remember that much the more terrible aspect of it in her eyes was the natural fear of a scar in a region not meant to be hidden, and suggestive in its concealment, if inevitable, of criticisms from dear friends. It certainly seemed a hopeless case, so far as escape from a scar was concerned; but the knife must have been remarkably clean and sharp in its edge. The wound could not have been made *secundum artem* more perfectly. Bringing the flap *in situ*, after washing it, I placed a piece of lint soaked in Tr. benzoin co. over it, enveloped the nipple, which was more than usually developed and prominent, though she had never had a child, in a nest of charpie similarly soaked, and left her. It healed up perfectly. No discharge of pus ever made its appearance; there resulted merely a faint linear trace visible to no eyes but her own and her husband's.

I think that Dr B. Ward Richardson has administered the *coup de grâce* to the germ theory as usually understood in relation to the healing of sores and wounds; but, notwithstanding, I willingly submit those cases to the believers in vibrionic and similar influence as productive of delay and secondary union in wounds. The first was in an hospital in the Punjab, by no means sanitarily situate, though neither crowded nor ill-ventilated. Vibriones and similar organisms existed there *en masse*, as I knew on quite independent grounds, in the course of other researches. Dysenteric and indigenous Punjab diarrhœa stools could scarcely be kept between the evening and the morning visit for examination, so rapid was the putrefaction engendered in them. In those days, it must be remembered, carbolic acid, in its commercial form, did not exist. Chlorine will act on the products of putrefaction, not hinder it; that alone was what we had to trust to. I do not regret it, since possibly under the use of the tar acid I should not now have so vivid a sense of the ærial influences there present. There was nothing applied in the soldier's case that could kill a vibrio, or hinder it from passing into or upon the ulcer. It was shut up from the air, but certainly not hermetically, nor beyond the unquestionable penetrative power of "germs." Let me also point out that, exposed to the air, the ulcer gave no signs of vibrionic action. There was no foul smell, no inordinate discharge of decomposition products; therefore, in itself, independent of its cicatrization in the undoubted presence of "germs," I would count it, if not fatal to that theory, at all events difficultly explicable on it. Either the "germs" have what vitalists would call an elective, or patients must be credited with a repellent power, or we must take refuge in that last refuge of the baffled, an idiosyncrasy, to regulate their action. In the case of the eyelid, the girl was lodged in a highland hut, close, ill-ventilated, and overcrowded. No better nor more thorough *nidus* for propagating germs can be conceived; and I beg leave to state that turf-smoke, of such pungent intensity and denseness as would probably kill the more delicate mammalian organization unaccustomed to it, has no influence hurtful to vibriones. True, the girl was much in the open air, heartily fed on oatmeal and milk *ad lib.*, but the fact remains indisputable, that her conditions were such as could scarcely hinder vibriones, with all their penetrant power, from reaching the wound. I did shut up her conjunctiva, but I could not have done so hermetically, even if I had wished; some discharge did pass along with the lachrymal secretion, undoubtedly. The mammary case is an illustration of a wound treated under circumstances of a fair average sanitary surrounding, such as the better clothed classes are able to enjoy. In that case, there was considerable exudation, the covering did not become dry all at once. No one can call Tr. benzoin antiseptic in the sense that carbolic and iodine lotions may be, even if they are not proved to be so; and wherever and whenever organic fluid is found, there will be a nidus, even if your patient were on the summit of Ben Macdhui, for the propagation and influence of "germs," whatever their action. I leave the cases without further elucidation. If they need more, I must trust it to those, as I do willingly, who may read them.

ARTICLE VIII.—*Case of Chronic Pleurisy and Hydrothorax, proving Fatal by General Emphysema.* By J. ROBERTS THOMSON, M.D., L.R.C.P. Lond., Bournemouth.

I HAVE deemed the following case worthy of being placed upon record, because, though perhaps not unique, I can find no mention of any one similar in any work on which I have been able to lay my hands. The patient was but a short time under observation, and therefore no complete clinical history could be obtained, while the autopsy (procured with great difficulty) was, on that account, not so complete as one would have wished.

James Sharp, æt. 34, a bricklayer's labourer, native of and resident in Hampshire, was first seen October 2d, 1867. Patient had been hard at work all day, had been feeling very well, and returned home in the evening without making any complaint. Having taken a usual supper of meat and vegetables, he went out for a little walk, and returned about 9 P.M. feeling somewhat sick. His wife offered him some beer, but he would not take anything, and went off to bed complaining only of sickness. Shortly after he was in bed he retched a good deal, and while doing so felt as if something "gave way in his chest." Shortly after this he brought up a little blood—about two or three drachms—bright red and fluid. He then felt great pain in the chest, most intense at the scrobiculus cordis, and shooting from that point into the back. This was almost immediately followed by great difficulty of breathing.

As to the antecedent history, the following facts were elicited. He never had been robust: had lost some brothers and sisters of phthisis: and, in his own case, there was a history of old standing tubercle, though there had been absolutely no cough for more than two years. Never complained of any pain or uncomfortable feeling in the region of the heart, but for some years has had considerable palpitation and dyspnœa on exertion, or when walking up hill, etc. There is a history of occasional epileptiform seizures. His habits were not steady. He had been working hard during the afternoon of the day on which he was taken ill, in shifting heavy poles, but was not conscious of having received any strain. There had been no violent muscular exertion preceding the seizure. The bowels had been moved, but without effort; the sickness had not been severe, and there was no cough. I was called to him at midnight on October 2d. I found patient sitting upright on a chair. There was great orthopnœa, respiration 40 per minute, regular, shallow, costal. Inspiration and expiration were about equal in length, both noisy, but expiration most so. Percussion gave a hyper-resonant note over the right apex, and as far down on that side as to the level of the third rib. Below that point the note was dull, though not absolutely so. On the left side, percussion was natural. On auscultation—on the right side, at the apex—the respiratory murmur was harsh, almost bronchial, but no moist sounds or friction murmur. Lower down, on that side, the chest was not examined. On the left side the breathing was slightly harsh, much less so than on the right; but here, as on the other side, the sounds were much masked by the laryngeal noise. There was no cough or expectoration. The alæ nasi were dilating very forcibly. The pulse 100, soft, compressible, regular, very small; cardiac impulse diffuse, but not intense. No increase of cardiac dulness. There was a systolic bruit distinctly audible at the ensiform cartilage. At the left nipple no sounds could be differentiated, and as the patient was unable to restrain respiration, only a jumble of noises was heard at that point. The superficial vessels were slightly tortuous. There was no trace of an arcus senilis. The surface was clammy, moist, and cold. Patient moderately well nourished. There was emphysema in the neck, most marked on the right side. It extended over the clavicles on both sides. It was very distinct over the sheaths of the vessels. There was some over the chest. Tongue moist; œdematous, covered with a thick white fur. There was great pain in the chest, at the ensiform cartilage, also in the back, about the level of the second and third lumbar vertebræ. It was of a very intense aching character, and occasionally stitchy. Pupils were natural; consciousness complete; all the senses were unimpaired. The urine was not examined. I gave him opiates and brandy, and had him put to bed and propped up. The emphysema strongly increased. It became most marked on the right side, spreading much over the neck, but not much over the trunk, and not at all over the limbs. The pain gradually became lessened, but there was no diminution of the dyspnœa. When seen in the morning, the orthopnœa was very great, the alæ nasi were dilating very much; respirations 60 per minute. The pulse could not be counted. Complained of very great pain in the back, but very little in front. The emphysema was considerably increased, especially on the right side. The right side of the face, eyelids, etc., were much distended. Was ordered some sulphuric ether with cardamoms.

From this time patient sank rapidly, and died at 2.30.

The autopsy took place twenty-five hours after death, and at this my friend Dr Burslem was very kindly present.

Rigor mortis and hypostatic congestion were both well marked. The crepitating feel of the emphysematous cellular tissue was very distinctly perceived all over the anterior part of the trunk; but it did not extend lower than the groin on either side. All over the back it could also be felt, but here it was not so evident. In the neck, and over the face, it was very considerable in amount. There was none in either the upper or lower extremities.

On reflecting the sternum, there was a considerable amount of air found in the cellular tissue on its internal aspect.

The pericardium contained about an ounce of serous fluid. The heart was pale and flabby. The right side somewhat dilated. The aortic and pulmonary valves were competent. The tricuspid valve was natural. The mitral valve had one of its cusps converted into a small hard nodule. There were a few very small atheromatous patches in the aorta. The cardiac muscular fibres, under the microscope, appeared healthy.

The right pleural cavity contained several quarts of fluid, which was of a chocolate-brown colour, and had a peculiar sour odour. The lung was compressed against the vertebral column, and was bound down by adhesions, which were most dense at the apex. The pleura over the lower part of the lung, the diaphragm, and a small portion of the costal surface was covered with lymph, which was chiefly in long shreds, and extremely friable. There were no traces of emphysema beneath the costal pleura. The lower and middle lobes of the right lung were quite solidified, and extremely friable, breaking down into pulp under the least pressure. The apex of the lung contained some nodule of cretaceous tubercle. There were only one or two points over the upper lobe which were emphysematous. Some bronchial glands were found containing a dark fluid, similar to that which is found in spurious melanosis. Some of the bronchi contained some bloody mucus. The left lung and pleura were healthy, except that there were some patches of interlobular emphysema over the anterior aspect of the lung.

The lower third of the œsophagus, so far as its muscular coat was concerned, was dark in colour, and almost as friable as the right lung. The mucous coat was also dark, but was quite firm.

The abdomen was not examined.

We have here a case where extensive disease of the right side of the chest, almost total obliteration of the lung on that side, and mitral insufficiency, existed not only without any complaint on the part of the patient, but without his being prevented following an arduous and severe occupation. Although the cardiac lesion seems to make the immunity from suffering very remarkable, it is not to this that we wish to direct attention. Many cases are on record where severe thoracic disease has been present, the patient suffering nothing, and quite unconscious of its existence.

What I wish to direct attention to, is the mode in which this case terminated fatally. It is probable that the chest affection which has been described depended upon a low form of inflammation, the period of existence of which it is impossible to determine. It had not prevented the patient leading an active life. Beyond occasional dyspnœa and palpitation, no history could be elicited of any disease. On the afternoon of the day on which he was taken ill he underwent severe muscular exertion, without apparently any bad effects; but, in the evening, during an act of vomiting, which was described as "not severe," he felt as if something "gave way in his chest"—a little blood was expectorated, pain was immediately felt, and a train of symptoms followed which proved fatal in eighteen hours.

The *sudden* accession of all the symptoms prevents us ascribing their cause to anything anterior to the sickness; but it is difficult to understand why a mild effort of vomiting should produce results so grave, when severe muscular efforts had been quite innocuous but a few hours before. The presence of blood in the bronchi, the absence of air in the posterior mediastinum, and its presence in large quantity in the anterior, seem to indicate that it was in some point in the *anterior* part of the right lung that the rupture took place. Had it occurred through the root of the lung, we should have expected to find some evidence of it in the posterior mediastinum. The aperture was in all likelihood a small one, as the increase of the emphysema was not rapid ; so far, however, this can only be conjecture, as we were unable to find the place where the air gained access to the cellular tissue. The additional impediment thus produced to a circulation already much embarrassed, sufficiently accounts for the rapid loss of strength, and the early period at which the fatal issue took place.

From the first an unfavourable prognosis was given to the patient and his friends, though the cause of the emphysema could not even be guessed at. Whether one would be justified in judging unfavourably of cases of emphysema which are non-traumatic in their origin, must depend on concomitant circumstances; but a cardiac complication, as in this instance, must terribly increase the dangers of such a condition.

ARTICLE IX.—*Reduction of Dislocation of the Shoulder-Joint by Manipulation.* By JAMES PITCAIRN BOOKLESS, L.R.C.S.E., and L.R.C.P.E., etc., Kelso.

HAVING read, with considerable interest, an article in the number of this Journal for May 1867, by Mr Annandale, Lecturer on Surgery, etc., "On the Reduction of Dislocations of the Hip-Joint by Manipulation," I determined at that time to try what seemed so simple a method of operation whenever an occasion should present itself. Since then I have successfully treated various dislocations, principally of the shoulder-joint, and in them all put in force the usually received plans of reduction. Somehow I had not courage enough to try "manipulation" in any of those cases; but lately, an occasion offered where I *did* employ this method, with such success, that I have made a few notes of the case, in the hope that they may be of use in inducing others to substitute for the harsher methods of reduction a plan which, leaving out the question of chloroform, and according to the testimony of others than myself, is so simple, so speedy, and at the same time so sparing of agony to the sufferer.

On the afternoon of December 20, 1867, I was summoned to the country to attend a man who, according to the messenger's story, "had knocked his shoulder-blade out of joint." On arriving, I found upon examination that my patient, a stout strong man of about fifty years of age, had dislocated his right shoulder downwards, eight hours before, from a twist he had received by the act of pulling up a horse on the verge of falling. He was suffering great pain and there was considerable swelling of the arm. I immediately sent out to procure the assistance of some labourers in the field, so that they might be at hand in the event of my not succeeding with the method I intended to adopt, viz., "the heel in the axilla." My patient and I meanwhile were left alone, and I could not resist the temptation to try the plan I had so often thought of pursuing.

We both stood in the middle of the room, side by side, in a position where our right shoulders almost touched each other—so that, while my patient looked "eyes front," I looked in the contrary direction. I now seized his right hand with my left, at the same time separating his arm from the side. I then passed my right arm underneath his axilla—hugging the humerus. While I placed my right leg behind his right leg, I brought his hand underneath my right arm with my left. I now raised the humerus forcibly upwards with my right, and bent his elbow under the same arm, pressing downwards. At the same moment I directed him to fall away from me with his whole weight, and, in a moment, crack went the head of the humerus into its proper place. The operation scarcely lasted one minute; and so sudden had been the transition from agony to joy, that he laughingly informed me, that "it was just like gettin' a *teeth* drawn, but no half sae sair." The surprise of the good woman of the house and her followers, who rushed into the room in a violent hurry, ten minutes afterwards, can be better imagined than described.

The position to an onlooker is exactly that known by the name of the "bend" in the wrestling world, where you try to throw your opponent—failing in which you go yourself; the only difference being in this case, and that a most important one, that the operator is the only one trying to throw, as it were, so that, whether he succeeds or not, nobody falls.

Viewing this method of reduction as an example of a lever of the first order, the operator's left hand, with the assistance of the patient's body, forms the power; his right arm hugging the humerus, the fulcrum; and the head of the humerus, combined with the forces which retain it in its unnatural position, the weight.

ARTICLE X.—*Case of Pericæcal Abscess, caused by Perforation of the Vermiform Appendix, and proving Fatal by Rupture into the Peritoneal Cavity.* By ROBERT FARQUHARSON, M.D., Assistant-Surgeon, Coldstream Guards.

A. C., æt. 19, a delicate-looking lad, was admitted December 31st, 1867, for diarrhœa. The attack, although somewhat severe, was not accompanied by more than a moderate amount of pain, and readily yielded to astringents, with arrow-root and brandy.

All went on well until the morning of January 2d, when feverish symptoms set in, but there was nothing at this time in the appearance of the case to excite alarm.

January 3d.—He is decidedly worse to-day, with a hot, dry skin, furred tongue, and rapid pulse. Complains of violent abdominal pain; and, on examination, a rounded, circumscribed, and obscurely fluctuating swelling, about the size of the palm of the hand, is detected in the right iliac fossa. It is acutely tender to the touch, but the surrounding integuments can be freely handled, and there is no reason to suspect peritoneal implication.

In consultation, therefore, with my colleague, Assistant-Surgeon Myers, it was agreed to regard this as an abscess of probably superficial nature, with which active interference had better be deferred. Warm fomentations were accordingly applied, and a simple diaphoretic mixture ordered to be taken every four hours. The patient was allowed to partake freely of beef-tea and milk, and expressed himself as feeling more comfortable towards evening.

January 4th.—It is now very evident that general peritonitis has supervened, the belly being tympanitic and exquisitely tender, the skin dry and pungent, and the pulse small and sharp, 140. The urine is scanty and high-coloured, and the bowels have not been relieved for four days. All trace of the swelling noted yesterday has disappeared. Six leeches to be applied at once, and followed by a large poultice, and a pill containing a gr. of calomel, and one of opium, to be given every four hours. Brandy and beef-tea.

6 P.M.—Is very much lower, the pulse being almost imperceptible, and the countenance sunken and collapsed. Says he feels better, but the pain is unrelieved, and the tenderness extreme. The leeches have bled well, and the pupils are rather contracted.

9 P.M.—He is rapidly sinking, his voice being almost extinct, and the pulse felt with difficulty, and from this time he gradually grew weaker until his death at 2 A.M.

On post-mortem examination, evidences of acute peritonitis were found, the intestines being smeared with lymph, and matted together in several places by recent adhesion.

The abdominal cavity contained a considerable quantity of dirty serum, and from 12 to 15 ounces of pure pus—the latter being situated principally in the right iliac fossa, and recto vesical pouch.

Its source was readily traced to a large abscess in the pericæcal cellular tissue, which had ruptured on its anterior surface by a ragged aperture fully an inch long. This opening accurately corresponded to the situation of the swelling noted on the 3d inst., but there was no evidence of its having caused the slightest absorption of the abdominal walls.

The condition of the vermiform appendix could not at first be accurately determined; but, subsequently, by the kind assistance of my friends Drs Wilks and Moxon, it was found in the sac of the abscess, closely adherent to the cæcum, and presenting a rounded opening towards its extremity. The abdomen was not specially searched for foreign bodies, but the careful manner in which the fluid was removed by sponges, would almost have ensured the detection of anything of moderate size.

The cæcum and intestines were perfectly healthy, and the rest of the abdominal and the thoracic viscera were free from disease.

A case like the preceding necessarily leaves a somewhat unsatisfactory impression on the mind, for not only was the diagnosis at fault, but the treatment at no time seemed to effect any impression on the disease. But let the insidious nature of the attack, in conjunction with the lad's previous good health, be taken into account, and I think we must allow that the primary symptoms could hardly bear the construction put upon them by the subsequent progress of events. No doubt an earlier examination of the abdomen might have discovered cæcal tenderness or swelling, but in the absence of any complaint of localized pain, attention was not directed to such a probability. And even had a more correct view of matters been taken, it is satisfactory to reflect that no very special difference of treatment would have been adopted, for by the occurrence of diarrhœa the error of administering purgatives was fortunately avoided. It is probable, however, that the use of full doses of opium might have been beneficial, even before the symptoms of peritonitis came on.

The period at which the vermiform appendix gave way, is a point on which conjecture must be rather vague. Possibly this event, followed as it must have been by the leakage of a small quantity of fæcal fluid, and consequent formation of matter, may have occurred before admission, and not been incompatible with apparent health. But I am rather inclined to regard the abscess as acute and rapid in its course, and this view seems favoured by its sudden appearance externally. Its walls were unable to resist the rapidly increasing accumulation within, and, being denied an exit in one way, it discharged its contents at the point of least obstruction.

The propriety of surgical interference was carefully taken into consideration at the time, and although it was negatived on erroneous premises, I am still of opinion that nothing could have been gained by operation. There can be no doubt of the propriety of opening an abscess in this situation, which comes on as the sequel to long-continued typhlitis, but it would seem rather a hazardous proceeding to plunge a knife into an abdominal swelling, without any clear notion of its connexions or extent. And the examination showed that, had any such incision been made, my patient must have inevitably been exposed to all the risks of extravasation of the contained contents into the peritoneal cavity.

Regarding the importance of absolute rest in the treatment of such cases there can be no difference of opinion, for not only do rash movements interfere with nature's efforts at repair, but we have to dread the occurrence of syncope on the patient's sitting up. An example of this came under my notice several years ago, when a gunner in the Royal Artillery, suffering from acute peritonitis, suddenly expired on being raised in bed. He had been kicked in the abdomen by his horse forty hours previously, and no symptoms of collapse had occurred prior to the fatal issue. Recovery was unfortunately out of the question, as post-mortem examination disclosed extensive laceration of the intestines, but there can be no doubt that his death was hastened by the partial assumption of the erect posture.

It is certainly one of the most unsatisfactory points in medical practice, that the affected organ can rarely be kept absolutely quiet, and that, while the fractured limb or diseased joint can be secured from all disturbing causes, the inflamed lungs or heart must continue to perform their routine of duties. In intestinal disease, however, a more manageable state of affairs exists, and we are in the pleasant position of being able to help nature in her efforts to procure repose.

It used formerly to be supposed that perforation of the vermiform appendix was frequently, if not invariably, caused by the intrusion of cherry or date stones. Dr Wilks, however, in his Pathological Anatomy, successfully controverts these views, and shows that a morbid secretion from the mucous membrane of the part may form a hard waxy mass, closely resembling these foreign bodies. And Dr Dickinson, in vol. v. of the Transactions of the Pathological Society, records three cases bearing upon this question. In the course of his remarks, he refers to the large number of cherry-stones found in the evacuations of the Swiss peasant at certain seasons of the year, notwithstanding which this accident does not appear to be of frequent occurrence.

My own case affords no evidence on the point, but it is reasonable to suppose that some small concretion may have escaped into the abdomen. Without such explanation, it seems difficult to account for the causation and sequence of the various symptoms I have noted.

I should have felt much hesitation in laying this imperfect paper before the profession, had it not been for the kindness of Dr Wilks, who, at my request, furnished me with an able and complete *resumé* of his opinions on this subject. It is with great satisfaction that I avail myself of his permission to append his remarks, knowing how much interest all must feel in a record of the experience of so eminent a pathologist.

Remarks by Dr Wilks.

In my experience, cæcitis or typhlitis is a common affection, and one often fatal. In the latter case, when fatal in a few days by an acute peritonitis, the cause is generally involved in obscurity until the post mortem be made; there may be some indication of the seat of the disease by pain and swelling in the right iliac region, but this is not invariably the case. It is rather in the chronic cases, and those which end in suppuration, that the disease thus makes itself manifest. In both cases, when the disease remains local, as well as in that when sudden and fatal peritonitis is the only symptom, the cause in the majority of instances is in the appendix cæci; more rarely from ulceration of the caput coli itself, and from external injury. Regarding disease of the appendix as the commonest cause of typhlitis, it is probable that in those cases which are speedily fatal by peritonitis, the end of the vermiform process has ruptured directly into the abdominal cavity; whilst in those cases which terminate favourably by external abscess, the worm-like body has been fixed in a more external position, so that the cellular tissue around the cæcum has become the seat of the inflammation. There will, of course, be the intermediate cases, where the suppurative process may be for a time local, and, finally, by implicating the peritoneum, set up a fatal inflammation of that membrane.

The case of the ulcerated perforation of the appendix is due, in my experience, to the discharge of a calculus which has formed therein. I can call to mind several cases, both in hospital and private practice, where a concretion of this kind has been found on post-mortem examination, but I cannot at the present time remember a single case where I have met with a foreign body of the nature of a cherry-stone or date-stone, as some authors have described. Knowing the resemblance which concretions have to these bodies, I cannot but think some error has been made concerning them. When cut through, they are composed of layers of phosphate of lime; whether their nucleus may be a foreign substance, and thus have determined their formation, I cannot say, but I know that a peculiar waxy substance is secreted by the appendix, and this will subsequently cretify. In fatal cases, the appendix cæci is found perforated at its extremity, and a calculus is found loose in the abdomen. In the case where the disease is local and suppurative, I have only on one occasion been fortunate enough to discover the calculus.

I believe the diagnosis of typhlitis is to the last degree important, because, if perforation of any part of the intestines have occurred, or there be any inflammatory process going on immediately beneath the peritoneum, absolute rest is above all other measures the most important consideration to have in view, but it unfortunately so happens that constipation is a frequent accompaniment of the disease, and thus purgative medicines are too often given by the mouth as well as injections by the rectum. If the case clearly evinces the presence of peritonitis, such treatment would be universally condemned; but it so happens that symptoms of a more masked character do often exist, and measures are adopted which the medical man himself immediately withholds when he understands the nature of the case. I have seen several cases where the bowels were confined for several days with urgent vomiting, and the means I have mentioned were used, but, as it subsequently appeared, with much harm to the patient; for, as common sense would dictate, if a perforation have occurred, and the orifice be glued by recent lymph to some adjacent surface, the most absolute rest cannot be too resolutely enforced. In one case, an abscess had been the cause of the temporary obstruction, and in others I apprehend it was due to the atony of inflammation or an actual impediment from the exudation of lymph. I remember one case of a young man, with typhlitis and peritonitis, who eventually got well after the bowels had been confined for three weeks. I have always taken the credit of this recovery to myself, owing to the stand I made against the administration of purgatives, and my recommendation of opium in their place.

Part Second.

REVIEWS.

Researches in Obstetrics. By J. MATTHEWS DUNCAN, A.M., M.D.
Edinburgh: Adam and Charles Black.

OBSTETRICS owe their advancement in modern times chiefly to the scientific methods employed in their study. The manipulative skill and practical experience of the predecessors in the art have not only been increased and confirmed, but have been explained and supplemented by rules and laws founded on accurate physiological, pathological, and statistical data. It would be tedious and unnecessary to allude to the numerous authors at home and abroad, from the time of Naegele, by whose combined labours the modern science of obstetrics has been constituted; but it is satisfactory to observe that the study continues to be pursued with unabated ardour and encouraging success. To no one in recent times has obstetric science been indebted for more frequent and valuable contributions than to the author of the work now before us, which, as containing some of his most important views, we recommend to the earnest attention of our readers.

Thoroughness is, as the perusal of these "Researches" will at once show, the distinguishing characteristic of all Dr Duncan's work. This is the second volume which in the course of a few months the author has given to the profession, and, like the first "On Fecundity, Fertility, and Sterility," its various chapters are chiefly made up from papers published in different medical and scientific periodicals. These, we are told in the preface, however, have all undergone revision, some having been so altered and added to as to be almost new, while a few of the essays are now published for the first time. The book is dedicated to the Obstetrical Society of London, and is divided into five parts, to which is added an appendix containing references to papers by the author, which have been introduced into the volume, a catalogue of authors referred to, and an index.

It cannot be expected that in a notice like this we can enter with any minuteness into the numerous subjects of interest discussed in the work. To do so would occupy more space and time than we have at our command, and we must therefore content ourselves with a brief reference to a few of those topics which appear to us to be of greatest general importance. The first part treats of the Statics of Pregnancy, and includes chapters on the position of the Uterus, the position of the Fœtus in Utero, the position of the Pregnant Female, and the Mode of Presentation of Dead Children in Labour. Dr Duncan combats the theories of Dubois and Simpson regarding the position of the fœtus in utero in advanced pregnancy, and holds that "it is impossible to conceive of its maintaining any position but under the influence of gravity." While admitting that the fœtus probably possesses the power of effecting temporary changes in its position by its own unaided movements, he thus concludes:—"Those authors, who with Dubois strive to prove that the position of the fœtus is determined by its own motions, have first to prove that it could maintain any position whatever against gravity, without such constant efforts as voluntary muscles are incapable of, and of the actual presence of which no evidence can be furnished."

The paper on the Presentation of Dead Children in Labour is of much interest, and the conclusions drawn from a lengthened series of experiments made by the author with a view to elucidate this question are these:—1. The healthy fœtus floats obliquely, with its head lowest, in a fluid of its own specific gravity,—a position corresponding to that which it has in utero. 2. The fœtus has a specific gravity of about 1050, while that of the liquor amnii at the full time is nearly 1010. 3. Soon after the death of the fœtus in utero, changes take place in it which alter its position of equilibrium in a fluid of its own specific gravity, so as to be generally the reverse of that of the healthy fœtus, that is, so as to be oblique, with its head highest. 4. It may happen that an advanced stage of decomposition of the fœtus, with collapse of the cranium, may make its position of equilibrium when floating again oblique, with the head lowest. 5. These circumstances have probably considerable influence in determining the frequent mal-presentations of dead children.

Part II., in which the pelvis is studied with a view to obstetrics, is chiefly taken up with anatomical questions relating to the Os Sacrum, the Formation of the Rickety and Malacosteon Pelves, the Development of the Female Pelvis, the proximate cause of the Oblique-ovate Pelvis, and the Pelvic Articulations in Parturition. All these chapters are of great importance to the obstetrician; but in the third part of our author's work, "some points in the physiology and pathology of pregnancy and the puerperal state" are touched on, which are of more general interest to the mass of practitioners. The chapters on Menstruation in Pregnancy and Superfœtation are particularly worthy of attention. It is well known that many obstetricians deny the possibility of a true menstruous discharge taking place after impregnation has occurred, even in the earliest months,—a view which is founded on the belief that after conception takes place the cavity of the uterus is sealed up. Dr Duncan, from his own observations and a reference to those of Coste and others, clearly shows that up till the end of the first three months of pregnancy the menstrual blood may come from its ordinary source, seeing that it is not till then that the intra-uterine cavity is really closed by the complete fusion of the decidua vera and decidua reflexa. Should menstruation occur after this, it must find its way to the vagina "by rudely separating coalesced layers, and almost certainly leading to abortion, of which in such cases it would probably be regarded as merely the first symptom."

As regards Superfœtation, the fact already alluded to of the cavity of the womb being still open up till the third month (except indeed in cases where the ovum is inserted over the os) distinctly shows that there is nothing to prevent the spermatozoa from finding their way into the uterus up till this period at least. The great difficulty is as to whether ovulation goes on after conception. On this subject Dr Duncan says:—"It is known that during pregnancy, maturation of Graafian vesicles and the discharge of ovula do not generally take place. But the occasional occurrence in early pregnancy of menstruation, with all its ordinary symptoms, suggests the probability that at such times perfect ovulation may take place." There is therefore a strong presumption in favour of the possibility of superfœtation taking place in the early months of pregnancy; and, referring to recorded cases, our author remarks,—"If we suppose in an instance of this kind that the first child is born prematurely, but within the limits of viability, we thus gain two months; and if impregnation may take place between two or three months after conception, we have thus four or five months of interval accounted for between the births of successive viable infants. In the same way it is easy to explain the difference in apparent age and development often observed in twin-births; which may thus be instances not unfrequently of superfœtation; with this peculiarity, that both children, though conceived at different times, are born at or near the same time."

Dr Duncan's original observations on the condition of the internal surface of the uterus after delivery are already well known, and it is to him that we are chiefly indebted for the more correct knowledge of this subject which now prevails. The favourite illustration amongst teachers when treating of this subject used to be the erroneous one given by Cruveilhier, and others,—namely, the resemblance between the internal surface of the uterus after delivery, and the raw suppurating surface of a stump. It is upon this idea that the distinguished French pathologist bases his theory of the nature of puerperal fever, which he designates "the traumatic fever of the puerperal state." To Dr Duncan belongs the credit of pointing out the error of the comparison, and his remarks on this topic are worthy of quotation.

"Finally," he says, "did there exist after every delivery a wound of the enormous dimensions of the internal surface of the uterus,—dimensions not inferior to those of the wound produced in amputation of the thigh, it is difficult to conceive how parturient females should escape the frightful mortality succeeding that operation, or the like. It would be difficult or impossible to explain why, instead of one in every three or four dying, as after amputation of the thigh, there should be only one in every two or three hundred. It can scarcely be asserted that the shock produced, and the circumstances of the supposed uterine wound, are a whit more favourable to recovery in the obstetric than in the surgical patient. The explanation lies in the fact that the chief analogy of the internal uterine surface after delivery is not with a stump, so far as it consists of incised and denuded tissues, but only in both surfaces presenting numerous open veins liable to become inflamed, or to absorb the obnoxious materials which may be brought into contact with them."

Dr Duncan holds that the internal surface of the uterus is not denuded after delivery, but is covered by a mucous membrane, which he believes to be the remains of the uterine decidua, and which he states is found to be of greater thickness at the site of the insertion of the placenta than elsewhere. This opinion is confirmed by later observers, such as Robin and others, and may, we think, be safely accepted as the correct one.

Twenty pages of the work before us are devoted to an elaborate defence of the correctness of William Hunter's observations on the Mucous Membrane of the Body of the Uterus,—a labour of love which Dr Duncan has been prompted to accomplish from one circumstance amongst others, namely, that the great anatomist was a Scotchman, and a pupil of our illustrious Cullen and Monro.

A chapter of much value, in a practical point of view, is that which is taken up with the consideration of Imperfect Deliverance, in which some cases of great interest are recorded; but we hasten to glance at Part IV., which contains eight chapters on some topics in natural and morbid parturition. This is decidedly the most philosophical portion of the volume; and there are many important observations on the power exerted in ordinary labours, the greatest power of labour exerted in difficult cases, the power of the uterus to resist a bursting pressure, etc. In the experiments conducted by Dr Duncan for the elucidation of these subjects, he had the benefit of the great skill of Professor Tait, the distinguished Professor of Natural Philosophy in our University, and the results arrived at may therefore be relied on as strictly accurate. Another paper is written "to show that the obliquity, or lateral obliquity of the fœtal head, when passing through the brim of the pelvis, described by Nægele, by some of his predecessors, and by his followers down to the latest authors, does not exist in natural parturition; and that obliquity, or lateral obliquity, of the fœtal head, when passing through the outlet of the pelvis, not described by Nægele and most of his followers, does occur in natural parturition."

With that love of accuracy and definiteness, which is a special characteristic of Dr Duncan's writings, he seeks, in another part of the volume under consideration, to lay down in exact terms a precise definition of what is meant by the Caput Succedaneum and the Presentation. These terms are in constant use amongst obstetricians, but great vagueness prevails as to their meaning. Thus, for example, in speaking of the presenting part, authors are at variance as to what portion of the child is really meant; and, in order to get rid of this indefiniteness, Dr Duncan submits the following definition, which he says is a modification of Nægele's:—"The only definition," he says, "having sufficient exactness which I can suggest is, that it is that point on the surface of the child's head, or other part advancing first, through which the axis of the pelvis passes. It is evident that this corresponds to the part first touched by the finger, if passed in the axis of that part of the pelvis in which the presentation is lying. . . . The definition just given of the term presentation has the very great advantage of adapting itself to the fundamental writings of Nægele on the mechanism of parturition."

The essay upon the Production of Inversion of the Uterus is one of the most interesting in the book, and advances an original explanation of this untoward accident. We make a few quotations.

"Four kinds of uterine inversion occur after delivery. 1. Spontaneous passive uterine inversion. 2. Artificial passive uterine inversion. 3. Spontaneous active uterine inversion. 4. Artificial active uterine inversion. The only uterine condition essential to the production of all these kinds is paralysis, or inertia or complete inaction. This is the condition of the whole organ at the time of production of the first two kinds. In the two last kinds it is accompanied by uterine activity, and as these cannot co-exist in the same part, the paralysis is partial, and the activity partial. Action of the uterine wall cannot cause introcession of it. Activity of the whole of the uterus renders inversion impossible. Activity of a part of the uterus renders introcession of that part impossible. There must therefore be paralysis of the whole or of a part before inversion can be begun. . . . Spontaneous passive inversion occurs in cases of paralysis or inertia of the whole uterus; the organ being large, its walls lax, and capable of being inverted by little force. Bearing down produces in general collapse and compression of the organ; but it may produce inversion, if the depressing force is applied under favourable circumstances; and the inversion will be complete if the bearing down is strong and continued. Artificial passive uterine inversion demands little description. It is the kind of inversion commonly described by the older authors. It differs from the spontaneous passive inversion only in this, that foreign force replaces the bearing down. The foreign force may be applied from above by pushing, or from below by pulling the cord, or manœuvring with the placenta. It would be a more frequent occurrence than it is, were it not the case that the interference which tends to produce it also tends to bring on that general uterine action which prevents it. "Spontaneous active uterine inversion is the kind which modern authorship is bringing more and more into notice as the most common kind. . . . In this kind paralysis of the fundus or of a portion of it, probably of the placental portion, occurs. The state of the retentive power of the abdomen, or positive bearing down, leads to this portion projecting into the uterine cavity. It is seized by the adjacent contracting segments of the uterus, is pushed down and expelled through the os uteri into the vagina or beyond the vagina. . . . Artificial active uterine inversion differs in nothing from the kind last described, except in this, that the inversion of the paralyzed portion is effected by pressure from above, or by pulling on the cord or other interference from below."

These are the author's views of inversion, and we have thought it of importance to quote thus largely, chiefly because they are comparatively novel, and are not as yet generally adopted.

The concluding part of the work is made up of three papers on the Retentive Power of the Abdomen, some points in Uterine Metrology, and cases of Vagina Duplex et Uterus Simplex, and of Saccated Uterus,—the latter being of unusual interest owing to the rarity of the malformation.

We have thus glanced briefly and superficially, it must be admitted, at a few of the principal subjects treated of in this important volume, more with the view of bringing them under the notice of our readers than for the purpose of offering critical remarks. No more valuable contributions to the science of obstetrics have been offered to the profession in recent times, and no practitioner can flatter himself that he is abreast of the age who has not bestowed attention on Dr Duncan's Researches. The production of such a work will do much to maintain the reputation of our famous medical school, and cannot fail to enhance the renown of its gifted author. It is a good example of what scientific inquiry ought to be—logical, exact, concise; and on this account, as well as for its intrinsic worth, we earnestly commend the volume to the consideration of the profession. We ought to say that, as regards the type, the illustrations, the paper, and the whole get-up of the book, the publishers are entitled to praise.

De l'Homme, considéré sous le rapport des Facultés qu'il partage avec les Animaux, et qui assurent sa conservation particulière et la perpetuité de son espèce. Par M. le Dr Felix Voisin, Medecin en chef des aliénés de l'Hospice de Bicêtre, Membre de la Legion d'Honneur, etc.

Man, considered with reference to the Faculties which he shares with the Lower Animals, and which ensure his own preservation, and the perpetuity of his species. By Dr Felix Voisin, Chief Physician of the Lunatic Department of the Bicêtre, etc. Paris: J. B. Baillière and Son. Large 8vo, pp. 392.

This volume constitutes the third and concluding part of a work, entitled, "Studies on the Nature of Man: what are his Faculties? what are their Names? what is the number of them? how should they be employed?" One is not surprised to learn that such a comprehensive series of inquiries have been spread over a considerable time. Sixteen years have intervened between the publication of the first part of the work and of this, the concluding one. The author, in alluding to this fact in the preface, somewhat naïvely consoles himself with the reflection, that his "book is not the book of any one period: it is written for all time, for all places, for all men." Such a startling bid for universal immortality at the outset is calculated to excite our curiosity, and raise our expectations to a high point.

We have not had an opportunity of perusing the two first parts of the work, which treat of man in relation to his moral and intellectual faculties; but we must confess that, as regards this volume, our expectations have not been realized.

In a long introduction, M. Voisin explains the position which he takes up, and the ends which he proposes to himself in writing this work. He begins by stating that in every age there have been men of grand intellect, and of immense energy, who have striven to develop the instinctive, intellectual, and moral faculties of the human race, and to control their manifestations. The teachings of all of these he looks upon as "revelations," placing those of Moses and Christ in the front rank; and he considers that each in his time, and each according to the measure of his capacity, has been the instructor and educator of the human race. M. Voisin, however, does not hesitate to affirm, with their works in his hands, that these men are far from having enunciated all the principles necessary to direct in a proper manner the activity of our faculties. He considers that they have none of them solved the grand problem: the "law of activity appropriate to each of our faculties has not yet been promulgated, and the human race, growing old in its long infancy, still remains in ignorance of its proper life."

Will any one be surprised to learn, after this, that it is M. Voisin himself for whom has been reserved the rare honour of at length promulgating this law, for want of which the human race has so long languished in swaddling clothes? Our author has the enthusiasm and confidence, if not the modesty, of a great reformer. Every man, he observes, has his mission to fulfil: "and I proceed," he adds, "to acquit myself of mine. Strong in convictions which I have established upon numerous and incontrovertible facts, I do not fear that, from one end of my book to the other, I shall once be found to have contradicted myself. . . . Whoever reads this book with some attention will find there, I am sure, the seal of an upright and well-meaning man: the style is not elaborated, nay, it is often enough incorrect; but it is clear, comprehensive, distinct, and positive. If I rise to the height of my theme, I shall have laid the foundations of the new philosophy, of the practical philosophy. I shall have taught all who wish to live the life of a man: what they ought to do with their time, and with their faculties, for themselves, for their fellow-creatures, for external nature, and, in fine, for the order, the happiness, the fulness, and the morality of their whole existence."

Every reformer is allowed a certain license in depicting the evils which he hopes to cure; even Luther, we may suppose, made the monks a little blacker than they really were; still is not M. Voisin just a little hard upon the present representatives of the human race, when he says that they know neither their forces, nor their duties, nor their rights, nor their end, when he leads us to believe that, as a rule, a man's career is one of mistakes and extravagances; and that if he succeeds in ending it otherwise than by the scaffold, suicide, or a lunatic asylum, he may consider himself fortunate, and has chance to thank more than anything else?

It is consoling, however, to a certain extent, to be told that there are a *few* who escape these evils; but it is still more comforting to be assured by our author that he has discovered the grand remedy for them all. This is "to take man as he is, and to trace for him the law of activity of all his faculties." Before a poor, misguided man, however, can avail himself of this law, it is essential that he should, as a preliminary, know "the name and the number of his instinctive propensities, the name and the number of his moral sentiments, the name and the number of his intellectual powers." M. Voisin enumerates some twenty faculties in this volume alone.

One does not like to think how many there may be altogether; but it would certainly be enough to appal any one who was not a perfect prodigy at "mental" arithmetic. So far, we should be inclined to put M. Voisin down as a philanthropic enthusiast—as a man who, having seen a great deal of the evil and misery which result from the infraction of Nature's laws, has conceived the Utopian idea that the world is to be regenerated by teaching people the number of their so-called "faculties," and the law of their activity, as one would teach them compound addition, or the rule of three. But when M. Voisin gravely announces that he is not merely a reformer, but is the medium of a new revelation to man, we are tempted to think not only that he is an enthusiast, but that "much learning" has had the same effect on him as Festus supposed it had had upon Paul.

The book is divided into chapters, which treat of the various instinctive faculties; and after the account of each faculty, the law of its activity is given as a direct revelation, purporting to be spoken by the Deity himself. These constitute what M. Voisin calls the new tables of the Law—a sort of revised edition of the Decalogue brought down to the present day. Anything more repugnant to our feelings, to our sense of propriety and reverence; anything more out of place in a work which professes to be one of scientific importance, and written for a scientific audience, it would be impossible to imagine. At the best, it is a feeble and unscientific attempt to invest the author's opinions with a fictitious importance; and we cannot refrain from adding, they require all the support which such a device can give them.

These "revelations," which occupy nearly half the volume, are made up, for the most part, of the veriest truisms, mixed with a great deal of interrogation, exclamation, and expostulation, which borders on both the ludicrous and the blasphemous, and is very suggestive of the opposite side of the channel. The "revelations" might be summarized thus:—"You have been very bad boys, but you must be good boys in future; you must not neglect any propensity, but you must not abuse it by any excess:" which is no doubt very good advice, even if one fails to recognise in it a new "law of activity" which is to regenerate the world.

In the chapters devoted to the "faculties" themselves, M. Voisin gives an account of certain physiological propensities, and of certain qualities, such as "savoir-faire," "prudence," "circumspection," etc., which he considers as common to man and the lower animals. These are evidently based on the now almost exploded phrenological classification; and in accordance with phrenological views, a great deal of unnecessary pains is taken to prove that each of these "faculties" is inherent, and that their number is fixed and definite. We would only inquire, Why should "alimentivity" and "reproduction" be regarded as inherent faculties, and not other physiological functions as well? why should Nature have endowed man with a "faculty" for "destruction," while the faculty of construction, we are to suppose, is an acquired one? If "affection" be regarded as a "faculty," why not "hatred" or "dislike?" As to qualities being inherent or not, it is little more than a war of words. The capacity for developing and exhibiting, *under favourable circumstances*, any particular quality is inherent in the organism, and in this sense every "faculty" is inherent. But circumstances may not be favourable; the needed stimulus may be withheld. The "faculty" of reproduction, for instance, requires for its manifestation a complicated series of anatomical changes; and if these are interfered with, the "faculty" is not developed, in spite of its being "inherent." But many other "faculties" not considered by M. Voisin as "inherent," and more immediately the result of education or other external stimulus, are in reality just as inherent as "reproduction:" that is to say, in both cases the organism, by virtue of its inherent "capacity," develops in a particular direction in response to a particular stimulus.

Altogether, this is a most disappointing book; its chief value seems, indeed, to be to show what a scientific work should *not* be. It seems to us to achieve nothing, and to add little or nothing to our knowledge of the subject. The book is one which, from its title, is calculated to excite attention. The minute structure and functions of different parts of the brain is felt by every one to be the "coming subject" in physiology, and is, in fact, the one great physiological problem which remains yet to be solved. If any great advance is ever to be made in this superlatively difficult path of research, one great branch of the subject which we conceive must be followed out, is a careful comparison of the cerebral phenomena in man and in the lower animals, in connexion with the gradual development and differentiation of the various parts of the brain, as we ascend the scale of animal life. The limits of reflex, sensori-motor, and automatic action among the various members of the animal kingdom,—how far any of them display mental phenomena proper, and kindred questions, would fall to be considered in such an inquiry; and it was in this direction that we hoped M. Voisin's researches would tend. M. Voisin, however, bases his work on an assumption which quite accounts for his not adding to our knowledge of cerebral physiology. He considers that the anatomy and physiology of the brain are already completely elucidated; that nothing remains to be done in that department. The structure and arrangement of the brain, and the localization of the different faculties, are all quite settled and "accepted without opposition by the majority of savants." (!)

The solving of this great problem, towards which in this country, at any rate, it is thought that only a very few steps have as yet been made, M. Voisin attributes to the labours especially of Gall and Spurzheim. These men, with the other founders of the phrenological school, undoubtedly gave a great impetus to the more exact study of the brain and nervous system; but it seems to us, that while they did make many valuable observations, they mixed up with these so much hypothesis and groundless assumption as very much to weaken the force of their statements; and we are at a loss to conceive how any man can seriously maintain that, after them, nothing was left to be done. Upon what we cannot help terming this radical error of M. Voisin's, the latter bases another idea, in our opinion, equally erroneous—viz., that a complete scientific knowledge of the functions of the body, and the faculties of the mind, not only favours the development of the highest morality, but is essential to it. To such a length does M. Voisin carry this notion, that he regards the want of such knowledge as the real cause of miseries, disorders, and crimes, and views the imparting of the knowledge as a new revelation from heaven. "Either," he says, "I am far wrong, or the hour has come to give to science the character of a public revelation. This is a great—possibly a rash—undertaking, but it is to me a sacred duty." There is an element of truth in this, it cannot be denied; but this is not the place to attempt to show what a false basis science, taken by itself, would prove for a system of morals. We are sorry that we have not been able to speak more favourably of this work, for its author is evidently an earnest and a philanthropic man, who has seen a great deal of the darker side of human nature, and is deeply impressed with the fearful amount of vice and misery which still exists, in spite of our boasted civilisation and the progress of science. It is the work of a man who, feeling that something is far wrong, and that there should be some way of setting things to rights, dreams that he has at length discovered a royal road to perfection, and elaborates his dream with an enthusiasm which borders on monomania. Still the work is addressed to the Academy of Medicine, and put forward as a scientific work, and as such we must judge it. We cannot conclude, however, without expressing our entire concurrence with M. Voisin in one or two points to which he incidentally alludes. We think M. Voisin is quite right when he says that the study of "the higher life of the human species, the life of the brain," has been too much left to metaphysicians and psychologists, and that it pertains just as much to medical science as the study of the other functions of the body; and, he adds, "if this subject presents great difficulties, if we have as many systems of philosophy as of would-be philosophers, if science has not yet said its last word upon the nature of man, the fault lies chiefly with the medical profession, who have allowed men, strangers to the study of natural history and of physiology, to take upon them a part which belonged exclusively to themselves." Certainly the metaphysical method of inquiry, studying mental phenomena by processes of self-interrogation, has not led to very satisfactory results; it is but right that the inductive method of studying them as functions of the brain should have a fair trial.

We would just mention another point alluded to by M. Voisin, and which is intimately connected with social questions of immense importance. M. Voisin states as his deliberate opinion—and his position as physician to the Bicêtre gives him excellent opportunities of forming an opinion on this subject—that he considers the immense majority of our criminals are not constituted like other people; their moral organization is congenitally defective. Their instincts or propensities are unduly violent; their moral sense is little developed, and their intellectual powers are below par. This he considers to be independent of the effects produced by example, education, habits of life, etc., powerful as these undoubtedly are. We believe that there are few men who have had opportunities of studying individuals belonging to what may be called the "criminal class," who will not agree with M. Voisin in his opinion. This, of course, has a most important bearing on the subject of responsibility, and the mitigation of punishment, especially in capital cases. We have no hesitation in saying that in many, if not all, cases of frightful crime in which such an outcry is raised if an attempt be made to bring in a plea of insanity, the wretched criminal, if not in the ordinary sense of the term insane, is not responsible in the same degree in which ordinary people are so: his moral nature is congenitally defective. It may be long before public opinion comes to look at these cases in this light; but there is already a growing repugnance to carrying out the extreme penalty in such cases, which, in course of time, may lead either to the abolition of capital punishment, or to the adoption of the principle advocated by Dr Skae,—of degrees of responsibility as decided by a jury of medical experts.

1. *Neligan's Medicines, their Uses and Mode of Administration.* By RAWDON MACNAMARA, Licentiate of the Royal College of Physicians; Licentiate, Fellow, Member of Council, and Professor of Materia Medica, Royal College of Surgeons in Ireland, etc. Seventh Edition, 8vo, pp. 934. Fannin and Co., Dublin: 1867.
2. *The Indigenous Drugs of India; or, Short Descriptive Notices of the Medicines, both Vegetable and Mineral, in Common Use among the Natives of India.* By KANNY LOLL DEY, Graduate of the Medical College of Bengal, etc. 8vo, pp. 130. Thacker, Spink, and Co., Calcutta: 1867.

1. "NELIGAN'S MEDICINES" has so frequently been noticed in this Journal, that it is unnecessary to dwell on the general merits of the work. Since the death of Dr Neligan, two editions have appeared under the editorship of Dr Macnamara, and with the latter of these we are now concerned.

This treatise is subdivided into chapters, each of which includes a more or less detailed account of all those articles that possess the same medicinal action. The basis of the work is thus a therapeutical one; and to this no great objection could be raised were our knowledge of the action of medicines at all perfect, but while this perfection is, as yet, far from having been attained, the adoption of such a basis produces very few advantages, while it necessarily leads to confusion and to want of compactness. It is impossible, for instance, to treat of the therapeutical uses of *Antimonium tartaratum* without referring to its employment as a diaphoretic, as an emetic, as an epispastic, as an expectorant, and as a sedative or contra-stimulant; but it is surely a great disadvantage to adopt a system which requires these therapeutical applications of one remedy to be discussed in five different and widely separated portions of the book. We would also protest against this system of subdivision as unscientific, and as, therefore, tending to oppose progress in therapeutics. The various remedial effects are too sharply cut off from each other; and this is certainly objectionable, as it separates, with too great distinctness, many effects that are in reality due to the same action, as the diaphoretic, emetic, and sedative effects of *Antimonium tartaratum*.

To assign a proper position to a drug in this system is also a matter of some difficulty, and we are not, therefore, surprised to find that the decisions on this point are occasionally contrary to the opinion of the majority of writers on Materia Medica, and even of the editor himself. Few would admit that *Acidum carbolicum* is chiefly valuable as an astringent, and that it should be described in this class only, along with gallic and tannic acids, alum, and catechu; or that the action of *Chloroformum* is adequately defined as a sedative or contra-stimulant; or, to content ourselves with another out of many similar examples, that *Pepsina* is in its proper place among the special stimulants. The editor has placed *Ammonii bromidum* in the class of special stimulants, and yet he records his opinion of the erroneousness of his own decision in the following sentence:—"The best description that has been as yet given of the medicinal properties of this salt is, that it is a *laryngeal anæsthetic*, producing remarkably sedative effects over the convulsive and irritant affections of the larynx" (p. 618). There is no allusion to a stimulant action in the description that is given of its therapeutical uses.

When a treatise on Materia Medica assumes the bulky form of a work of reference, it is obviously unfortunate that it is insufficient for its implied purpose. The absence of references to the sources from whence statements and opinions are culled must render "Neligan's Medicines" a mere class-book. This is a grave misfortune, as the literature bearing on many of the articles that are treated of appears to have been carefully examined and judiciously made use of. We are strongly of opinion that, even for a class-book, it is important that references should be given. Nothing, for example, can be more unsatisfactory to the student than the occurrence of such statements as the following:—"In addition to these" (constituents of belladonna) "Luebekind has announced the existence of another principle, *belladonnine*, to which, probably, may be attributed a portion of its physiological effects" (p. 409); and, again,—"In Lepelletier's essay, two cases of pneumonia are mentioned, in one of which the pulse was reduced from 120 to 34 beats per minute in nine days, and in the other from 72 to 44 beats per minute in three days, under the use of continued doses of tartar emetic" (p. 492). Who is Luebekind, and who Lepelletier? What value is to be attached to their statements? Is belladonnine a neutral body, or is it an alkaloid? and, if the latter, is it a fixed or a volatile base? These, and the other numerous questions that are naturally suggested by these statements, must remain unanswered as far as any assistance that can be obtained from this book is concerned. We do not expect that every detail should accompany statements of this kind, for the limits of the work must prevent this; but if such statements are considered of sufficient importance to be made, they ought certainly to be supported by the means of verification, and of more ample consideration. There are several other statements which show that these criticisms are not uncalled for; but the assertion of the opinion that "the physiological researches of Professor Haughton leave no room for doubt that the true antidote" (for nicotia) "is strychnia" (p. 523), is alone sufficient to justify them. We are sure that an examination of these researches would convince many, as they have certainly convinced us, that Professor Haughton's investigations, though of great merit, are by no means sufficient to justify the expression of so decided an opinion.

We have termed the active principle of tobacco *nicotia*, and in this we differ from Dr Macnamara, who prefers the name *nicotina*. Until this alkaloid be elevated to an officinal position, the name that is applied to it may be, to a certain extent, a matter of choice. The Pharmacopœia committee have adopted the feminine termination to distinguish those vegetable active principles that are alkaline, and the neuter for those that are indifferent in reaction. The word *nicotina* is certainly feminine, but we believe *nicotia* is preferable as being more properly in harmony with the names of the other alkaloids, as *morphia*, *strychnia*, *conia*. *Nicotina* can harmonize with only *morphina*, *strychnina*, *conina*. We are also at a loss to understand why Dr Macnamara should prefer to use in different places, *quina*, *quinine*, and *quinia*, instead of uniformly using the Pharmacopœial name *quinia*.

Dr Macnamara has added, in this edition, two chapters,—one upon "Waters," and the other upon the "Administration of Medicines,"—and both of these contain much useful information admirably expressed. This edition contains, besides, a valuable appendix, consisting of a classified list of the chief plants used in medicine, together with the preparations of those that are included in the British Pharmacopœia. We observe several important omissions from this list, and believe that its value would be increased if all the medicinal plants treated of were included in it. We regret that the appendix of *formulæ* has been retained. A few typical *formulæ* are certainly useful to the student, but the immense number of these that this appendix contains is more than sufficient to serve any educational purpose, and it increases, in a very unnecessary manner, the already formidable dimensions of the volume.

We draw attention with pleasure to the valuable details that this edition contains on the therapeutical and physiological actions of remedies. These are sufficient to entitle "Neligan's Medicines" to occupy one of the first places among our works on the actions of remedies. The remaining contents of the book,—the definitions of drugs, the physiological properties, the botanical and chemical characters, the tests, the explanations of chemical processes, and the information regarding dosage and modes of administration,—have been prepared with evident care, and are in all respects sufficiently trustworthy to render this an admirable text-book for the student.

2. Many of the most valuable drugs in our Pharmacopœia are obtained from Hindostan; and it has long been suspected that there are others among those that are daily employed by the native doctors, or makeers, which might be advantageously adopted by us. The labour of collecting for, and presenting to the consideration of British practitioners, the evidence in favour of the value of these comparatively unknown remedies is, therefore, deserving of our warmest commendation. O'Shaughnessy, Waring, and others have done important service in this direction; but, without at all depreciating from the merit of these distinguished writers, it has been the good fortune of a native of India to bring before us the first comparatively complete account of the rich medicinal resources of that country. Baboo Kanny Loll Dey's volume is unpretending in form, but it possesses merits that should entitle it to careful consideration.

The arrangement of the work is alphabetical; the scientific and vernacular names of each substance are given before its description; the medicinal properties are then concisely stated; the preparations, and, in some cases, their doses, are mentioned; and, finally, the price of the drug in the Calcutta market is given. After a careful examination, we can confidently record our opinion of the accuracy of those statements of which we are entitled to judge.

Although a matter of very secondary import, we would draw the attention of the author to a slight but prominent inconsistency. The scientific name of each substance is given in the index and immediately before the description of its properties without any abbreviation, except in the cases of alum and sulphate of copper, which are written as *Alumen sulph.* and *Cupri sulph.* In the index this form is adopted without any indication of its being an abbreviation, while in both the index and the body of the book the other sulphates are recognised under their full names, as *Ferri sulphas, Potassæ sulphas*, etc.

We have read with great interest the descriptions of the medicinal uses of the drugs; and we are especially obliged to the author for his statements regarding their value as contrasted with that of their principal British officinal representatives. These statements have convinced us that not only is India in a great measure independent of extraneous resources for the supply of her medicines, but also that she is able to supply this country with many valuable medicines in addition to those for which we are already indebted to her. Among her indigenous plants is *Aloe Indica* (Ghrito-koomaree), from which an efficient substitute for our Pharmacopœial aloes may be obtained; *Calotropis gigantea* (Akund), whose bark possesses the valuable properties of ipecacuanha; *Datura alba* (Sada Dhatura) and *D. fastuosa* (Kala D.), both having an action similar to *D. stramonium; Dipterocarpus lævis* (Gurjun-tel), which supplies a balsam that compares favourably with balsam of copaiva; *Garcinia pictoria* (Ossara-rewund), which yields a gamboge at least equal to that of Siam or of Ceylon; *Nardostachys Jatamangsi* (Jatamangsi), whose roots have properties exactly the same as those of valerian; *Pharbitis nil* (Kalla-danah), from the seeds of which a most efficient cathartic oil may be expressed; and *Scilla Indica* (Koondroe), which is already extensively used in place of the officinal squill.

We are informed in the preface that the publication of this interesting volume is, in great measure, due to the liberality of the Indian Medical Department, and to the appreciative discrimination of Inspector-General Green. We feel sure that a wise liberality has been exercised. The publication of Baboo Kanny Loll Dey's volume will direct the attention of the medical officers to the indigenous drugs of India, their properties will be more definitely determined, and it is rational to hope that comparatively cheap substitutes will be thereby obtained for the costly English medicines now in use in that extensive empire.

Observations on the Nature and Treatment of Polypus in the Ear.
By Edward H. Clarke, M.D., Boston, U.S., America, 1867.

This is a monograph of 71 pages, entering fully into the nature, structure, and treatment, of growths in the ear. Dr C. mentions the diversity of opinion which has existed, and still exists, concerning them. He relates 13 cases in full, and then tabulates them so that they may be analyzed at a glance. From his cases it is evident that age has little influence as regards the occurrence of polypi; the age of his youngest patient was four and a half years, and the oldest forty-five, still he thinks, notwithstanding the figures, that aural polypi are more likely to occur in childhood than later in life.

One symptom always occurring, and that from an early period, is otorrhœa, and he remarks that "the condition of the meatus, which produces a discharge from it, and which may lead to a growth in it, can generally be brought back to a healthy state with care, if treatment is adopted as soon as there is any otorrhœa."

In none of the 13 cases did the polypi spring from the cavity of the tympanum, and in only 3 are they described as being near the membrana tympani, which latter, however, was perforated or totally destroyed in 7.

According to their structure, these growths are divided into two groups:—1. Fibrous, similar to Toynbee's fibro-gelatinous; and, 2. Epithelial, or fibro-epithelial, which are similar to fibro-plastic growths.

In every case the growths were completely eradicated after a longer or shorter period of treatment. In about half the cases, hearing was not improved by their removal; while in the other half it was so, more or less. Dr Clarke has made an alteration in the form of the snare of Dr Wilde of Dublin for extracting polypi, by having a handle placed at right angles to the steel bar carrying the wire, so that it can be introduced into the external meatus without any chance of the operator's hand interfering with his view; and the slide which moves the wire and contracts the loop is manœuvered by a trigger.

The treatment employed by the author consists of extraction, cauterization, injection into the substance of the tumour of Tr. Ferri Perchlor., and similar means directed to the growths themselves; while, to reduce inflammation and its results in the neighbouring tissues, counter-irritation, astringents, styptics, syringing, leeching, and constitutional measures are recommended.

On the whole, the subject is well treated; and this contribution to aural surgery deserves the notice of those specially interested in disease of the ears.

Plastics: A New Classification and a Brief Exposition of Plastic Surgery. By DAVID PRINCE, M.D. Philadelphia, 1868.

THE *first* portion of this work consists of an Introduction, which treats of,—1*st*, The definition of plastic surgery; 2*d*, The general hygienic conditions favourable to success in plastic surgery; 3*d*, The general therapeutic indications; 4*th*, The local therapeutics.

Under these heads the well-established principles in regard to the treatment of wounds are discussed, but we have not been able to find anything new or original in the author's remarks on this subject. Dr Prince has apparently not yet studied Professor Lister's important observations on the "antiseptic" treatment of wounds.

In the *second* part, a "philosophical" classification is given. In this classification Dr Prince distinguishes "six general methods, with their varieties." The six methods are,—

1. Sliding in a direct line. 2. Sliding in a curved line. 3. Jumping (Indian method). 4. Inversion or eversion. 5. Taliacotian (the part being obtained from a distance). 6. Grafting.

We do not attempt to enumerate the *varieties*, but would recommend those of our readers who are interested in the subject to study Dr Prince's classification, which is distinct and comprehensive.

Cicatrices form the subject of the *third* portion of the book. We agree with the author that cicatrices, even of large size, when producing deformity by their contraction, may be removed with great advantage, and the deficiency left by their removal relieved either by granulation—proper means being used to prevent the reproduction of excessive contraction—or by transplanting a healthy flap from neighbouring parts. In addition to the author's testimony on this point, the operations of Mr Butcher, and operations which we ourselves have seen practised, prove the success of this method in suitable cases.

An important fact in connexion with cicatrices is the one observed long ago by Mr Syme,—namely, that, as long as a cicatrix is *recent*, it readily permits of stretching by the employment of some simple mechanical means.

The remaining chapters contain a description of the plastic operations for the relief of the deficiencies and deformities of the nose, eyelids, mouth, lips, bladder, and penis. The author's account in these chapters of the various plastic operations which have from time to time been suggested by surgeons, and his remarks upon them, are concise, and the accompanying illustrations are good and intelligible.

We regret, however, to find Dr Prince advocating the removal of the centre portion of the lip in the operation for double hare-lip, a proceeding which, in our opinion, is quite contrary to the principles of sound practice. If Dr Prince could see the admirable results obtained in this school—thanks to the labours of Mr Syme—by retaining the centre portion and adjusting it so that it assists in filling up the cleft, he would, we venture to hope, change his views on this point.

Dr Prince deserves the thanks of the profession for his labours in this department of surgery; and we trust to receive at some future time the results of his further experience in connexion with plastic operations.

Cancer of the Uterus and other parts. By A. WYNN WILLIAMS, M.D., etc. London: Henry Renshaw: 1868.

WHATEVER the profession may be, the public is not sick of cancer-curers. These gentlemen hold a place in medical literature that we shall not characterize. The little pamphlet before us is the last bid for the benefits which we have no doubt cancer-curers get. Of course they are jealous of one another, and especially of their immediate predecessors, whom, above all things, they must displace. Here is a passage from Dr Williams, which illustrates what we have said:—

"The attempt to destroy cancerous growths by injection is not novel. For instance, Dr Broadbent has lately advocated the use of acetic acid; but I am somewhat inclined to think that the idea of injecting the tumour occurred to him in consequence of a conversation I had with him, when my colleague at the Western General Dispensary, on the feasibility of injecting cancerous tumours with bromine. On observing the action of acetic acid on cancer cells under the microscope, he was led to believe that it would destroy the life of the cell, causing it to shrivel up, and that it might ultimately be removed through the means of the circulation, without setting up suppurative action in the part. How far this hoped-for result may have been realized in other hands I am unable to say, it has however not been realized in mine; nevertheless, the idea was a most happy one, and ought to have been followed by greater success. It is by such means as these, and by constitutional treatment, that I hope some day to see all this fearful class of diseases made amenable to medical treatment."

Dr Williams need not be jealous of Broadbent's reputation. He has a long list of predecessors, all equally loud and careless in statement, and equally fitted for oblivion as cancer-curers. Some of them have the merit of having made careful and good trials, a merit we cannot award to Dr Williams, judging from his statements. Were we so foolish as to recommend him to republish his account of his treatment, we should advise him to give the world an account of one case, sufficiently well attested as a case of uterine cancer in a progressive condition, in which his treatment has produced a cure which can be well attested one year after its performance. To ask one case is surely little. It would not convince us. But it would be encouraging, and it would be worth a thousand of these pamphlets. Here is an account by Williams of one of his cases, almost the only one he gives. We forbear to characterize it. Speaking of medullary cancer of the uterus, he says:—

"This form of cancer may be confounded with syphilitic or other chronic indurations. Indeed, in one instance, when a respectable married woman came to me, supposing herself to be suffering from constitutional syphilis communicated by a former husband, I was myself misled until the disease failed to yield to the usual remedies; when, finding these fail, I treated it as a case of medullary cancer with the happiest results."

Here is his account of the two cases of scirrhus of the uterus which he says he has met with :—

"Scirrhus or hard cancer, as before stated, seldom attacks the uterus, and, when it does, the patient rarely applies for relief until the disease has become so far advanced as to be beyond the possibility of cure. In one of the cases I met with, the whole uterine tissue was infiltrated with the carcinomatous deposit, feeling to the touch like one of those masses of nodulated ironstone seen in the British Museum. The other was the case of a middle-aged woman who had had the neck of the uterus incised some two years before consulting me. The remaining portion of the neck was converted into a hard solid mass."

To conclude, we present our pathological friends with a description of colloid cancer of the uterus. If not edifying, it is not a tedious account.

"The colloid variety is also of very rare occurrence in the uterus. In the only case I have witnessed, there was an opaque fibrous-looking mass, about the size of a small walnut, imbedded in the posterior lip of the uterus, around and in close contact with which were seen two or more semi-transparent cysts the size of large peas."

Dr Williams says he may be, and he hereby is, charged with too great precipitancy in advocating a mode of treatment which has not been longer submitted to the test of time.

Catalogue of the United States Army Medical Museum. Prepared under the direction of the SURGEON-GENERAL, U.S. Army. Large quarto, pp. 960, illustrated. Government Printing Office, Washington, 1866–7.

THIS very large and magnificent catalogue is divided into three distinct sections, prepared by different authors, devoted to the surgical, medical, and microscopical sections of the museum respectively. Of these, as might be expected, the first—the surgical section—is by far the largest, and consists almost entirely of specimens obtained during the fearful carnage of the great war. Including specimens, casts, photographs, and weapons, the surgical section contains very nearly 5000 distinct objects; and considering the enormous difficulties under which many of these were obtained, during the hurry and confusion of battles and retreats, it is astonishing how definite and precise most of the little notices are, even as to the initials and regiment of the original owners, and the circumstances under which the specimen was obtained. At least two-thirds of the preparations are examples of gunshot injuries of bones and joints; most of them are dry specimens; many show the balls *in situ*. The method of arrangement in the catalogue seems very simple. The first great divisions are marked by Roman numerals, and refer to the part injured, such as cranium, shoulder-joint, hand, abdomen, etc. Next under each, in large Roman capitals, the specimens are classed according to the nature and cause of the injury—thus, under I, Injuries and Diseases of the Cranium, we find A, gunshot injuries, etc. A still further subdivision, in smaller Roman capitals, enables similar conditions of wounds to be grouped together, thus: A, contusion; B, penetrating fractures; while, lastly, with small letters, we have the final groups according to treatment adopted: *a*, primarily fatal; *b*, operated on by trephining.

The information given with each specimen is of necessity very brief. It consists first of a description of the specimen, the operation, if any, that had been performed, and any important collateral facts discovered at the post-mortem examination. Then the initials of the patient and the number of his regiment, with the letter of his battery or company; date and battlefield on which wound was inflicted; date of operation; date of death or recovery; name of operator, and name of contributor. Considerable statistical value must be granted to cases thus reported when the report is complete, and much interesting material may accordingly be found in the catalogue. For example, of twenty-seven specimens of fragments of bone and discs removed by trephining for gunshot injury of the cranium, we find that five recovered, twenty died, and in two cases the result was unknown.

Some of the notices, pithy as they are, are very curious and interesting. Here are a pair, both illustrative of the doings of a certain civil practitioner in Washington:—

> "2263. The right carpus and metacarpus, and portions of the bones of the forearm; the radius is fractured for one and a half inches, and several of the carpal bones are broken. The point to be observed in this specimen was primary amputation at the insertion of the deltoid. Lieutenant S., 14th N.Y. State Militia, amputated by a civil practitioner in Washington, and contributed as a surgical curiosity.—*See* 3288, from the same operator. 3288. The bones of the right foot and leg, and the two lower thirds of the femur. The tibia and fibula were fractured in the lowest third by the subject falling in the street while intoxicated. An oblique fracture of the tibia extends into the ankle, and the fibula is transversely fractured at the junction of the upper thirds. A civil practitioner cut off the extremities of the fractured bone, and the next day amputated the femur in the upper third, for no assignable cause. Death followed."

In pleasing contrast with the effects of such *radical* treatment is the following instance of *conservative* surgery:—

> "1738. The head and five and a half inches of the shaft of the right humerus excised for fracture by shell. . . . This man feeds himself, and can use his arm with tolerable facility, and is able to lift and carry very heavy weights without the assistance of an apparatus.
>
> 4711. Shows some of the bones from a case almost unexampled. Private B. F., 2d Minnesota Cavalry, 26, exposed, from 11th to 17th December, to a snowstorm,

in which both hands and forearms were frozen to the middle thirds, both feet and legs to the upper thirds, both knees over and around the patellæ, and both alæ and the tip of the nose. The gangrenous parts of the nose fell away. The whole four limbs were removed at different dates. The conical stumps of the four limbs were covered with solid cicatrices, and the patient entirely recovered."

Many other interesting cases might be quoted, had we room, but the above must suffice. The preparation of the catalogue of the surgical section was entrusted to Assistant-Surgeon Alfred A. Woodhull.

The medical portion of the catalogue is interesting, chiefly from the series of preparations of the viscera, illustrating the morbid appearances in camp-fever and dysentery. It is enriched by some exceedingly good plates. It has been prepared by Assistant-Surgeon J. J. Woodward.

The microscopical section, prepared by Assistant-Surgeon Edward Curtis, has a histological, rather than a pathological, interest; but seems to contain a collection of preparations which will prove exceedingly valuable for teaching purposes.

The whole work is most creditable to the Surgeon-General's office; in paper, size, and type, it leaves nothing to be desired.

Part Third.

MEETINGS OF SOCIETIES.

EDINBURGH MEDICO-CHIRURGICAL SOCIETY.

SESSION XXVII.—MEETING VIII.

6th May 1868.—Dr Omond, *President*, in the Chair.

I. REMARKABLE POLYPUS UTERI.

Dr Matthews Duncan exhibited a polypus of the size of a small hen's egg, and which resembled a false fibrous polypus, having no investment of mucous membrane or of proper uterine tissue, which he had removed lately from the vagina of a young unmarried lady, whom Dr Crawford of Peebles had sent to him for the purpose. Its stalk, closely resembling the pedicle of an ordinary polypus, passed through the healthy cervix uteri. The tumour was removed by avulsion, no cutting being required.

The patient had a fetid discharge. This arose from the state of the tumour. Its section was not like that of a fibrous tumour, being less uniform. The tumour was much more friable than a fibroid. Its lower parts, as seen in section, were intensely red from bloody congestion. The inferior surface of the tumour was invested with a sphacelated greenish layer, nearly a line thick in some parts. This layer was at some points detached, at others adhering only by a thin yellow semi-purulent stratum. The mass of the tumour was easily broken up into irregular bits.

About a fortnight after the operation, she ceased to have any kind of vaginal discharge and felt quite well. At this time the cervix uteri was elongated, movable, and healthy. No trace of the pedicle of the polypus could be felt. The body of the uterus was bulky, not so mobile as is natural, and partially retrosected.

Dr Duncan, at first, supposed he had to do with a fibroid, altered by the inflammatory process. But careful microscopic examination of it led to some doubt on this point. For while Dr Grainger Stewart regarded it as presenting under the microscope appearances closely resembling those of some forms of scirrhus, a further careful examination by Professor Turner revealed conditions as follows:—Thin sections of the tumour, when examined with a magnifying power of 200 diameters, were seen to consist of a dense network of fibres belonging to the white fibrous form of connective tissue, which disappeared under the action of acetic acid. In the fine meshes of the network small pale cells, about the size of a human white blood corpuscle, were seen in abundance, some circular, others oval, others elongated into the caudate form.

II. ENUCLEATION OF TWO FIBROUS TUMOURS AFTER DELIVERY.

Dr Matthews Duncan exhibited a large fibrous tumour which he had removed by partial enucleation. The woman, æt. 32, had been delivered after a long but natural labour of her first child. Twenty-four hours after delivery, uterine labour-like pains came on. This was soon followed by some continued hæmorrhage. The hæmorrhage ceased when the tumour escaped the circle of the dilated os uteri, beyond which it did not advance. After seventeen hours of this kind of labour, Dr Duncan removed the tumour and another much smaller one by avulsion. There was no hæmorrhage. The case occurred in the practice of Dr Moffat of Falkirk, who recognised both its history and nature.

The tumour was of a flattened globular shape, and measured 4½ inches in its smaller diameter. It was very lacerable, a circumstance which threatened to cause difficulty in removal. Examination of it showed that about half of it was enucleated in the operation. This was effected by seizing the lower spontaneously enucleated part by the hand, and using the necessary force for pulling it out of its bed. Flaps of uterine tissue which had embraced it were felt after its removal. The patient did well.

On a section being made through the tumour, about a third of it, being the lower spontaneously enucleated portion, was seen to be so injected with blood as to present a hæmatoid appearance. This was easily accounted for, its conditions being mechanically like those of the scalp when a caput succedaneum is formed in labour. The rest of the tumour presented a section like that of an ordinary fibroid, only it had more of a pinkish tint than usual, and besides it presented well-marked patches of yellow tissue, distinguished from the rest by colour and opacity. Professor Turner examined the tumour microscopically. He found it composed of nucleated connective tissue, intermingled with unstriped muscle. The muscular element was especially found in the yellow masses, and in them could be seen in great perfection fatty degeneration of the muscular fibre. The demonstration of the muscular fibre consisted not only in the recognition of the pale bands with their rod-shaped nuclei, but in the isolation of many specimens of fusiform contractile fibro-cells. In those places where the fatty changes were most marked, the fat granules obscured the muscular structure.

These microscopical details were particularly interesting, because the tumour belonged to a uterus at the term of pregnancy. They confirmed the observations of Bristowe and others, regarding the changes in a fibrous tumour in a uterus in that condition.

III. *Dr A. G. Miller* showed a specimen of

LARGE SEBACEOUS CYST FROM THE UPPER EYELID.

Dr Miller stated that he had removed this tumour from the upper eyelid of a man on the 28th April. It was of sixteen years' growth, and when removed was the size of an ordinary hen's egg. It is one of the largest ever removed upon that region. It was situated under the orbicularis palpebrarum. The necessarily large wound was dressed with dry lint, and, almost without suppuration, healed in five days.

IV. COMPOUND DISLOCATION OF THE THUMB.

Dr Miller also showed the case of a mason who, on the 24th of January last (the day of the great gale), received a compound dislocation of the thumb of the right hand. The wound was 2½ inches in length, and the distal extremity of the proximal phalanx protruded to the extent of half an inch. The projecting portion of bone was removed, the wound washed out with carbolic-glycerine, and dressed with carbolic ointment spread on tinfoil. In three days the deep part of the wound was healed, and in three weeks the wound was perfectly healed. There is now a perfectly useful joint; the patient having been able to work and write as well as usual a month after the injury. He never suffered any pain.

Dr Miller remarked on the apparent anæsthetic influence of carbolic acid when applied to wounds. This, he thought, was owing to its destructive action on the nerve fibres, especially like creosote. He referred to this case, and one mentioned in the *Lancet* for 2d May, as examples of this action.

V. *Dr Argyll Robertson* exhibited a minute

CALCULOUS CONCRETION FROM THE MEIBOMIAN DUCT.

This specimen, which he had lately removed from a patient, was remarkable for its great rarity. It had given rise to obstinate ulceration of the cornea.

VI. *Dr Mathews Duncan* then read a paper

ON THE UTERUS, AS MERELY FIXED BY ADHESIONS,

which will appear in next number.

Dr Stephenson corroborated the statements made by Dr Duncan. The cause of pain occurring in certain cases, and being absent in others, of uterine adhesions, was very obscure; but Dr Stephenson thought Dr Duncan went too far in objecting altogether to such terms as irritability—which, although it did not explain, served to designate a peculiar morbid condition.

VII. *Professor Bennett* then read a paper

ON THE PATHOLOGY AND TREATMENT OF URÆMIA,

in which he gave an account of the principal opinions entertained in regard to the pathology of uræmia; and concluded by the narrative of three cases which had occurred in his wards in the Royal Infirmary. Dr Bennett, in the first of these cases, when the patient was in extreme danger and apparently moribund, had resorted to blood-letting, with the most astonishing good effects. In his other cases, venesection had not proved successful. He had not yet been able to determine exactly in what kind of cases of uræmia blood-letting should be employed; but he suggested the inquiry as a very important one, to which these cases were brought forward as a contribution.

Dr Warburton Begbie had listened with much gratification to Dr Bennett's communication on a subject of very great interest and importance. The results of carefully conducted experimental inquiries were unfortunately so conflicting, that we are still unable to say with anything like certainty what the precise morbid condition of the blood in the so-called uræmic convulsions or in uræmia is. The theory that the retention of some urinary constituent or constituents in the blood gave rise to the altered nervous and muscular actions, was in all probability correct, but the question what the precise constituents are which thus operate, is not so readily answered. It is doubtful, for example, if it be urea at all, while it may very possibly be the creatine and creatinine, or some of the other extractive matters of the urine. Hence the propriety of using the term urinæmia in preference to uræmia. Dr Bennett had alluded at some length to the view advocated by Dr Owen Rees, namely, the possible dependence of the so-called uræmic convulsions on a spanemic or watery state of the blood. He (Dr W. B.) was inclined to ascribe considerable importance to this view. Dr Warburton Begbie was pleased to hear Dr Bennett speak of the good effects of blood-letting judiciously employed in certain cases of so-called uræmic convulsions. No one would think for a moment of abstracting blood in an instance of very advanced renal disease in which coma or convulsions had occurred,—the pale, almost bloodless countenance and surface of the body forbidding such interference. But it is quite different in cases of violent convulsions, usually coming on suddenly, attended by great turgidity af the vessels of the head and face, and congestion of the eyes, in which he believed blood-letting to be not only an available means of treatment, but, in truth, our *summum remedium*. Instances of the kind had occurred to him in practice, particularly after scarlatina, in which he had no hesitation in saying that, without this treatment, the result must have been fatal, the end seemed to be so imminent. In these cases all other remedies had already failed; while bleeding was speedily followed by a cessation of the serious nervous disturbance, and complete recovery.

Dr Matthews Duncan was pleased to hear the recognition of the value of bleeding in recent cases of uræmia in strong persons, now made by Drs Bennett and Warburton Begbie. The most frequent and most characteristic cases of uræmia occurred in connexion with pregnancy, parturition, or childbed; and, in such cases, when eclampsia occurred, until lately, no remedy had been so much relied on and valued as blood-letting. Recently, since the introduction of the use of chloroform in such circumstances, bleeding had, in many quarters, without any sufficient reason assigned, been laid aside, and Dr D. was sure, put aside without sufficient grounds. It was a valuable remedy in properly selected cases. Dr D. had seen it useful. He had seen the use of chloroform in such cases sometimes apparently advantageous, sometimes apparently injurious. Cases for its use required also to be selected and watched.

Dr D. did not think, as the author of the paper did, that coma or convulsions, or both, were of the essence of uræmia. Cases of the kind, with the blood poisoned by urea, and with carbonate of ammonia in the blood, as shown in the usual way, went on to cure or to death, sometimes without coma and without convulsions, and without dropsy. In cases in which coma, convulsions, paralysis, and dropsy came on, the progress of the disease was not invariably attended by such occurrences. In the present state of our knowledge of uræmia such occurrences as coma, convulsions, paralysis, dropsy, though often occurring and forming prominent part of the disease, did not form essential parts. The uræmia might go through its course to recovery or death without these epiphenomena appearing.

Dr Sanders observed, that in regard to the pathology of uræmic convulsions, it was well known that opinions were greatly divided, and the results of experiments had been contradictory or inconclusive. The discrepancies could to some extent be reconciled or accounted for; but at the present time no one theory was supported by sufficient evidence to be held established. Dr S. had for some time past been inclined to ascribe much importance to the anæmic and watery condition of the brain as the more immediate cause of the convulsions, and he thought it probable that the urea, by facilitating transudation through the minute bloodvessels, might, under certain conditions of susceptibility of the nervous centres, give rise to œdema of the brain and the consequent convulsions, just as under more ordinary circumstances the retained urea, occasioning serous transudation in other parts, produced dropsy, vomiting, diarrhœa, etc.

Dr S. considered that, at the present time, clinical cases carefully observed and recorded were of the utmost importance for the progress of medicine.

From what he had been able to gather in listening to Dr Bennett's report of his cases, however, he regretted that he could not regard them as sufficiently definite or conclusive. In none of the cases had the blood drawn been tested for urea—a most important omission, especially when it was by no means clear that the cases were all really examples of uræmia. In one case (the second related) in particular, uræmia had not been even suspected during life. According to Dr Bennett's account, the case, as observed clinically, was one of pneumonia, diagnosed to be free from complication. The urine, on examination, contained no albumen. Contrary to Dr Bennett's expectations, the case, which appeared to be progressing favourably, proved fatal; yet the symptoms observed excited no suspicion of renal complication. At the autopsy, however, Dr Grainger Stewart had detected "cloudy swelling" of the epithelium in the uriniferous tubules, and on examining urine taken from the bladder had found it albuminous. On these post-mortem data, Dr Bennett had founded the diagnosis of acute desquammative nephritis, and consequent fatal uræmia. But apart from the objection that diagnoses made only post mortem, without corresponding symptoms being observed during life, were not satisfactory, Dr S. was not convinced that the facts discovered at the autopsy were sufficient to prove the conclusions drawn from them. A granular state of the tubular epithelium is often met with in fatal cases of pneumonia, without either albuminous urine or uræmia having been present during life; and Dr S. was not prepared to admit that a "cloudy swelling" of the epithelium was equivalent to "desquammative nephritis," as Dr Bennett had called it. And, on the other hand, the detection of albumen in the urine of the bladder after death was fallacious. It was frequently found that the urine which had been quite healthy during life, was coagulable when taken from the bladder at an autopsy; apparently in consequence of the serous transudation which takes place into the bladder, as into other organs after death. So that this observation was not a valid proof of albuminous urine existing during life, especially as no tube casts had been detected to confirm the supposition of desquammative nephritis. On such data as these, inconclusive in themselves, and unsupported by clinical observations during life, it was somewhat hard to believe that a patient with healthy urine, and without renal symptoms, should, between one afternoon and the following morning, be attacked with desquammative nephritis—that the nephritis should be followed by uræmia—and that the uræmia should prove fatal Was it not more probable that the man died of the pneumonia?

Dr Grainger Stewart explained, that he regarded this "cloudy swelling" as evidence of commencing inflammation of the kidney tubules, and that he thought the albumen found in the urine at the autopsy was in greater amount than he should expect from post-mortem serous transudation. At the same time, he admitted that he had found more advanced changes in the kidney without uræmia during life.

Dr Sanders said the case must, at least, be considered a doubtful one. In regard to the practical part of Dr Bennett's paper, the propriety of venesection had found several advocates in recent years, although the general practice was opposed to its use. Dr Richardson had recommended it, and in an able communication read to this Society about three years ago, Dr Haldane had discussed the pathology of uræmic convulsions, and had given his opinion in favour of bloodletting in certain cases, and for the reasons stated in his paper (*Ed. Med. Jour.*, April 1865). Dr Sanders had on one occasion used venesection with marked benefit in uræmic convulsions. About five years ago, when Dr Arthur Gamgee was acting as his resident physician, an ostler was admitted into his ward in the Royal Infirmary. The man was of intemperate habits, and at eight o'clock in the morning, while at work, had been seized with convulsions. Repeated attacks of great severity followed at intervals of about ten minutes till he was seen at mid-day visit. There was intense venous congestion of the face and body during the paroxysms. Several remedies (purgatives, enemata, etc.) had been tried without success. Thereupon venesection, as a last resource, was suggested, if he recollected aright, by Dr Gamgee, and at once put in practice. Just as Dr Sanders inserted the lancet into the vein of the arm, a convulsion came on; but the blood flowed freely, and after a few seconds the attack was suddenly arrested. The effect was immediate and striking. About eight ounces of blood were taken, and there was no return of the convulsions. The patient remained in hospital for three weeks, partly on account of an ulcer of the leg, partly to keep him under observation. The urine, which on his admission was strongly albuminous and of rather low specific gravity, improved in quality, so that the albumen had disappeared before he was dismissed. The patient was seen a year afterwards in fair health, with no return of his convulsions. It was looked upon as a case of the granular contracted kidney. This case had produced a strong impression on his mind in favour of venesection in cases of great congestion; but at the same time it was very unsafe to argue from single cases however striking. Dr Sanders had not again had occasion to resort to the same treatment. Cases of uræmic convulsions, even when apparently very severe, may generally be treated successfully by purgatives and diaphoretics. Diuretics, cupping, or poultices over the kidney, should also be had recourse to, but their action cannot be expected to be sufficiently rapid for immediate effects—the kidneys being obstructed, it is like knocking at a closed door. Purgatives will act more rapidly. Dr S. thought the great advantage of venesection was the promptness of its action; in this respect excelling other remedies. But he had found the remedies above mentioned without bloodletting quite sufficient; the only cases where he thought bloodletting should be tried as a last resort, were those in which intense venous congestion supervened during the attack, and formed in itself a source of danger.

Dr G. Balfour thought that the anæmic condition of the brain usually found in dissections of Bright's disease proving fatal by head symptoms, furnished a strong, if not a conclusive, argument against the propriety of bloodletting in uræmic convulsions. Dr Bennett's first case, where the patient was apparently rescued by the venesection, was merely an instance of the *post*, not the *propter hoc*. Such instances of extraordinary and sudden amelioration occurred under all kinds of practice, or even under no practice at all. Of this, several remarkable instances had occurred to himself. For his own part, he adopted the treatment by chloroform during the attacks as the most suitable and successful; and he had also used vinegar with the view of neutralizing the carbonate of ammonia, to which Frerichs ascribed the convulsions and other phenomena of uræmia.

Dr Bennett was glad that the subject had excited so much discussion. He thought that the beneficial effects ascribed by former writers and teachers (*e.g.*, his former clinical professor, Dr Graham) to venesection in inflammatory dropsy, was not due to any good done to the inflammation of kidney itself, but very probably to the influence exerted on the uræmia. On this view, he thought bloodletting might be useful in acute cases. With regard to the first case he had related, although Dr G. Balfour considered it merely a coincidence, he (Dr Bennett) was as convinced as he could be of any fact in therapeutics, that the man's life was saved by the venesection. Dr Bennett had considered the changes in the epithelium as described by Dr Stewart to indicate renal disease; and this had suggested uræmia as an explanation of the fatal result in the case of pneumonia, as it was the first time he had met with a fatal case of pneumonia when free from complication. It was to be regretted that the blood had not been analyzed for urea.

PROCEEDINGS OF THE EDINBURGH OBSTETRICAL SOCIETY.

SESSION XXVII.—MEETING X.

8*th April* 1868.—Dr Burn, *President*, in the Chair.

I. *Dr Keiller* read a paper on Cæsarean section, and related the particulars of various cases in which the operation had been performed. He likewise exhibited a series of preparations illustrative of the subject, and intimated that on some future occasion he intended to give to the Society a fuller and more complete account of the cases of Cæsarean section which had come under his observation. The President, in the name of the Society, thanked Dr Keiller for his interesting remarks.

II. *Dr Ritchie* showed a curious worm-like object which had been passed from the bowels of a little girl. It was evidently of a tubular structure, but it was difficult to make out its real nature.

SESSION XXVII.—MEETING XI.

22*d April* 1868.—Dr Burn, *President*, in the Chair.

I. *Dr Messer*, Penicuik, read a paper relating to a

CASE OF CRANIOTOMY.

Dr Andrew Inglis remarked, that in head presentations the cephalotribe often slipped, whereas when the face presented, or in cases in which the body was first delivered, the instrument took a better hold, and might be used with great force. He said the first attempt to use Sir James Simpson's cephalotribe was made by Dr Weir and himself, and after a great deal of trouble they were obliged to use the blunt hook. The instrument, however, at that time had not been entirely completed. Dr Inglis was of opinion that the instrument constructed by Dr Charles was much better adapted for taking a firm hold.

Dr Cairns stated that he had just come from a case which had been going on for about three days, but had terminated successfully without instrumental interference. He had exercised great patience, humoured the patient, and changed her position frequently, and in this way delivery was accomplished without instruments. He was of opinion that in many similar cases there was no necessity for having recourse to instrumental interference, and he had only once in his practice ever required to use even the forceps. He never could forget the lesson taught by the late Professor Goodsir, about the advantage of position in labour; by putting the woman on her knees the symphysis was allowed to separate.

Dr Burn corroborated Dr Inglis' remarks as to the tendency of the cephalotribe to slip. He thought that all pains which did not affect the os should be regarded as spurious, and he did not think that, in a case like that which Dr Cairns had related, any one would have attempted to interfere with instruments. He had, however, seen some distressing results from delay, and remembered one case to which he had been called, along with the late Dr Young, in which a midwife had been in attendance for three days. The parts were then sloughing, and although delivered, the woman died next day.

Dr Pattison stated that he had once waited 108 hours on a case, and he agreed with Dr Cairns, that unless there were any alarming symptoms, it was better not to interfere.

II. CASE OF MALIGNANT TUMOUR OF THE OMENTUM, WITH SPECIMEN.

BY DR JAMES YOUNG.

I must offer an apology for bringing this case under the notice of this Society, inasmuch as the subject is not allied to obstetrics; but I hope the interesting nature of the case, combined with its rarity, will be a sufficient excuse. The specimen before you is the half of a tumour which I removed from a patient lately, after death, and the following are the particulars:—P. M., aged 65, began to complain of shivering, accompanied with a feeling of depression and sickness, in October 1867. I saw him on the 8th of that month, and prescribed for him some mild alterative aperient. He was shortly thereafter able to attend to his business, but not so well as to leave the house every day. At the end of three weeks, or the beginning of November 1867, he called my attention to a small tumour which he felt in the right side. This small walnut-like tumour was easily felt, floating midway between the umbilical and right lumbar regions, but was in no way connected with the liver or stomach. He felt no pain, but suffered much from vomiting. Food was no sooner swallowed than it was rejected, sometimes unchanged, and sometimes it was mixed with bilious matter. All the usual medicines were tried, such as cerium, ice, carbolic acid, and externally iodine and blisters. During November there were intervals of improvement, but vomiting again returned in the beginning of December, at which time I discovered that the tumour was twice as large as it was in November; his strength also was yielding slowly. He was confined to bed nearly all December, and only able to take a little nourishment; at the close of the year he was out once or twice. Dr Begbie was asked to see him, and on examination he feared that the tumour might become malignant, and recommended bromide of potassium. Dr Thomas Keith saw the patient in January 1868, and he thought the tumour (then larger than an egg) was likely to become malignant. The patient now began to complain of pain on pressure, and the question naturally arose, "could the tumour be removed?" After a consultation it was agreed not to touch it, as the transverse colon seemed to run over the tumour. The patient was soothed as much as possible, but on February 1, I regret to say, he was seized with singultus, and which gradually increased in frequency and severity all February and March. On the 1st of the latter month, the tumour had increased to the size of a fœtal head, and the hiccough had become long and loud, and most painfully distressing. All the usual remedies were tried, but in no way tended to remove the convulsion of the diaphragm, and (almost more remarkable still) when the hiccough came on, the vomiting abated, and he never vomited again during his lifetime. The bowels became confined, urine scanty, considerable pain was felt, and incessant hiccough prevailed. The tumour increased in size rapidly, the strength gradually gave way, and he died from exhaustion on the 7th of April. Autopsy 24 hours after death. I need not touch on the condition of the several parts of the body seriatim. Suffice it to say, that there was no disease found in any organ of the chest or abdomen. When the abdominal cavity was opened, the tumour was found lying between the umbilical and right lumbar regions, with the colon underneath it. The tumour was semi-solid, somewhat cerebriform, and weighed between 4 and 5 lbs. Dr Grainger Stewart, after examination of the tumour, says, "I examined the portion of tumour which you sent me, and found it distinctly cancerous. The most peculiar feature of it was, that in certain patches which appeared yellow and opaque to the naked eye, the cancer elements had undergone an extreme fatty degeneration."

Dr Cairns remarked that pressure on the phrenic nerves often allays hiccup.

Dr Burn said he had some time ago seen a case which was in some respects allied to Dr Young's. The patient had a tumour in the abdomen, thought to be malignant, but unfortunately there was no sectio allowed.

Dr Dickson had a case of a woman aged 40 under his care, who had a tumour of the omentum which lasted for two years. In his case there were hiccup and vomiting.

Public business having been concluded, *Dr Pattison* mentioned that he had lately been sent for to see a case of hæmorrhage from the mouth. On looking into the cavity he discovered two small wounds like leech-bites on the palate, from which the blood was issuing. The nitrate of silver and the perchloride of iron were tried, but the bleeding was only arrested after the administration of sulphuric acid.

Dr Ritchie had sometimes seen severe hæmorrhage in cases of psoriasis, in which the lining membrane of the nose had become affected.

Dr Cairns wished to ask the opinion of the Society as to the propriety of administering arsenic to pregnant females. He had had a case in which, while under its use, abortion came on at the third month.

Dr Burn said that he had used arsenic in such cases with perfect safety.

Dr Inglis thought that the association of psoriasis with abortion pointed to syphilis as the cause.

Dr Sidey remarked that he had two patients in whom psoriasis always made its appearance during pregnancy. He thought the abortion had been accidental in Dr Cairns' case.

Part Fourth.

PERISCOPE.

PATHOLOGY AND MEDICINE.

ON THE PIGMENTS OF THE RESPIRATORY ORGANS. BY DR KUAUFF OF HEIDELBERG.

The author gives the results of experiments, undertaken to determine the nature of lung pigments, whether animal colouring matters or inspired carbon. He seeks to explain the natural pigmentation which occur in the lungs as life advances, and the different morbid pigmentations. He remarks that we must assume that daily a quantity of finely-powdered carbon is deposited in the lungs of everyone, and asks what is its ultimate fate; seeing that as an insoluble body it can scarcely be supposed simply to disappear.

He experimented on dogs, shutting them in a chest into which he introduced great quantities of smoke, confining them for periods varying from one day to three months, feeding them carefully, and in the end killing them, and then examining the lungs. In animals confined for one day he found the bronchi and lungs coated with a deposit composed of carbon mingled with mucus, and here and there, especially in the finer bronchi, mucus cells containing black pigment were seen. He describes at length certain anatomical features of the mucous membrane, which show that the ciliated cells, and the granule containing cells, as well as certain intercellular spaces, represent different stages of a typical metamorphosis of the ciliated epithelium.

In animals which had been kept a longer time in the smoke chest, carbon existed in the deeper parts of the respiratory organs, earliest and most marked in the bronchial glands. On section of such lungs, he found the cells of the alveoli partly free from, partly charged with carbon. In the lumen he found free carbon, carbon was present also in the parenchyma. On the surface of the lung black lines were seen, constituting injections of the lymphatics. These observations are explained, if we assume that from the surface of the alveoli the carbon passes into the parenchyma, thence into the lymphatic vessels and glands. He thus conceives that the ordinary pigmentation results from inhalation. *That all so-called lung pigment, which cannot be distinguished from carbon, is carbon inhaled, not formed in the body.*

He then refers to the state of the bronchial glands, and of the pleura, and describes as occurring in the latter certain small bodies in which pigment is copiously deposited, and which are composed of groups of small vessels not unlike the Malpighian bodies of the kidney surrounded by lymphatic structures. Dr Kuauff then considers the pathological pigments under three heads:—

1*st*, The true melanoses.
2*d*, The brown induration.
3*d*, The black induration.

True melanoses are here as elsewhere peculiar in their origin, being derived from the blood. It is like the pigment of the choroid membrane of the eye, has the same chemical character of great but not complete resistance to the strongest re-agents, particularly mineral acids. It is brown rather than black, and is certainly to be referred to the true animal pigmentations.

2*d*, The brown induration is always a result of transformation of blood, it progresses slowly, and transition forms are found.

3*d*, The black induration has no connexion with true melanosis, and is markedly separated from the brown induration. There are no transition forms; there is in truth an increase of the normal lung pigment, associated with a chronic inflammatory process: Kuauff conceives of it as *chronic pneumonia, with unusually copious carbon deposition.* The development of black induration presupposes two things, the inhalation of a chronic irritant, and of a considerable quantity of finely divided carbon, which is not, as in healthy lungs, mostly coughed up, but is retained. The constant irritation results from almost exclusively fine chisel-particles. Stone hewers and carvers suffer thus:—The dust is often so fine that they lay aside their watches else they would be stopped by the dust in a few days. Masons are somewhat better situated, using much water, and not making such fine work. Millers suffer like hewers, and in them black induration is common. Metal-grinders suffer similarly. Coal-miners also suffer. Before the coal is reached other rocks must be penetrated; those who work at the stone wall suffer most. Fragments of coal may sometimes be inhaled, but never in such quantity as the carbon, which is very light, and also irritating. All these workers breathe at the same time much lamp-smoke, ventilation being imperfect and the space small.

Kuauff conceives that, while healthy people cough up carbon, those who have had frequent catarrhs have the ciliated epithelium destroyed, the carbon is retained, and more is absorbed. If ulceration occurs, lymphatic vessels are opened, and foreign bodies may the more easily pass in. He conceives that in such cases pigmentation may result from altered blood, but that for the greater part it is due to inhalation.

ON PUS FORMATION IN THE GUM. BY PROFESSOR ARTHUR BŒTTCHER OF DORPAT.

He found cells of various kinds—1st, Ordinary pus corpuscles; 2d, Larger cells, whose quantity was to the pus as white corpuscles of the blood are to the red. Of these there were two classes—(*a*) Cells without distinct nuclei: among these some were large mother cells full of pus corpuscles, with numerous oil globules; others were smaller, and contained less formed elements, rounded balls; others with numerous small glancing masses; and yet others about twice the size of pus corpuscles. (*b*) Cells with distinct nuclei, the nucleus often of considerable size, and quite transparent. Of these also the author found several varieties, viz:—Some besides the nuclei only enclosed a number of shining granules; others which contained in addition granular matter; and yet others with peculiar contents. What is the relationship between these different forms? Certainly the large mother cells present the most advanced, but how does it begin — from division of nucleus, or from free endogenous, or apart from the nucleus? The observations appeared to support both. On the whole, Bœttcher concludes that a division of nuclei occurs, and may advance to a breaking of it up into small fragments. Round such fragments protoplasm consolidates, the daughter cells are set free by breaking up of the mother cells, of which portions may here and there be seen. —*Virchow's Archiv*, August.

ON SUPPURATION OF THE CORNEA. BY F. A. HOFFMANN.

The author concludes from his observations that pus cells are directly formed from the protoplasm of the stellate cells of the cornea. He also, however, confirms Cohnheim's statement that some of the cells are not locally formed, but derived from the bloodvessels.—*Virchow's Archiv*, February.

ON A PECULIAR DEGENERATION OF VESSELS OF THE BRAIN. BY DR RUDOLF ARNDT, PHYSICIAN TO THE LUNATIC ASYLUM AT HALLE.

The condition was found in a woman who died of general paralysis. The consistence of the brain was tough, and presented various morbid conditions; but the special point of interest was that in the right hemisphere, in a gyrus of the third and one of the first frontal convolution; at the point where the gray substance passes into the white, there were deposited gray gelatinous masses, easily removed from their surroundings, firm and granular to the touch, and not readily compressed by the finger. In a man who died from general paralysis, the same condition was found. Subjected to a careful microscopic examination, the vessels seemed surrounded by dense tough gelatinous material, which was, however, soluble in warm water. When hardened in chromic acid, it was found that the masses were connected with thickening of the walls of the vessels, the vessels remaining open throughout, and groups of capillary and other vessels were in many parts agglomerated into single masses by the colloid substance.

The colloid substance attracted indigo and carmine; also iodine in a very marked manner. Anilin, on the other hand, coloured the affected parts less than the healthy vessels.

When hardened and examined under the microscope, these appeared capable of being broken up into layers; in these layers solitary shining nuclei, with bright nucleoli, were seen. In the spaces between the layers, the nuclei were more numerous, and mingled with glancing granules. Under the action of alkalies and acids, particularly acetic acid, the nuclei became more distinct and granular; they also appeared more numerous.

The lancet-shaped plates or masses, the author regards as cells which have been distended by the development of a gelatinous, so-called colloid matter, and that the intervals represent the intercellular spaces in which development of nuclei is going on.

The apparently natural vessels are rich in nuclei, the vessels being in some parts completely covered with them. In some parts it appears that these newly-formed cells undergo further changes; the cells enlarge even to a colossal size; the outer ones break up and give the surface a rough appearance; the nuclei seemed to the author to be the same in the spaces and in the enlarged cells.—*Virchow's Archiv*, January.

NOTICE OF THE DISCOVERY OF VEGETABLE ORGANISM IN THE LYMPH OF SHEEP-POX. BY PROFESSOR DR HALLIER AND DOCENT A. ZUERN OF JENA.

THESE authors find in the lymph of sheep-pox very fine threads and small bodies, the latter moving actively. Similar bodies were found in the vaccine lymph. Examination of sheep-pox in process of development showed the whole tissue full of small nucleated cells. The different elements of the epidermis showed these nuclei in great quantity, and very fine threads. They propose to make experiments by cultivation.—*Virchow's Archiv*, November.

NOTICE OF VEGETABLE ORGANISMS IN THE LYMPH OF SMALLPOX.
BY ERNST HALLIER.

IN lymph obtained from a case of smallpox, the author found numerous micrococcus cells far more abundant than he had found them in vaccine lymph or in sheep pocks. Magnified 800 times, they appeared distinct rounded bodies moving about, and here and there attached in large numbers to lymph corpuscles. He also observed many fine threads. He states also that his cultivation experiments, in regard to the lymph from ovine and vaccine pock, have always produced the same characteristic organisms.—*Virchow's Archiv*, February.

ON THE MICROSCOPIC ELEMENTS OF VACCINE LYMPH. BY DR F. KEBER, OF DANTZIC.

IN vaccine, as well as in variolus lymph, the author has recognised peculiar organic bodies, and a very active series of changes. He has found such bodies in vaccine lymph, in the lymph of smallpox, and in that of varicella. He has not been able positively to satisfy himself of their occurrence in the blood of the affected, or in the air which surrounds them. The character of the bodies is as follows:—There are granular cells 1-150th to 1-300th of a line in diameter, innumerable free nuclei from 1-800th to 1-3000th, and numerous minute molecules. The cells cannot be mistaken for pus corpuscles; they have a very fine membrane which swells with water, and present a characteristic appearance quite distinct from mucus or pus corpuscles.—*Virchow's Archiv*, February.

HUPPERT ON TESTING FOR BILE PIGMENT IN URINE.

The test for bile colouring matters, according to the author, is often doubtful, even in cases where you have reason to believe that a large quantity of pigment is present. This depends on various circumstances, the most important of which is, that the icteric urine does not always contain the yellow pigment which, on the action of the acid, gives the change of colour (bilirubin), but only becomes green (biliprasin).

If, again, you act on urine free from bile pigments with nitric acid, you have formed, especially in the pale urine of anæmia and chlorisis [in consequence of the decomposition of Indican?], at the borders of both fluids, violet red and yellow zones, which are not to be distinguished by the eye from those formed in urine containing bile pigments. Dr H. advises, therefore, the following method of procedure for detecting quickly and surely even a trace of bile pigment:—He makes the urine alkaline by the addition of milk of lime, collects the generally very yellow precipitate immediately on a filter, and heats a portion of this in a test tube, with concentrated sulphuric acid, carefully and gently, so that the colouring matter is not destroyed. If bile pigment be present to a moderate amount, the fluid soon assumes a dirty green colour, and a dark green froth collects upon it. If only a little colouring matter be present, add some alcohol, and carefully heat for a short time, when you obtain a fine dark green coloration. It is besides advisable not to shake the fluid on adding the alcohol. If you have heated it too short a time, the alcohol is either very slightly coloured, or not at all; in this case you only need allow the fluid to stand for several hours a day, in which time the solution becomes green.—*Prag. Vierteljhrschrift.*

ABEILLE ON THE INHALATION OF CINNABAR BY MEANS OF STEAM IN CROUP.

The cases reported are of the disease in children, in whom tracheotomy was declared necessary as the "dernier ressort," everything else having been tried in vain, but where, notwithstanding the desperate state of the patients, the parents would not allow of the operation being performed. The treatment consisted in the administration of an emetic of ipecacuanha, so that vomiting occurred thrice daily, and at the same time the inhalation of cinnabar spray (steam), which was managed by boiling in a wide vessel mallow and lime-tree flowers, and on each occasion when the temperature of 212° F. was reached, two grains of cinnabar were added. The child got along with this some simple drink, and wine and nourishment when they could be borne. Under this treatment, croup membranes were ejected. The cinnabar spray was employed as long as any traces of croup appeared.—*Prag. Vierteljhrschrift.*

EULENBURG ON HERPES ZOSTER.

Herpes zoster has, according to Dr Eulenburg, always a peripheric origin, and consists of a characteristic change in the skin, with an accompanying affection of the vaso-motor system of nerves, and not, as Von Bärenspung insists, in a primary affection of the spinal ganglia. In no case is it the rule that the zoster follows the whole course of a certain spinal nerve; frequently, only a simple twig of a nerve, and certain branches of the plexus, are affected, which sometimes is observed in the neighbourhood of individual branches of a plexus (for instance, the brachial), with interruption of motor power in the corresponding nervous branches especially complicated with paralysis of individual muscles (Broadbent). Dr E. made observations in the case of a healthy shoemaker, æt. 41, as follows:—The patient had suffered for two years from severe pain, to which was added the sensation of deadness, and a cramp-like drawing up of the fingers, all of which followed a chill. The pain, almost always constant, and only seldom aggravated, followed nearly exactly the course of the ulnar nerve. Pressure over the course of this nerve occasioned pain. With the neuralgia was united a diminution of sensation, so that over the skin covering the course of the ulnar nerve, deep pricks with a pin were hardly felt. The sudden accession of the contraction of the fingers pointed distinctly to extension to the motor portion of the ulnar nerve. Nine days after the first examination, the patient appeared with a zoster, which had existed for three or four days, and which corresponded exactly to a cutaneous branch of the ulnar. The skin in the interstices between the groups of herpes, as also between the individual vessels and scabs, exhibited to a high degree loss of sensation—a symptom in herpes not rare, but frequently not noticed. All the former-mentioned disturbances of motor and sensory power remained. A constant current (from 12 to 20 of Siemenssen's elements) was employed daily for three or four minutes from the trunk of the ulnar nerve to the plexus.

After ten sittings, the pain and cramp were completely, and the anæsthesia, to a great degree, removed. In the meanwhile, the exanthem had also dried up. After a few more but irregular sittings, sensibility and mobility were perfectly restored, and the patient returned to work. The order of the symptoms were as follows:—Neuralgia, then anæsthesia and cramp, in those parts to which the ulnar nerve is distributed, diminution of sensibility, and, lastly, herpes zoster, with distinct localization to the course, and distribution of a cutaneous branch of the ulnar.

Primarily, here, we had the affection of the ulnar apparently rheumatic or perineuritic; from this arose diminution in the conducting power of the scissory and motor portions, and loss of function of the vaso-motor and sensory fibres which supply the skin, and hence the zoster.—*Prag. Vierteljhrschrift.*

CASE OF COMPLETE TRANSPOSITION OF THE THORACIC AND ABDOMINAL VISCERA. BY N. HICHMAN, M.D.

In the anatomical rooms of the University of Pennsylvania, a subject was dissected, in which the following curious arrangement of the internal organs was discovered:—

In the thorax, the lungs were reversed, the left and larger consisting of three lobes, while the right had only two. The heart extended obliquely from left to right, the apex pointing to the intercostal space between the fifth and sixth ribs of the *right side.* The right side was also thicker in its walls; and while the ventricle gave off the aorta, the auricle received the pulmonary veins. On the left side, the auricle received the venæ cavæ, and the ventricle gave off the pulmonary artery. The aorta arches first to the left, and then to the right, and descends on the right side of the vertebræ to its bifurcation at the top of the fourth lumbar vertebræ. The first branch given off from the arch is the innominate, which subsequently divides into the left common carotid and subclavian arteries. The next is the right common carotid and subclavian, which it will be seen is the reverse of the usual distribution. The cœliac axis is absent. The ascending cava is on the left of the aorta.

In the abdomen, the viscera are also reversed. The liver is situated in the left hypochondriac region, the larger lobe being under the ribs, while the smaller extends into the epigastrium. The spleen is situated deeply in the right hypochondriac region. The stomach has the cardiac end turned towards the right, and the pyloric extremity to the left. The large intestine is also reversed in its relations, and terminates in the rectum at the *right* sacro-iliac symphysis. The pancreas extends from the spleen in the *right* hypochondrium to the duodenum, beginning in the *left.* The pneumogastric nerve on the right side descends in front of the arch of the aorta into the posterior mediastinum in front of the œsophagus and stomach, thus taking the course usually followed by the left. The left nerve passes posterior to the œsophagus and stomach.—*American Journal of Medical Sciences,* January 1868.

EXPERIMENTS WITH LIGATURES.

Dr B. Howard, at the end of last year, exhibited to the Pathological Society of New York three specimens of ligature of the carotid arteries in sheep, by different ligatures, and in different manners. In the first experiment, he applied a silver wire ligature to the common carotid of a very strong sheep, and fastened it by twisting it very tightly. Fifty-six days afterwards, he cut down through the cicatrix, and found that the collateral circulation which was established was very complete, and that the capillary circulation was not great; so that there was little hæmorrhage. On coming down to the point of ligation, he was astonished to find what at first appeared to be an aneurism, but which turned out to be an abscess full of inspissated pus. It occupied a space of about four lines on either side of the ligature, beyond which points the plugs were intact for about seven lines. In the centre of this abscess lay the ligature with the middle and lining coats of the artery completely sloughed away and detached.

In the second experiment, he used a leaden ligature, and had it rather large in order to avoid its cutting through; he tied it tightly. Twenty-six days after, he cut down through the cicatrix, and instantly a pellet of pus appeared in the incision, as if propelled from behind. He made another incision, when the ligature came away. As he proceeded towards the original site of the ligation, the hæmorrhage was exceedingly great. He ligated above and below, and removed the abscess. There had been a good deal of inflammation around the artery, and there was considerable fibrinous material effused in the neighbourhood. The artery itself was well plugged on its cardiac and distal aspect, and was perfectly occluded. Between the proximal end of the distal plug and the distal end of the cardiac plug was apparently an abscess containing about a drachm and a half of sero-purulent fluid; otherwise, the clot was perfect. The points of interest in this specimen are the great amount of irritation and inflammatory action that occurred about the metallic ligature. In the third experiment, he again used a silver wire ligature, but tied it incompletely after the manner of the silk ligature, and was careful to tie it loosely—tight enough to diminish the canal of the artery, but avoiding a complete closure. On cutting down afterwards upon the cicatrix, he could scarcely discover where the ligature had been applied, so small was the amount of irritation it had produced. The plug in this case was more perfect than in either of the other cases. There was no abscess, the fibrinous deposit being just sufficient to cover the ligature. The result, so far as occlusion, extent, and firmness of the plug are concerned, is very much better than in either of the other cases. From these experiments, so far as they go, he concludes, 1st, That the metallic ligature does not always reside quietly where it is placed. 2d, It is not always necessary for complete occlusion of the artery to tie the ligature so tightly as to divide the middle and internal coats. 3d, The last experiment is very suggestive; that a silver wire ligature applied so loosely as to produce no lesion of the artery, nor even to completely arrest the circulation, may suffice for the cure of aneurism, when an ordinary ligature would be rendered inexpedient by the condition of the arterial coats.—*Medical Record.*

SURGERY.

CASE OF POLYPUS OF THE BLADDER IN A FEMALE CHILD OF TWENTY-TWO MONTHS OLD—OPERATION—DEATH ON THE EIGHTH DAY.

The following is a brief abstract of a case of great rarity which lately occurred in the practice of M. Guersant:—

The child was under the care of Dr Penochaud, of Boulogne. It seemed otherwise in good health, suffering only from difficult urination with prolonged efforts. These symptoms were aggravated by an attack of diarrhœa, which first induced the mother to seek medical aid. On being sounded, a fleshy mass was found in the bladder, and a small tumour like a nut was seen to project into the urethra during the child's efforts to make water. The diagnosis of fleshy tumour of the bladder was made, and the child was sent to Paris to be under M. Guersant. The fleshy tumour at the urethra, the straining, and the absence of calculus, were then made clearly out; soothing external applications were recommended, and M. Demarquay was called into consultation. During four or five days, all the symptoms were aggravated. The child became feverish, the efforts at expulsion became more vigorous and frequent, so that the child's mother compared them to the pangs of labour, repeated every five minutes. The projection of the fleshy mass increased, and its surface appeared as if about to slough, being evidently strangulated. The vulva became inflamed; the urine, when drawn off, was muddy and fetid. It was obvious that something must be done to save the life and mitigate the sufferings of the poor little child; yet it was obvious that any interference would be dangerous.

The child was anæsthetized with great ease by a mixture of equal parts of chloroform and ether, and was placed on a table in lithotomy position. The tumour being seized and drawn forward by M. Demarquay, M. Guersant passed a metallic thread as far as possible round the neck of it, after gaining access by dividing the urethra by the scissors. The thread was gradually and slowly tightened by a *serre-nœud*, but broke before the tumour was separated, so the operation had to be finished by the ecraseur. The tumour was very vascular, and under the microscope was found to be composed of connective tissue, with numerous cells and nuclei. There was little bleeding at the time.

Next day the urine was bloody; there was no more efforts at expulsion, as it came away easily, but gave pain in its passage. The child gradually lost strength; and though the bladder symptoms improved, the soft parts became inflamed; there was sloughing. It had diarrhœa, refused its food, and died exhausted on the eighth day. No post-mortem examination was obtained.—*Gazette des Hopitaux*, No. 23, 1868.

ON A LESION OF THE SUBACROMIAL SEROUS BURSA, THE RESULT OF CONTUSION.

M. JARJAVAY has lately written a memoir, and delivered various clinical observations, on cases of this nature, and wishes to prove that those cases which are sometimes called luxations of the long tendon of the biceps, are really due to inflammatory affections of the bursa below the acromion.

M. Jarjavay holds that it is impossible that a tendon passing over the extremity of a long bone can be dislocated without the occurrence either of fracture or dislocation of the joint, and this after the following process of reasoning:—The prominent symptoms which are supposed to denote displacement of the long head of the biceps are, a sharp pain in the prominence of the shoulder during a severe twist of the arm; a sensation of displacement in the deeper parts, so as to make the patient think the limb has been dislocated; a greater or less interference with the movements of the arm on the scapula, even in some cases to complete immobility; a stiffness of the biceps along with flexion of the elbow, and a sensation of something being reduced after passive movement of rotation of the head of the humerus. These symptoms M. Jarjavay regards as insufficient to prove the lesion. He finds that the precise seat of the pain is nearly half an inch outside of the position of the long head of the biceps, and that the noise heard, and the sensation of sudden displacement communicated to the hand, are phenomena due to something quite different from displacement of a tendon. A very well-marked case came under notice, which, both from its causation and symptoms, seemed a typical example of displacement of the tendon. By a careful examination, the following, among other points, were made out:—1. That the precise seat of pain, and of jerk or movement, was towards the outside of the shoulder, close to the beak of the acromion. 2. That the movement of rotation given to the head of the humerus, when the arm was brought up into a horizontal position, produced pain, noise, and jerk; while if an assistant, with both his thumbs, pressed the head of the bone downwards from the acromion, no noise or jerk were obtained even during rotation. Under bandaging and pressure, this case rapidly got well.

In all the cases recorded by M. Jarjavay, the injury had been caused by a sudden twist of the arm inwards and backwards. Now, to have a luxation of the long head of the biceps, or the tuberosity of the humerus, it is necessary not only that the groove in which it lies covered by ligament be opened, but also that the insertions of capsule of the joint, and the supra-spinatus muscle, be destroyed. An injury capable of producing such displacements would rather cause a rupture of the tendon of the biceps, or a dislocation of the shoulder, or both. Carrying our attention, on the other hand, to the serous bursa, which lies below the acromio-coracoid arch, we see that the movements, induced by a twist, press severely on its wall, especially if the humerus had a direction either forwards or backwards; especially will it be noticed if a dissection be made by throwing the deltoid upwards from its insertion, that in abduction the head of the humerus, and specially its greater tuberosity, play on this bursa. If the walls of the bursa are healthy, this play takes place smoothly and noiselessly; if, on the other hand, they are thickened, or the cavity of the bursa traversed by fibrous ends, a jerk takes place which gives rise to a sort of snap.

M. Jarjavay has frequently met with this condition of the bursa; the walls are dry and hard, and the normal cellular threads which cross its cavity, and which should be thin and smooth, are changed into fibrous cords. This is not uncommon in workmen who use their arms in hard labour, and in them it is easy, by abducting the arm, to produce the symptoms referred to.

From these facts and considerations, M. Jarjavay has arrived at the following conclusions:—1. That simple luxation of the long tendon of the biceps does not exist, or at least has not been demonstrated. 2. That the lesion which has been mistaken for this luxation, is really seated in the subacromial serous bursa. 3. That this lesion consists in an inflammatory swelling, occasioned by a contusion or tear of this serous bursa, or secondary to the inflammation, in the hypertrophy with induration of its walls, and a fibrous transformation of the normal cellular cords which exist in it. 4. That the following symptoms denote the case:—Sensation of displacement at the moment of the accident; swelling of the prominence of the shoulder; pain, limiting the movements of the arm, especially abduction; permanent flexion of the forearm, and, as a consequence, stiffness of the biceps, and fatigue at the bend of the arm. Increase of pain and production of a peculiar bruit when the tuberosity is made to glide under the acromion; disappearance of pain and restoration of motion under rest and soothing applications.—*Gaz. des Hopitaux*, No. 25, 1868.

Part Fifth.

MEDICAL NEWS.

BRITISH MEDICAL ASSOCIATION.

REPORT OF MEETING OF COMMITTEE OF COUNCIL, HELD AT BIRMINGHAM, 23D APRIL 1868.

Arrangements for the Annual Meeting at Oxford, 1868.

THE following resolutions were passed at the last meeting:—

1. That the following gentlemen be appointed Officers of Sections:—MEDICINE.—*President*, Sir W. Jenner, Bart., M.D., F.R.S., London. *Secretaries*, E. L. Fox, M.D.Oxon., Clifton, Bristol; William Roberts, M.D., Manchester. PHYSIOLOGY.—*President*, Professor Rolleston, M.D., F.R.S., Oxford. *Secretaries*, W. L. Church, M.D.Oxon., London; Professor Beale, M.B., F.R.S., London. SURGERY.—*President*, James Paget, Esq., F.R.S., London. *Secretaries*, T. P. Teale, M.A., M.B.Oxon., Leeds; W. Stokes, jun., M.D., Dublin. MIDWIFERY.—*President*, Sir C. Locock, Bart., M.D., F.R.S., London. *Secretaries*, J. G. Wilson, M.D., Glasgow; J. G. Swayne, M.D., Clifton, Bristol. STATE MEDICINE.—*President*, J. Simon, Esq., F.R.S., London. *Secretaries*, J. E. Morgan, M.D.Oxon., Manchester; T. J. Dyke, Esq., Merthyr Tydvil.

2. That a Sub-committee be appointed to consider and to report as to the proper subdivision and organization of the various Sections, and the best mode of appointing their officers; and that the Sub-committee be requested to observe and report upon the working of the different sectional meetings at Oxford.

The Sub-committee to consist of the following gentlemen, with power to add to their numbers:—The President; the President-elect; the President of the Council; the Treasurer; the General Secretary; the Officers of Sections; Mr Cordy Burrows; Dr Paget; Dr Edward Waters; and Dr Wilkinson.

3. That the report now read from the Committee for considering the representation of the profession in the General Council of Medical Education be adopted; and that the members of the Sub-committee, with the President of the Council, be appointed a deputation to wait upon the Medical Council.

REPORT OF THE SUB-COMMITTEE APPOINTED TO CONSIDER THE REPRESENTATION OF THE PROFESSION IN THE GENERAL MEDICAL COUNCIL.

THE Sub-committee, in accordance with their original recommendation, supported by the almost unanimous vote of the Association, agreed to at the anniversary held in Dublin in 1867, are of opinion that the profession will not be adequately represented in the General Medical Council, as at present constituted, by less than eight representatives, to be elected by the registered members of the profession resident in the United Kingdom of Great Britain and Ireland, with the exception of those on active service in the army and navy.

The Sub-committee are of opinion that the addition of eight members to the General Medical Council, as representatives of the profession generally, far from embarrassing the proceedings, or militating against the influence of the Council, will, on the contrary, have the opposite effect.

The Sub-committee entertain no doubt that the profession, on which the payment of the representatives of the various medical and surgical corporations, and of the nominees of the Government at present rests, will willingly pay the additional cost of their own representatives. At the same time, the Committee think the propriety of paying their own representatives by the various universities, colleges, and corporations, is an object for the consideration of the Council.

The Sub-committee are of opinion that, if the Council, in consequence of the addition of the members to be elected by the profession, should deem it advisable to recommend a diminution of the number of the representatives of the corporations, a corresponding reduction may then be made in the representatives of the profession. EDWARD WATERS, *Chairman.*

DEATH OF LORD BROUGHAM.

LORD BROUGHAM, Chancellor of the University of Edinburgh, died at Cannes, on 7th May, aged 89.

REBUILDING OF THE MEDICAL HOSPITAL OF THE EDINBURGH ROYAL INFIRMARY.

THE appeal for subscriptions for this great and truly charitable object has been responded to with unprecedented liberality. The sums already subscribed amount to upwards of £51,000. The total sum required is £100,000.

GOODSIR FELLOWSHIP—SUBSCRIPTION LIST *continued.*

James Robertson, Esq., Welbeck Street, London,	£1	1	0
Dr Ralph Richardson, Exmouth,	1	1	0
Dr Mahony, County Hospital, Huntingdon,	0	10	6
Dr John Miller, Edinburgh,	1	1	0
Dr Dick, Beechworth, Australia,	2	0	0
Dr Paterson, Adelaide Asylum, Australia,	5	0	0
Professor Cleland, Galway,	2	2	0
James Moore, M.D., M.R.I.A., Belfast,	2	2	0
Henry Murney, M.D., J.P., do.,	1	1	0

FEES OF MEDICAL WITNESSES.

To the Editor of the Edinburgh Medical Journal.

Cupar-Fife, May 1868.

Sir,—We are desirous, through the medium of your Journal, to lay before the profession a statement of certain facts which we conceive to be of the highest importance to the medical world.

On Monday the 8th April 1867, we attended the Circuit Court of Justiciary at Perth, in obedience to a citation as professional witnesses for the Crown in the case of John Kinnear, charged with the crime of murder. As the Court met at 10 A.M. (the first time it was ever known to open on a Monday at Perth at this hour), we left by the last train the previous day, in order that no accident should prevent our due attendance (and also to see the Advocate-Depute in relation to some circumstances not sufficiently brought out at the precognition), and consequently charged for two days' fees.

We were tendered, not by the Sheriff or Sheriff-Clerk, but by one of his officers, the allowance for one day and railway fares.

Believing we had a just cause, and that justice would be done on application to the Lord Advocate, one of us addressed his Lordship, narrating the circumstances, and requesting payment for two days; but in a letter dated 24th May 1867, we were informed that the matter to which "you refer is one which does not fall within my province to interfere, but is one which falls under the department of Exchequer: your complaint should therefore be made to Mr Henderson the Queen's Remembrancer."

Acting on this, personal application was made to Mr Henderson; but his answer was, that he acted simply as auditor of the Sheriff's accounts, to see that his disbursements were according to the Exchequer table, and would give no advice as to who was the party liable.

On communicating this to the Lord Advocate, his Lordship was pleased, under date 5th June 1867, to say, "he was still of opinion that it was not a matter in which he should interfere." . . . "I find on inquiry that the Sheriff of the county is the proper accountant with the Crown with matters of criminal expenditure. If you consider yourself wronged therefore, your application should be made to the Sheriff of the county; and it is in his power to communicate with Exchequer on the subject of your payment as a witness." Donald Mackenzie, Esq., Sheriff of Fife, was written to: he replied,—"I regret I cannot sanction the payment which you ask."

The Lord Advocate was for the third time addressed. He informed us that (under date 17th June 1867) "he is *now* of opinion that in a case where a witness can, by leaving by a morning train, reach the Court in time, the case cannot be treated as an exceptionable one."

The opinion of the Lord Advocate amounts to this,—that, if a witness can get to the Court by leaving his residence at a few minutes after midnight, it cannot be considered exceptional, and agrees with that expressed by Mr Henderson of the Exchequer, that the word "day" used in the Exchequer tables means a few minutes less than twenty-four hours of the same day, so that a witness, by leaving home a few minutes after midnight, reaching the Court, say at 10 A.M., and being able to get back a few minutes before 12 P.M., is only entitled to a fee for one day.

It is proper here to say that applications were made to the Sheriff-Clerk of Fife before applying to the Lord Advocate. He declined to pay us or any witness, because he had never done so since his appointment in 1848.

After being referred from one official to another in regular circumlocution office style, we determined to sue the Crown Agent, as the party who instructed, indirectly, the Sheriff-Officer to cite us. We brought the action in the Sheriff Small Debt Court in Edinburgh. The following is the report of the case in the public prints, and embodies the chief points evolved:—

In the Sheriff Small Debt Court, Edinburgh, on January 22, 1868 (Sheriff

Campbell on the bench), Dr James William Reid Mackie, surgeon, Cupar-Fife, sued Thomas Graham Murray, Esq., W.S., as Crown Agent, for the sum of £4, 15s., being his fee for—in obedience to a citation, dated 4th April, given by his instructions, or by the instructions of a person or persons for whom he was in law responsible—proceeding from Cupar to Perth to give professional evidence at the Circuit Court of Justiciary there as a witness for the Crown at the trial of John Kinnear, then or lately a patient or inmate in the Fife and Kinross District Lunatic Asylum, in the parish of Cupar, charged at the instance of Edward Strathearn Gordon, Esq., Her Majesty's Advocate for Her Majesty's interest, with the crime of murder, or other crime. Time occupied, two days—£4, 4s.; travelling expenses from Cupar to Perth and back 11s.;—in all, £4, 15s.

Mr Rhind, advocate, who appeared at the request of Mr Saunders, the agent for the pursuer, stated that the action was one for a sum of money due to the pursuer in respect of his attending a trial in obedience to the citation mentioned above. He left Cupar on the Sunday to be in time for the trial on the Monday morning, and thus lost two days. It might be alleged that he could have been in time for the trial by leaving on the Monday morning, but he held that a professional gentleman in his position was not bound to get up at five in the morning, when he might be up late the previous night, and also the night following.

Mr Murray stated that in this case, under the Exchequer rules, the pursuer was held only entitled to charge his time for one day: but, without entering into the merits of the case, he must state the preliminary objection, that he as Crown Agent was not liable. His Lordship was well aware of the practice of the Circuit Court with reference to the payment of witnesses. It was the Sheriff-Clerk who paid them—the Sheriff-Clerk acting under the directions of the Sheriff, who was the accountant with the Crown in such matters. He received funds from the Treasury for that purpose, which he had to account for. The Crown Agent had charge of no public moneys for payment of Circuit witnesses, and it would be very detrimental to the public service if the payment of witnesses was not left in the hands of local parties cognisant of the facts and of the position of the witness, and if every Circuit witness who was discontented with the payment offered was to be held entitled to come to Edinburgh and sue the Crown Agent in the Sheriff Court there. The order for the citation of the witness was given by the Sheriff-Clerk; and to him and to the Exchequer he had previously referred the pursuer.

Mr Rhind said they had been shoved about from one party to another. He believed the citation was only sent by the Sheriff-Clerk acting on instructions given by the Crown Agent. He did not consider it necessary to have him here as a witness, but he might bring him; besides he was surprised that the Crown would take such a paltry objection as that of "wrong party sued," instead of honestly meeting us on the merits of the case.

The *Sheriff* said the question was one of importance. In the county the Sheriff-Clerk was in attendance, and then and there settled the witnesses' claims, adjusting the amount from his own local knowledge. He was not prepared to say if they had a claim against the defender, but his impression was that it should be the Sheriff-Clerk. It seemed to be a fair claim, and one which he would allow. He was sorry to bring the parties again into Court, but it would be better to take the matter into consideration. The case was therefore continued till next Wednesday.

On Wednesday 12th February 1867, Sheriff Campbell gave judgment. Before doing so, Mr Rhind and Mr Murray briefly addressed the Court, recapitulating their former arguments. But, in addition, we understand Mr Murray stated for the first time this important argument, reading from an old book of regulations, viz., that the Crown was only empowered to make payment to witnesses, if, from poverty, they were unable to attend at Court without great hardship to themselves, and that even in such a case what the Crown did give in money was not to be considered as proper compensation for time lost, but that any sum tendered to a witness was so tendered merely *ex gratia*, to which he had no legal right.

The *Sheriff* gave judgment. He said he was not called upon to say who was the proper party against whom the action could be brought. He had taken the case into careful consideration, and had consulted his colleagues, including the Sheriff-Principal, and he was of opinion that the claim against the Crown Agent was not one which could possibly be recognised. It was quite notorious that the Crown permitted no allowance to witnesses, and the mode of taxation which was adopted in Exchequer was such a secure one that scarcely any witnesses were ever satisfied with the remuneration. He apprehended that every witness in a criminal case was bound to attend and give his evidence without any remuneration whatever. By a long course of practice, however, the Crown had been in the custom of paying the witnesses a moderate allowance, and had drawn out a scale of rules and regulations for that purpose, and they were recognised as claims to be enforced in a particular way. But they were never recognised as claims against the Crown Agent, and money was never put into his hand for that purpose by the Crown. The summoning of witnesses in each case was made out by the Sheriff-Clerk, who, at the end of each trial, by the Crown money which came into his hands, paid the witnesses what, in his capacity as auditor, he thought they were entitled to. That custom was sufficient to exclude any claim being brought against the Crown Agent. Whether or not a claim could be enforced in a small debt court or any court against the Sheriff-Clerk, in respect that he had not fully discharged his duty, was a very different matter, but in that case the appeal lay to the Exchequer, and there alone. In respect of the case in dispute, he could only say that, as regards the Crown Agent, there had never been anything in it to warrant any claim against him.

We do not propose to say anything here on the minor hardships of our case, but simply desire to let the medical public know the greater principles of injustice which have been evolved from this comparatively trifling action. According to the decision of Sheriff Campbell, concurred in by three of his colleagues, medical men are liable, like ordinary witnesses, to be called to attend any criminal court, wherever situated in Scotland, for any length of time—and this without fee or reward; in other words, it is lawful and competent for the Crown to withdraw a medical man from his practice, to speak to matters of skilled professional opinion, not merely matters of fact. Our profession is liable to what no other one can be subject to. It is no exaggeration to say, that if Sheriff Campbell's ruling is correct, a medical man in Edinburgh, or anywhere else in Scotland, may be summoned to attend criminal courts in Aberdeen, Ayr, Perth, Glasgow, or elsewhere, that he may be detained there for an indefinite period, at great personal inconvenience and pecuniary loss, and yet be unentitled to any payment.

Surely this is a matter which the heads of our profession should take up and use their influence to have settled on some satisfactory basis. We cannot doubt, that if proper means were taken to represent the hardships we have spoken of, regulations for the proper payment of scientific witnesses would be instituted. In the meantime, the profession should hold itself aloof from incurring any responsibility connected with giving evidence in Crown cases, and, where circumstances make it impossible for them to do so, they should refuse to speak to anything but matters of fact, retaining to themselves any professional opinion which may be demanded of them by counsel till such time as they are acknowledged to stand in a different position from that of ordinary witnesses. Of course it is the duty of every citizen to give evidence wherever and whenever he may be desired to do so *as to facts;* but it is preposterous to suppose that any man's skilled opinion can be demanded of him unless he be properly remunerated.

We would now earnestly impress upon the profession the necessity for some combined action in these matters, as it is one which involves not only their individual comfort, but also their dignity as a body. The idea of the President of the Royal College of Surgeons of Edinburgh attending the Circuit Court at

Perth lately, for three days, and receiving his fee, *in forma pauperis*, is not only insulting but demeaning to the profession at large.

We do not think we demand too much from the Medical Council, the various Colleges of Physicians and Surgeons in the country, to use their combined influence to effect a change in the present unsatisfactory relationship between the law-officers and the medical faculty. We append excerpts from the Exchequer tables, showing what fees can be claimed in making post-mortem examinations. reports, and undergoing precognitions, and also the fees which the Crown or some of its subordinate officers may give *ex gratia* to medical men when acting in the capacity of witnesses.

We have no doubt this table will be of considerable use to many of our professional brethren, as from the want of it we have lost a few precognition fees. —We are, your obedient servants,

J. W. Reid Mackie, M.D., F.R.C.S. Edin., Cupar.
J. B. Tuke, M.D.,
Med. Sup. Fife and Kinross District Asylum.
C. H. Skae, M.D.,
Assistant Physician, Fife and Kinross District Asylum.

Excerpt from "Schedule of the Rates of Payment to Witnesses in reported Criminal Cases in Scotland, *as allowed* at the credit of the Sheriff's Accounts in Exchequer, dated Exchequer Chambers, Edinburgh, December 1862.

4th, Medical men, when cited to give professional evidence, from a distance of more than one mile from the towns in which they reside, if detained above four hours, are allowed, not exceeding per day, £2, 2s.

Besides fares: but nothing additional for maintenance.

If in town, they are allowed, not exceeding per day, £1, 1s.

In a letter, dated Exchequer Chambers, Edinburgh, 17th April 1865, and signed by Mr John Henderson, Q. and L.T.R.

"Medical men are allowed in Exchequer for each visit or examination, and relative report, a fee of *not exceeding* £1, 1s. if in town; and £2, 2s., and fares, if at a distance and detained above four hours. The term 'day' includes 24 hours of same date."

Medical men are allowed a fee of £1, 1s. for each precognition, unless it is simply to verify their report.

PUBLICATIONS RECEIVED.

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Archibald Billing, M.D.,—First Principles of Medicine. London, 1868.

S. Fenwick, M.D.,—On the Morbid States of the Stomach and Duodenum. London, 1868.

A. B. Garrod, M.D.,—The Essentials of Materia Medica and Therapeutics. London, 1868.

Half-Yearly Compendium of Medical Science,—Part I. Philadelphia, 1868.

Journal of the Royal Agricultural Society of England,—No. 7. London, 1868.

Key to Galloway's First Step in Chemistry. London, 1868.

J. M. Klob, M.D.—Pathological Anatomy of the Female Sexual Organs. New York, 1868.

A. de Labordette,—De l'Emploi du Spéculum Laryngien dans le Traitement de l'Asphyxie, etc. Paris, 1868.

James Morris, M.D.,—On Irritability. London, 1868.

Nouveau Dictionnaire de Médecine et de Chirurgie Pratiques,—Vol. 8. Paris, 1868.

F. Oppert, M.D.,—On Visceral and Hereditary Syphilis. London, 1868.

E. A. Parkes, M.D.,—A Scheme of Medical Tuition. London, 1868.

J. Forbes Royle, M.D., and F. W. Headland, M.D.,—A Manual of Materia Medica and Therapeutics. London, 1868.

E. C. Seaton, M.D.,—Handbook of Vaccination. London, 1868.

James Hutchison Stirling, LL.D.,—Materialism in relation to the Study of Medicine. Edinburgh, 1868.

J. C. Thorowgood, M.D.,—On Consumption and its Treatment by the Hypophosphites. London, 1868.

Transactions of the Obstetrical Society of London. London, 1868.

A. Trousseau,—Lectures on Clinical Medicine. Translated by P. V. Bazire, M.D. London, 1868.

A. W. Williams, M.D.,—On Cancer of the Uterus and other Parts. London, 1868.

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